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Psychological Review

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PSYCHOLOGICAL LITERATURE.

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THE PSYCHOLOGICAL REVIEW.

PRESIDENT'S ADDRESS BEFORE THE NEW YORK MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.*

BY PROFESSOR GEORGE TRUMBULL LADD,

Yale University.

GENTLEMEN OF THE PSYCHOLOGICAL ASSOCIATION:

The time and manner of the organization of this Association seem to me significant of certain important truths which concern the science in whose behalf the organization has been effected. Without undue modesty we should perhaps speak of ourselves as the youngest—the most nearly embryonic—of all similar scientific bodies; and it is, of course, well known that many workmen in other lines of scientific endeavor, and even some of the most notable and helpful among ourselves, still deny that psychology is entitled to be called a 'science.' On the other hand, it is not unbecoming pride which leads us to maintain that no similar organization is more hopeful, more disposed to be creditably aggressive, than are we. For few, if any, of the most firmly established and highly accredited scientific associations can rely upon a more devoted and well-trained membership, or upon more interest—both popular and permanent—in the results of their researches and speculations, than can those formed for the cultivation, in the use of modern methods, of the science of psychology.

Such a position as that which we occupy has certain disadvantages and certain equally great advantages. It cannot, indeed, be truthfully claimed that psychology has at present

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the same settled and accepted principles of method as those which belong—for example—to the modern sciences of physics and chemistry. Possibly—though doubt is certainly permissible here—in respect of its possession of an accredited method, it is not even the peer of biology, or of a so-called 'social science.' Neither is it possible for psychology, at least as yet, to formulate its ascertained facts, and announce the discovery of universal 'laws,' with the precision which belongs to the more advanced physical sciences. On the other hand, I am bold enough (perhaps *rash* enough would seem the more appropriate word) to predict that some of the most widely accepted of these physical formulas are destined to be thoroughly shaken up, in the not far away future. But, however this may be, there is always a certain advantage in the plasticity, the superior mouldableness, of the origin of scientific products and their developments. And if psychology, as a science, must be considered embryonic in its present stage, there is on this account the more opportunity for a band of students and investigators, such as we aim to be, to contribute something important to its more stable and higher evolution.

Now it seems to me that the large and final success of an Association like this will depend very conspicuously upon the attitude which its members maintain toward the three following classes of inquiries. I say 'classes' of inquiries, because each of the questions which I am about to raise includes an indefinite number of subordinate questions. As to these subordinate questions, probably no two members of this Association could be found in perfect agreement. But as to the right general attitude toward each of the three classes of inquiries, it seems to me possible that we may start our special lines of work with something approaching a common consent. A detailed discussion of even such general questions, and a defence of the attitude which I think should be taken toward them, would be quite too much of a task for the present occasion. I shall limit myself to a brief statement, followed by some rather indefinite remarks upon what seems to me to be the right attitude toward each. I shall be content with gaining something in comprehensive-ness of view, and in largeness and freedom of spirit, even if I lose much as respects precision and satisfactoriness of proof.

In other words, gentlemen, let us allow our eyes to wander with an æsthetical and ethical, rather than purely scientific, intent over our broad and fair domain, before we settle down, as a well-organized colony, to its minuter exploration and cultivation.

The three classes of inquiries to which reference was just made are the following: (1) What is the relation in which the statistical and experimental investigation of mental phenomena stands to the total science of psychology, in the larger meaning of the latter words? (2) What is the relation in which the science of psychology, thus understood, stands to that interpretation of the external world and of human life, in its yet larger and profounder experiences, which we are wont to call philosophy? (3) What is the relation in which the science of psychology stands to conduct and to the practical welfare of mankind? More briefly expressed: How shall we regard the science of mental life as related to the methods and conclusions of the most nearly allied physical sciences, to philosophy, and to human action and character?

Now, if I were to speak my mind at all fully in answer to either of these three questions, I should doubtless find the amount of assent which my words commanded varying for each of the three. As to the last of the three questions, it would probably be possible to receive the adherence of all my auditors; as to the second of the three (namely, the relation of psychology to philosophy), a pretty general agreement might perhaps be reached; but as to the nature of the science called psychology, and as to the use of laboratory and other allied methods for its cultivation, perhaps any one of our number, if addressing the Association, should be amply satisfied if he had succeeded in carrying the assent of a bare majority. This last remark is made in passing, partly as a matter of rhetorical policy; for the order of treatment which I have adopted compels me to speak of the most controverted subjects first.

First, then, as to the question of method, and of the possibility of rendering psychology more truly scientific, by use of right method; as well as of the value and limits of the more modern statistical and experimental researches. On all these matters I begin with an exhortation addressed not less to myself than to all my colleagues in the common work. Let us all always be just: nay, let us be something more than merely just; let us be generous. And let our generosity include all workmen of all times, with their works, from Aristotle's *De Anima* to the latest thesis by the youngest aspirant for the degree of Doctor of Philosophy,—even if months of painstaking experiment in some German or American laboratory have led him to merely 'negative' results. Let this same generosity also include all methods of dealing with mental phenomena; from experimenting with key and chronometer, through thousands of trials in reaction-time and elaborate mathematical discussion of general averages, to the introspective seizure of some rare happening in individual consciousness, with the felicitous guess which genius makes as to the meaning of the fact thus surprised; or to the reflective study of that artistic delineation of soul-life in which the best novels, poems, and dramas are so wonderfully successful.

Nor can I approve of the proposal to restrict the use of the words 'science' and 'scientific'; or of the denial that psychology *is* a science, or of the refusal to accept as *scientific* other contributions than those of the physiological or psychophysical laboratory. Apropos of the correct and courteous use of these terms, I recall a colleague of my younger days, a young professor of physics; this ardent 'scientist' boldly denied the right of any other branch of human knowledge to the term 'science'; with him, there was only one *science*,—namely, physics. But, on the other hand, I shall not soon forget the reply, made by one of the most distinguished investigators and writers on this subject, to my question (half jestingly put), whether he considered meteorology a science. Said the veteran: "Meteorology is *just as much* of a science as geology is."

Now no fixed line can ever be drawn between science and ordinary knowledge; and science begins whenever and wherever facts begin to be carefully observed and classified, and attempts at explanation, by way of stating the customary forms of the occurrence of the facts in relation, are made. To affirm that psychology is not a science, whether reference be had to the study of mental phenomena by the so-called introspective or by the so-called experimental method, seems to me both philologically and historically indefensible. While to postpone the gift of the title until some law, like that of gravitation in physics or of chemical æquivalency in chemistry, has been discovered, is to assume, unwarrantably, that some *such* law is actually followed by the phenomena. Such an assumption is, itself, at least premature and unscientific; even if we are not justified in saying that the very nature of psychical facts, and of their origin and sequence, is such as to render it forever unrealizable. In brief, there is every reason why we should be both just and generous in our use of terminology. This Association is formed for the advancement of a *science* already existing, and, indeed, like all the other principal sciences, some centuries old. We of to-day have entered into the inheritance of past ages; and it is becoming for us to do so with generous acknowledgment of what the past has done for us. And yet, although we are children of the ages, we are pre-eminently children of the present age. For a certain way of studying the phenomena of mental life is comparatively modern; and the hopes which are entertained respecting results from this method are by no means altogether misleading.

This last remark introduces certain considerations respecting the relation of introspection and the use of statistics and experimentation in psychology. I need not speak in detail of the burning and strife which have too often accompanied the mere mention—not to say, the discussion—of this subject. I venture to hope that I speak for the great majority of this Association when I say that this feeling is to be deprecated; and, except so far as all controversy, however conducted, helps in a measure to elicit truth at last, it is to be distinctly avoided. Whoever takes a wide historical and philosophical view of the evolution of science in general—I do not say simply of psychology, in particular—can sympathize fully with neither of the two extreme views. He will neither, on the one hand, quake with fear lest the foundations of the world's stock of truths in ethics and philosophy are to be undermined by the discovery of the function of Broca's convolution, or of the laws of reaction-time where apparent choice is concerned; nor, on the other hand, will he undertake to deny the verity of æsthetical, ethical, and religious consciousness, or pride himself on his ability to dispense with introspective psychology and philosophy, because of some new device in mechanism to aid the solution of certain subordinate psycho-physical problems.

The question how far laboratory and other methods akin to those of the most advanced physical sciences can be used in the development of a scientific psychology will answer itself only in the course of history. It is always a venturesome thing to lay down limits that anticipate the requisite experience. That can be which will be; and what will be cannot always be precisely predicted by means of what now is. Yet certain observations occur to me which seem more or less certain of realization. That no method can be developed in psychology which will enable us to dispense with introspection, or which will cease to be very largely dependent, for its own value, upon the value of the introspection which accompanies it, is too obvious to require discussion. Of course, the proposal wholly to get rid of self-consciousness as the medium of knowledge of the phenomena of consciousness is absurd. And however we may seem compelled to interpret the language of any advocate of experimentation and 'objective' observation *in the stead of* introspection, we can scarcely believe that his proposal is to be seriously and intelligently understood. The results of any 'series' of experiments, the generalizations from any 'pile' of statistics, become material for psychology only when they are interpreted in terms of consciousness. For scientific psychology *is* the science of the phenomena of consciousness, *as such.* And no interpretation of consciousness is possible in any terms whatever without self-consciousness. Every intelligent and sincere worker by laboratory methods knows that there is nothing of more doubtful scientific value than are the results obtained when the man behind the key is reacting in the interests, as it were, of his own self-consciously or unconsciously adopted theory;

unless, indeed, it be the interpretation of results obtained from an unprejudiced reacting agent by some prejudiced theorizer. Twist the matter as we may, we cannot get rid of the fact: skill in introspective observation and analysis sits at one end of the series of experiments as witness being examined, and at the other end as judge pronouncing after, or even before, the examination of the witness. It is plainly worth while to remark in passing that the same thing is true, though in far less degree, in all the physical sciences. The history of biology, of geology, and even of astronomy is full of examples of failure to arrive at truth objective and universal through lack of skill in self-knowledge. Hence the safe conclusion that a scientific psychology is the handmaid of all the sciences.

Furthermore, any attempt to separate introspection from experimentation and the more objective estimate of statistical material is as impolitic as it is plainly impossible. In past time the science of psychology has been advanced far more by those guesses at the truth based upon *my* truth,—those leaps from what is self-consciously discerned as *in me* to what belongs to all men, to human nature as such,—which characterize the "born psychologist," than by long series of trial experiments or by vast collections of "data" so called. Nor am I sure that this will not always continue to be so. Here again, however, the method of psychology is not so wholly unlike that by which the physical sciences have grown. They, too, have made their great advances chiefly through the intuitive flashes of that genius which sees the general and the universal as it manifests itself in the particular. In psychology, as in these physical sciences, the truth which Aristotle recognized, of course, always holds: there can be no *science* of that which is individual merely. But in psychology more, by far, than in the physical sciences, the observation and skilled interpretation of the facts of individual experience are likely to lead directly to what is true and valuable for the entire species.

Once more, it seems to me that there are certain factors and aspects of all, even the commonest mental life, which will never readily lend themselves to refined methods of experimental analysis and interpretation; which will never yield to the attempts, however persistent, of the collectors of 'data.' I am well aware that my opinion here will by no means command universal assent. It will probably seem to some that I am violating my own caution, not to limit in lofty *a priori* fashion the possibilities of triumph which lie before the new methods of solving psychological problems.

In illustration of my meaning, however, let me call attention to the following facts. We have had of late many considerable volumes on psychology; as, indeed, voluminous works abound on all the modern sciences. But perhaps we do not often enough consider how exceedingly meagre, as compared with the wealth and complexity of actual mental life, are the most voluminous of these treatises. Let the plain man read carefully through the biggest of all these books; and the astonishing thing is that so large a part of his daily experience is, not simply left unexplained to his satisfaction; it is not even treated at all. This is, of course, no sure proof that psychology is still in a lamentably backward condition. It is an illustration of the general truth that all human science is but patches of a shallow, superficial stratum, dimly lit through occasional rifts in the clouds, over the fathomless depths of the ocean of reality. For example, how absolutely dumb is all our most advanced evolutionary biology, when the common gardener asks for an explanation of the changes through which pass the phylloxera that are destroying the roots of his vines, or the moths that feed upon the leaves of his fruit-trees!

In further illustration of my meaning let me—though with a protest looking toward its definitive and final rejection—adopt for the moment the customary division of mental phenomena into knowledge, feeling, and will. It is simple matter of fact that, thus far in the development of laboratory and statistical methods for dealing with mental phenomena, it is the ever-present sensation-content of all these aspects of mental life which has chiefly, and almost exclusively, been the subject of treatment. This is, in part, perhaps the reason why some who are most nobly impatient of the limitations which have hitherto surrounded the use of experimental methods are so strongly inclined to identify feeling with quality of sensation, 'pleasure-pain'-wise, and volition with dominant stress of sensation. But let us place the plain man in the presence of any common thing, and let him attain what he calls a 'knowledge' of that thing, and then summon the psychologist who is most expert in laboratory methods, and most learned in the results of such methods, to explain his *knowledge* of the 'Thing,' and—pardon the uncouth word—the *thinghood* of that which is known; and how far, pray, will the explanation go in reliance on the conclusions of a strictly inductive and experimental psychology? The expert will have to stop short when he has enumerated certain principles that have respect to the quality, quantity, time-rate, and combination of the sensation-factors whose synthesis is the sensation-content of knowledge. But in doing this he has not explained, he has not even described with any approach to completeness, that state of consciousness which we call an act of knowledge. And here I am not asking of the psychologist a system of metaphysics or a theory of knowledge, to incorporate into his experimental resultant. But I am simply asking that he shall describe and explain, *as such*, that common enough state of consciousness which everybody calls 'the knowledge of a thing.' Nor does it seem to me at all likely that our physiological and psycho-physical laboratories will ever be able to handle certain factors and aspects of this psychological problem of knowledge. For example, how shall we experiment or collect statistics to elucidate the 'belief' in reality which different writers have assigned, now to intellection, and now to feeling, and now to will, but without which no knowledge of anything can take place? For my part, I am just as firm in my opinion as the most old-fashioned psychologist, while in admiration for the new psychology I yield to none, that self-consciousness envisages a self-activity, and a conviction of extra-mental reality; in all knowledge, which experimental data are quite powerless either to deny or to explain.

Nor do I look forward with much confidence to the elucidation of our so-called 'higher' æsthetical, ethical or religious sentiments, by experimental analysis or by collection of statistics. Something worth while will doubtless be done in the region of the simpler and more fundamental feelings, by laboratory methods; and what is done in this region will help us the better to understand what happens in the higher regions of affective phenomena. But I suspect that the limitations of the successful use of these methods are likely to be pretty quickly reached; and that we shall have to go to art and to literature, as interpreted through our own best self-conscious feeling, for the clearer understanding of all such phenomena.

With respect to choice and free will so called, as elucidated by the modern experimental methods, my hopes are very moderate, and my fears are—*nil.* In this line of investigation it is quite too often forgotten what it is, taken in its depth and entirety, which needs to be described and explained. You may seat your reacting agent, tabulate and arrange your results, and conclude—we will suppose, for the sake of illustration—that theoretical determinism has received an experimental demonstration. But suppose that I, in common with the great majority of men in all ages, doubt the truthfulness of your conclusion; and that to your conclusion I oppose a certain conviction that sometimes, somehow, I determine instead of being determined,—a conviction which I also share with the great majority of mankind. Now this doubt and this conviction are themselves psychological facts; they are of no small import and of almost universal occurrence. But how are you going to investigate them experimentally; how describe, explain, or explain away, the doubt and the conviction, by psycho-physical methods? To be sure, you may tell me that, if a stone, which flies through the air to its predetermined spot on the ground, had a *plus* of consciousness added to its motion, it would be conscious of self-directed motion, in the absence of any knowledge of the laws of gravitation, pressure from atmospheric currents, etc. But here again suppose that I doubt; and perhaps revive the time-worn conviction. For I do not see why consciousness + motion should equal anything more than consciousness *of* motion; or why consciousness *of* motion + ignorance should develop doubt of determinism and conviction of freedom. But, since it is in no respect my intention to argue this ancient problem, I will conclude this point by returning to my main thought: I do not see how the hypothetical instance of a conscious machine enables us the better to handle the aforesaid doubt and the aforesaid conviction, by the methods of the psycho-physical laboratory.

It is high time, however, to turn our attention to the other side of the relation we are discussing. A mere glance at this other side is sufficient; because I suppose there is not a member of this Association who does not approve of the study of mental phenomena by experimental and statistical methods. This country, following Germany and in marked contrast to Great Britain, has eagerly and—on the whole I am sure very intelligently and safely—adopted this method. Our larger universities have already equipped, or are rapidly equipping, themselves with psycho-physical laboratories; our smaller institutions even are demanding of their teachers some acquaintance, at least, with modern ways, and modern results, in the study of psychological science. All this is very stimulating, very hopeful. The expectation is not unwarranted that the United States will soon become the coworker, on equal terms, of the best European laboratories. It is not for purposes of flattery, but rather of warning, that I venture to say: The fate of this movement in this country will depend very largely upon the action, individually and in corporate fashion, of the members of this Association. For myself, within limits which I have already roughly and inaccurately sketched, I look for a large development of the science of psychology, in the near future; and I am certain that this development will not be without influence upon the current philosophy and theology, as well as upon the practical welfare of the people. This confidence has its principal reasons in the necessarily close relations that exist among all the subordinate departments of the science of psychology, and the especially intimate relations in which the science stands to philosophy and to the life of conduct and the development of character. This last remark brings me to the second of the three points which it is my purpose to consider.

Philosophy is on the whole much older and more interesting to the human mind than is the science of psychology. Indeed, philosophy is older than any science, whether of mind or of matter. Various definitions setting forth different conceptions of philosophy have been put forth at different epochs in its development. Perhaps the chief characteristic of the modern conception has reference to the relation in which philosophy stands to the various concrete or particular sciences. A passing glance at the way in which the present more cordial understanding of the two has come about may fitly be given; for here, as everywhere, the history of the evolution of human knowledge is full of instructive lessons. We go no farther back than to recall how the most stupendous systems of speculative thinking were built on ground which had been apparently swept quite bare by the criticism of Kant. This 'astounding' thinker, as Schopenhauer has called him, supposed that the negative result of his labors would be to remove forever the pretence of ontological *knowledge*, while 'making room'—to use his own phrase—for faith in the verity of certain postulates respecting ethical and religious entities. Much has been written concerning the failures and successes of the Kantian criticism, and concerning the causes of both. In my judgment—although I speak somewhat diffidently, because I am not aware that any of the most distinguished critical students of Kant have put the matter in just this light—the chief cause of the failures of this greatest of all modern thinkers lay in his imperfect and wrong conceptions of a psychological sort. Kant did not understand in a scientific way the common consciousness of the race. Especially defective and erroneous is his conception of *knowledge;* I do not now mean his theory of knowledge, but his descriptive history and implied analysis of that state of consciousness which all men recognize as entitled to be called 'knowledge.' But without proving this charge, and not to be drawn too far aside from the main current of my intention,—the issue showed that men would not be warned off by a critical theory of cognition from the 'pretence' of ontological and systematic knowledge. And, indeed, how could they be; since there is no such thing as knowledge that is not ontological? 'Ordinary' *knowledge* and 'scientific' *knowledge* are as full of unverifiable postulates as were the old-fashioned rational psychology or the rational theology; the only net, valuable result of no end of criticism being to discover what postulates, or fundamental faiths, enter into *all* knowledge, and how they may be so understood and expressed as best to hang together.

Now, contemporaneously with the strong reaction against the negative conclusions of the Kantian criticism, in philosophical circles, there went on a mighty forward movement of the physical sciences as pursued by the more strictly inductive method, with the determination to prove all speculative hypotheses by experimental tests, and to express such of them as stood the testing in the intelligible and accurate terms of mathematical formulas. It was inevitable that these two movements should have a somewhat varied and sometimes painful experience in the effort to adjust relations with each other. In my opinion, if we set aside the theologians, the students of philosophy have on the whole behaved far better than the 'scientists' so called. I am not aware that even Hegel anywhere manifests a contempt for facts, as such, or flouts at the conclusions of his contemporaries in physical science, so far as he understood them to be scientifically derived. He undoubtedly everywhere manifests an overweening confidence in his ability to give an ultimate explanation of all these facts in accordance with the method and principles of the dialectical philosophy. But Mr. Herbert Spencer has not half as much expressed horror for the merely abstract, or manifest eagerness to get at the heart of the concrete and the real; and perhaps Hegel, when he steps over too far upon the domain of the positive sciences, is not—making allowance for the condition of things in his day—any more ridiculous than some of the modern disciples of science have been when they have transgressed the limits of their specialties (for example, Mr. Huxley in the arena of biblical criticism).

For an entire generation, which now seems happily drawing to a close, the relation of philosophy to the positive sciences, or rather of these sciences to it, was one of open antagonism or half-concealed contempt. 'Metaphysics'—for so all branches of philosophy were often sneeringly called—was a tabooed subject for the student of physics. And yet what was actually going on all this time? Why, within the domains of physics, chemistry, and biology, a system of 'metaphysics' was being evolved which, although it does not know itself by that name and rarely arrives at an adult stage of self-consciousness, is quite as wonderful and stupendous in respect of its postulates as were any of the philosophical systems which followed the criticism of Kant. Nor will it do to maintain that this underlying and interpenetrating ontology can be removed and the modern system of the physical sciences remain, as sciences, in the same condition as before. The rather is it true that the complete removal of this metaphysical system would reduce the sciences from the condition of knowledge to the mere pretence of knowledge; from the claim to be systematized truth about real things and real events to mere *Schein*, as it were. Indeed, if it were in the line of my present pursuit to do this, I think it could be shown that the only result of the consistent carrying-out of this negative criticism and the resulting agnosticism is the falling in one common ruin of the rational foundations of daily conduct, of the natural and physical sciences, of ethics, and of theology—of the whole temple of human *knowledge*.

Of late and for some time, however, there have been plain signs that the age of opposition and conflict between science and philosophy is being replaced by an age of 'reconciliation.' Indeed, 'reconcilers' of science and religion, of science and philosophy, of philosophy and religion, are everywhere, thicker than bees in the blossoming-time of a Southern spring. Plainly, it is reconciliation which is in the air; and he is an 'old fogy' in spirit, however youthful he may be in age or appearance, who continues to talk with Haeckel about "strangled snakes lying around the cradle of the young Hercules"—namely, modern science—and other high-sounding but ill-timed phrases to the same effect. Better and wiser, by far, and at once more philosophical and more scientific, to hold out the hand, with Helmholtz and Du Bois-Reymond, or with Lotze and Herbert Spencer, towards the other party, to whichever of the two one happens to belong. For my own part, I have no great confidence in the permanency of the actual reconciliations thus far effected; and this both because they have been made for the most part by men of only second-rate quality, and also because they have been quite too superficial in the selection of subjects on which to make the attempt at reconciliation. But the spirit is admirable; and good results cannot fail to follow in the near future.

For science and philosophy will always exist; neither can expel the other from the region of human interests and human endeavor; indeed, no rigid demarcation can ever permanently divide them; each will flourish only in dependence upon the other. Moreover, minds whose interest is chiefly in facts and whose skill discovers itself chiefly in collection of, and lower generalizations from, facts, will always exist; nor will the race of other minds cease whose interest leaps forward toward the places from which to survey the more ultimate meanings of the facts, and whose greatest skill shows itself in the wider speculative treatment of them. And occasionally great minds will be vouchsafed to the race, who will combine the ability to acquire a large amount of scientific data of various kinds with skill in philosophical analysis and a genius for philosophical synthesis; and these minds will be among the greatest benefactors of mankind with respect to the development of both science and philosophy. For philosophy is but wild and mischievous speculation, unless it build itself upon the concrete and particular sciences; and science is but the unsatisfying husk of knowledge, is without rational self-consciousness and highest import and divinest interest, unless it intelligently lend itself to help, and to be helped by, philosophy.

But of all the particular sciences it is psychology which stands in the most intimate relation to philosophy. We are in this day making an attempt, both valiant and in large measure wise, to separate between the science of mental phenomena and those metaphysical assumptions which have hitherto so largely overlaid and suppressed the growth of the science. In the interest of this separation we are told that a mixture of metaphysics and psychology as 'a natural science' spoils both ingredients, and, as a mixture, is apt to please neither of the two classes of patients for whom it may be supposed to be prepared and prescribed. This is true; although the truth depends chiefly upon the proportion of the ingredients which enter into the mixture.

Yet there is another side to all this which we cannot afford to lose out of our total account. We have seen that it is difficult to pursue any form of a so-called 'natural science' without being called to consider, philosophically, its principles, their import, and their relation to the principles of other more or less closely allied sciences. The moment we begin to strive for a knowledge of principles, however, we come perilously near to the border-line—all invisible as it is—between science and philosophy. But, however this may be in the case of the physical sciences, there can be little doubt that the relations between psychology and philosophy are much more intimate. They are *so* intimate, indeed, that many of the most profound students of both, approaching them from varying points of view, have declared it to be impossible to separate between the two. It is well known that Herbart—to whom, in spite of his many errors, the modern science of psychology owes an enormous debt—declared : "The whole series of the forms of experience must be investigated twice over, once metaphysically and then again psychologically;" although he adds that these investigations must lie "side by side" and be compared so that we may never again confuse them. His most distinguished disciple, Volkmann von Volkmar, whose work on psychology, although I differ *in toto* from many of its conclusions, seems to me the most mature and magnificent of modern times, declares it to be impossible to separate between psychology as a science of mental phenomena and rational psychology, or the philosophy of mind. While Wundt, who differs very widely in method and conclusions from the Herbartians, affirms that the relation of psychology to philosophy is so close and peculiar that the partition of sovereignty between the two is an abstract scheme which, in the presence of actuality, must always appear unsatisfactory.

It is both significant and amusing to notice the actual behavior of many who theoretically deny that any such intimate relation must be acknowledged. For example, Höffding, in his most interesting and excellent treatise on psychology, after formally announcing that he proposes to treat the subject solely in a scientific way, almost immediately, and without waiting to marshal his facts, declares philosophical monism to be the only tenable view of the relations of mind and body. And it is not long since I read in a magazine article, designed to set forth results in experimental psychology, the astonishing statement that no truly 'scientific' (*sic*) psychologist in these days held any other view on this question than the monistic; it would seem, then, that all dissenters on philosophical grounds from the modern Spinozism are to be read out of the ranks of science by this ardent young brother.

Now I do not mention these two classes of opinions for either confirmation, denial, or debate. That a worker in psychology may conduct an elaborate series of experiments, or discuss some important psychological principle, without once raising the questions in discussion between monism and dualism, materialism and spiritism, there can be no doubt. But surely psychology has as much right as has physics to its speculative hypotheses and supreme generalizations, if only these are kept in their place of close dependence on observed facts and sound reasoning. Much more than this is, in my judgment, however, true. For the relation of psychology, as a science, to the philosophy of mind, and through it to all philosophy, is so intimate and binding that not one of the larger psychological problems can be thoroughly discussed without leading up to some great debate in the field of philosophy. As long as psychology is naturally propædeutic to philosophy every one must be puzzled to tell just when he has crossed over the line and left the plain paths of science behind in order to get lost in the jungles of metaphysics.

If time permitted, this general statement might be enforced with almost indefinite detail. Thus it is exceedingly difficult, if not impossible, to give a thorough psychological discussion to the phenomena of perception by the senses without taking some position—at least an implied one—respecting the philosophical questions in debate between Realism and Idealism. Who would willingly undertake to separate strictly between the psychology as 'natural science' and the psychology as philosophy that are involved in the Empiricism of Wundt and Helmholtz, and in the Nativism of Dr. Ward or of the exceptionally admirable treatise of our own Professor James? This relation of well-nigh inseparable intimacy must continue to exist, and it will survive all warnings and all exhortations; because it is not, in the last and supreme and most difficult effort, some account simply of the intensity and content and time-rate of sensations which psychological science has to render; it is rather of the faiths and fears and opinions and knowledges of mankind about things. And as the late Professor Croom Robertson said: "We *may* view knowledge as mere subjective function" (that is, psychologically); "but it has its full meaning only as it is taken to represent what we may call objective fact, or is such as is named (in different circumstances) real, valid, true." But he goes on to say: "Philosophy, on the other hand, is the theory of *knowledge* (as that which is known)." Yet again, we agree further with Professor Seth when he says: "It is evident, then, that philosophy as theory of knowledge must have for its complement philosophy as metaphysics or ontology." Putting all these declarations together, what can be made to follow besides the obvious proposition that a full-orbed science of psychology is propædeutic to and implicative of both epistemology and ontology?

This Association is formed in the interests of a science of psychology; it cannot therefore be expected to occupy its time and energies largely in the discussion of philosophical problems. Its members, however, would be something either more or less than completely human if they took no interest in any of these problems. The preceding remarks have been designed to introduce the exhortation that, since, from the very nature of our science, we shall scarcely always be able to avoid all seeming of entanglements more or less epistemological and ontological (and perhaps even ethical and theological), we should, first, add the philosophical spirit to our scientific intent; and, second, be not only wise and cautious, but also tolerant and generous toward the various possible expressions of philosophical views.

A few words will suffice for suggestions on the third of my three points; since—as has already been said—no considerable divergence of opinion is to be anticipated here. It is reasonable now to be very enthusiastic concerning the contributions which a scientific psychology may be expected to make toward the practical welfare of mankind. This fact seems to me to place a certain weight of responsibility, which is of a quasi-ethical sort, upon such an Association as ours. It is sometimes supposed that the truly *scientific* spirit and attitude require a man to be interested in science solely for science's sake; or—to put the case in a yet more captivating way—in scientific truth for this truth's sake. Now I should not willingly be inferior to any one in devotion to the truth for 'its own sake' (as we are wont to say); and I trust that I have a sufficient admiration for the scientific spirit and for the splendid triumphs of modern science so called. At the same time my observations lead me to admit that not a few who cry most loudly in the name of 'science' show quite too plainly that it is chiefly for their own sakes; nor do I find that it has been the thing of smallest import with the truly great men in science that their pursuits enabled them to be, in no small measure, benefactors of mankind. And while they have been more unwilling than ordinary men to swerve a hair's breadth consciously from the strictest truth, and have had a generous confidence in that blessing which adherence to the truth brings, they have also recognized that the highest and truest truth which it is given us men to know, somehow seeks and finds an embodiment in conduct and character.

For example, astronomy, being originally devised in the interests of humanity as astrology, and then becoming truly scientific, has returned far more than all its costs as navigation, meteorology, natural theology so called, etc.

A fortiori is the obligation to be of practical benefit heavily laid upon psychology. The more I study and teach this science, the deeper does the impression become that it is able and destined to contribute greatly to the welfare of mankind. I shall now close these remarks with a brief enumeration of some of the well-known directions in which we all hope to see this impression realized.

First: the science of psychology may be expected to make large contributions toward the improvement of the art and practice of teaching. Pedagogics so called is already a considerable 'fad' in this country. It is, however, I assure you, something far different from the contempt born of professional pride which leads me to say that, with comparatively few exceptions, the written and oral work on this important subject, in America, is shallow and misleading to an almost incredible degree. Meantime, an enormous waste—amounting to some three or four years in the ten or twelve of our public-school life, on the average, for each one of the millions of our school-children—is ceaselessly going on. Several causes combine to bring about this deplorable result; the most complete cure possible can, of course, be effected only by dealing with all these causes. But one of the most important helps to improvement must come through the instruction of the teachers of these children in the principles of a truly scientific psychology. And such instruction must emanate from the highest expert sources, and penetrate to the lowest strata and the remotest regions in the public-school system. It can never come in the form of half-baked treatises put forth by writers who, however seemingly successful they may have been in practice, have no scientific understanding of the principles on which even their own—too often merely apparent—success has been based.

Again, the science of psychology may be expected to contribute much to the science and practice of medicine,—especially, of course, in the department of neurology. Even modern surgery has already been guided by the help which physiological psychology has rendered in the discoveries, since 1870, in the localization of cerebral function. Looked at from a truly rational point of view, what can be more amazing than the fact that thousands of doctors are to-day treating patients suffering from 'mental' disease, who themselves never made the slightest study of mental phenomena, sane or abnormal, in any scientific way? With so many quacks, on the one side, medicating the mind with drugs, is it greatly to be wondered at that there are so many cranks on the other side who are advocating the treatment of all disease with 'mind-cure' or 'faith-cure'? In my opinion the time will come when no reputable medical school will think of giving a diploma to a student who has not made a thorough study of psychology, at least as far as its elements may be pursued from the physiological and experimental points of view.

In the diagnosis and treatment of the insane, the incorrigible, the idiotic, etc., scientific psychology is surely destined to exert a growing influence. In time it will come to appear that the student of anthropology, of criminology, or of sociology, who has failed thoroughly to lay his foundations in the modern psychology has been guilty of an oversight or neglect fatal to his highest success. Nor will certain forms of jurisprudence long continue to disregard their natural relation to the scientific study of mental phenomena. As civilized nations come to distinguish between the man who is fit to be a 'keeper' of the insane or the criminal, and the man who is fit to give expert testimony to distinguish between the insane and the sane criminal, the advantages of prolonged psychological investigation for the improvement of jurisprudence will be more clearly discerned.

But there is no need to specialize further. In general, why should we not expect to see our science contributing to the improved conduct and character of men, in the school, in the court-room, the prison, and the asylum? Nay, I am not without hope and expectation that even the sacred offices of the religious teacher, as well as the no less sacred offices of the teacher of ethics by parental influence—of the mother whose breasts with their stores of nourishment, and face-to-face intercourse with the infant according to principles which regulate the earliest sensory-motor and imitative functions, fix the lines of behavior and of destiny—may all be helped and blessed in no small degree by the recent rapid advances of human psychology. And this is the chief reason why I close, as I began, with words of cheering reminder that this Association snould enter upon its career with a sufficiently generous estimate of its privileges and of its responsibility.

THE CASE OF JOHN BUNYAN. (I.)

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THE casuistry of the numerous forms of insistent mental processes of a pathological character has of late years become very extensive. The names and sub-classes of these morbidly insistent kinds of feeling, thought, or volition have occasionally been multiplied beyond any reason, until, in view of the endless '*manias*' and '*phobias*' that some writers have been disposed to dignify with special titles, I myself have sometimes wondered whether it would not be wise for some one, in the interests of good sense, to try to check this process by defining, as a peculiarly dangerous type of insistent impulses, a 'new mental disorder,' to be described as the 'mania' for multiplying words ending in *mania* or in *phobia*. Meanwhile, despite this inconvenience, and despite numerous hasty speculations upon the whole subject, there can be no doubt that the theoretical interest of these morbidly insistent mental processes is great, and that the pathological secret and the genuine natural classification of these disorders will be such as well to repay the trouble of the most minute study of cases, if only that secret ever comes to be made out, and that natural classification is ever set up. And while we wait for further light, the careful preliminary scrutiny of cases is indeed the only course open to students of psychology.

The present paper is but a very modest contribution to the casuistry of the morbidly insistent mental processes. I have no new *phobia* or *mania* to define, and in any case I speak only as student of psychology. The medical reader might be able to see much more in the documents to which I here wish to attract his attention than I am able to see. My task is simply one of summary and report. The case to which I wish to call attention is meanwhile one of peculiar interest, namely, that of the author of the *Pilgrim's Progress.* The principal document concerned is John Bunyan's remarkable confession, entitled *Grace Abounding to the Chief of Sinners*, an autobiographical statement which Bunyan wrote and published, as the title-page tells us, "for the support of the weak and tempted people of God." This little book is, from the literary point of view, of very high interest, ranking, as I suppose, amongst all the author's works, second only to the great *Pilgrim's Progress* itself. As a record of human experience, the *Grace Abounding* will never lose its charm, both for lovers of religious biography, and for admirers of honesty, of sincerity, and of simple pathos. Nothing that can be said as to the psychological significance of the author's recorded experiences will ever detract from the worth of the book, even when viewed just as the author viewed it, as a 'support' for the 'weak and tempted.' Bunyan, as we shall see, had at one time a decidedly heavy and morbid burden to bear. But, like many another nervous sufferer of the 'strong type' (Koch's *starker Typus*), Bunyan carried this burden with heroic perseverance, and in the end won the mastery over it by a most instructive kind of self-discipline. In view of this fact, a clearer recognition of the nature of the burden, from the psychological point of view, rather helps than hinders our admiration for the author's genius, and our respect for his unconquerable manhood. It is this sort of case, in fact, that renders the study of the nervous disorders so frequently associated with genius, a pursuit adapted, in very many instances, not to cheapen our sense of the dignity of genius, but to heighten our reverence for the strength that could contend, as some men of genius have done, with their disorders, and that could conquer the nervous 'Apollyon' on his own chosen battle-ground.

But an estimate of Bunyan's genius belongs not here. I venture only to say that I write as an especially profound admirer of this wonderful and untaught artist, whose homely style shows in almost every line the born master, whose simple realism in portraying human character as he saw it amongst the live men about him often puts to shame the ingenuity of scores of cunning literary craftsmen in these our own most realistic days, and whose few highest flights of poetic imagination, such as the closing scenes of the first part of the *Pilgrim's Progress*, belong without question in the really loftiest regions of art. Range of invention, self-control in production, perfect objectivity in the portrayal of human life,—these are leading traits in the work of this man; and these things, as well as others that we shall later see, forever forbid our classing Bunyan, taken as a whole, amongst the weaklings. It is perfectly consistent with this fact, however, when we find this admirable man and artist living, for a bitter and instructive period of his early years, a life of stern conflict with a nervous foe of a fairly recognizable and, under the circumstances, decidedly grave type. How, unaided and ignorant, he won the victory, is in itself an interesting tale. And, for the rest, the case tends to throw light on the interesting problem as to how far the presence of elaborate insistent mental processes of a morbid type is of itself a sufficient indication of the depth of the 'degeneracy' of constitution of the subject who, is for a time burdened with them.* That Bunyan's malady must have had a certain constitutional basis will, I suppose, appear decidedly probable to most readers of the following summary. Yet it will be hard to question the fact that, quite apart from his special creative abilities, Bunyan's general constitution,—his extraordinary and persistent power of work, his long endurance of very serious mental and physical hardships, his reasonably lengthy life of sixty years (ended by an acute disease, due to an exposure), his apparently even temper and self-possession in later years, his sustained influence over men as leader, adviser, and preacher,—when taken all together, must give us an idea of his inherited organization that will, in any event, stand in a fairly strong con-

* The frequent association of the morbidly insistent processes with the nervously 'degenerate' type is a commonplace in the literature of the subject, and a few years since it was, I believe, an almost if not quite universal dogma that considerable masses of insistent fears, impulses, or thoughts occurred only as part of the 'stigmata' of degeneracy. The possibility of the development of even elaborate systems of such insistent impulses upon a basis of wholly acquired neurasthenia was maintained by Dr. Cowles, in his well-known paper on *Insistent and Fixed Ideas* in the *Amer. Journal of Psychology* (vol. I. p. 222 sq.), and has also been asserted by others.

trast to the impression that the temporary nervous disorder of his early manhood, if it were taken alone, would leave upon our minds.

But a deeper estimate of such things I must leave to more competent judges. I have here only to present the facts.

I.

John Bunyan was born November 30, 1628, and died August 31, 1688. The principal known facts of his life which bear in any way upon the question of his health and constitution, apart from the narrative in the *Grace Abounding*, are as follows:* Bunyan was a native of the little village of Elstow, near Bedford. His family can be traced in Bedfordshire as far back as 1200. In the sixteenth century, an ancestor of Bunyan, and the wife of this ancestor, appear in court records as brewers and bakers. Thomas Bunyan, his grandfather, was 'a small village trader.' Difficulties in the courts are the occasion of some of the records preserved of these ancestors, but the difficulties named are petty, e.g., minor violations of excise laws, disrespect to churchwardens, and perhaps religious nonconformity.† Bunyan's father was notoriously, like Bunyan himself, a 'tinker' or 'brasier,' probably, says Brown, "neither better nor worse than the rest of the craftsmen of the hammer and the forge." Tinkers had, to be sure, in that time and place, a reputation as rather hard drinkers; but on the other hand they wandered much on foot, and so lived freely out of doors. Bunyan's father lived until 1676, dying at seventy-three years of age. The poet's mother was of a poor but very honest and thrifty family; she died when John Bunyan himself had reached the age of fifteen. Little more is known of the family before we reach our poet himself. He was not an only child. One sister is known to have died early. One brother is known to have lived until 1695.

*I use, for the most part, the principal recent biography, that of John Brown (2d edition, London, 1886)—an elaborate and extremely patient research into every discoverable detail relating to Bunyan's family and fortunes. Other recent accounts are those of Venables (in the 'Great Writers' series, London, 1888) and of Froude (in the 'English Men of Letters' series). The ground has thus been very thoroughly gone over, for all literary purposes, in recent years.

† Brown, pp. 27–31.

Of John Bunyan's childhood history we shall see a little soon. In youth he was apparently, until after the time of his marriage, of pretty lusty health. The 'wicked' early life of which he speaks so severely in his *Grace Abounding* proves, on the whole, to have been, physically speaking, a wholesome life, during all the time preceding his conversion. Alcoholic excesses and unchastity are, in the opinion of all his modern biographers, nearly or quite excluded by what we most certainly know of him at this time. At about sixteen years of age Bunyan was enrolled in the army, probably on the Parliamentary side, and remained some two years in service, but apparently without any physical ill effects. He married at twenty years of age, both himself and his wife being very poor. He now followed his trade as tinker. Within the next four years fall, first his conversion, and then the experiences of which we are principally to speak in what follows. In these years, furthermore, falls also the birth of his first child, a daughter who was very early blind. In 1653, after he had passed through these principal experiences, he joined the church in Bedford. In 1654 his second child was born, also a daughter. In 1655 he began that career as preacher which he continued thenceforward, so far as he was permitted to do so, until the end. In 1660 he was imprisoned in the county jail at Bedford, for violating the law by acting as an irregular preacher; and there he remained, in a confinement which varied in its degrees of strictness, for some twelve years. The physical strain of this imprisonment must have been great, and the mental anxieties involved were of the severest, as we learn from his own account; yet Bunyan plainly experienced no return of his previous mental troubles with anything like their old force. He was now often weak in body and depressed in mind, but never long despairing. He busied himself both in preaching to his fellow-prisoners and in writing. He was released in 1672. For three years thereafter he was at liberty. In 1675–6 he suffered a second imprisonment, during which it was, according to recent research, that he wrote the *Pilgrim's Progress*.* Thenceforth he continued working as writer and preacher to the end.

* Brown, p. 254; Venables, p. 151.

The list of his works contains 'sixty pieces,' says his first bibliographer, 'and he was sixty years of age.' One standard edition occupies four volumes octavo. His works are, of course, largely theological. They are certainly laborious productions, even apart from the genius involved; for this man was never trained to write.

As to his health otherwise, we know that, after 1653, there was a time in his early life when, as he says, "I was much inclining to a consumption, wherewith, about the Spring, I was suddenly and violently seized with much weakness in my outward man, insomuch that I thought I could not live." Other times, still later, he mentions, when he was 'very ill and weak'; and he notes great depression of spirits as characteristic of his state at all such times.* Brown † holds, concerning Bunyan, that "at any time he was far from strong" as to physical health. But when one considers his remarkable activity both as writer and preacher, and the long and severe strains to which he had been subject before he reached sixty years of age, and when one remembers also the possibly hypochondriac nature of the disorders of which his own account, as just cited, speaks, it seems hard, after all, to form any exact opinion as to the actual degree of the physical weakness of his constitution. One is disposed to set the work done and the external sufferings endured over against the rather meagre record of later illnesses in his life. "His friend," says Brown (a friend, namely, who wrote an account of Bunyan), "tells us that though he was only sixty he was worn out with sufferings, age, and often teaching." One remembers hereupon that a persecuted genius who had written 'sixty pieces' without having received any sort of early scholarly training, and who had passed more than twelve years in unjust imprisonment, and all his life in struggle, had a right to be somewhat worn at sixty.

He died of 'a violent fever,' or, as others say, of 'the sweating distemper,' after having been exposed to 'heavy

* "The Tempter did beset me strongly (for I find he is much for assaulting the soul when it begins to approach towards the grave, then is his opportunity."—*Grace Abounding* (Clarendon Press Ed.), p. 375.

† *Op. cit.* p. 390.

rains and drenched to the skin' while on a preaching journey. Bunyan was twice married. He had in all three daughters and three sons. His first child, born during the time of his early disorder—a daughter—was, as observed above, blind, and died before him. Descendants of another of his daughters are the only descendants of Bunyan still known to survive. The later history of the family is incomplete, but, as reported by Brown, contains nothing of any note for our present purpose,—no record, namely, of remarkable disease or ability.

Of Bunyan's outward seeming, in his later years, we have two good accounts by contemporaries. One runs thus:

"As for his person, he was tall of stature, strong-boned, though not corpulent, somewhat of a ruddy face, with sparkling eyes; . . . his hair reddish, but in his latter days time had sprinkled it with gray; his nose well set, but not declining or bending, and his mouth moderately large; his forehead something high, and his habit always plain and modest. He appeared in countenance to be of a stern and rough temper, but in his conversation mild and affable, not given to loquacity or much discourse in company, unless some urgent occasion required it; observing never to boast of himself or his parts, but rather to seem low in his own eyes and submit himself to the judgment of others. . . . He had a sharp quick eye, accomplished with an excellent discerning of persons, being of good judgment and quick wit."

The other account speaks of his countenance as 'grave and sedate,' and of a sort to "strike something of awe into them that had nothing of the fear of God." The writer adds that his memory was "tenacious, it being customary with him to commit his sermons to writing after he had preached them." Bunyan's executive ability in church management and discipline is also noted in this account. As to his eloquence as a preacher, all accounts agree. This great 'dreamer,' then, was also, in his later years, a man of decided practical power, dignified in bearing, accustomed to control other men.

II.

So much, then, for the man as a whole. As to the experiences of his early manhood, recorded in the *Grace Abounding*, biographers in general have felt their perplexing intensity and abnormity, but have been accustomed either to refer them once for all to Bunyan's theological associations and ideas, or else to conceive them as indeed somehow pathological, but then to define their abnormal nature with the utmost looseness and confusedness.*

Patent, then, as are the reported experiences, beautifully as Bunyan confesses them, transparently as he unveils himself, one still has to go almost alone in trying to portray their actual connections; for biographer after biographer has passed these connections by with blindfold eyes. Yet the story, read in its psychological aspect, is as follows:

As a child Bunyan showed some of the familiar signs of the sensitive brain. He is not at all concerned, in his Autobiography, to gossip as to any minor matters. He tells us almost nothing of the externals of his life. He is wholly concerned in setting forth what God has done for his soul. He feels it worth while, however, to describe to us, in beginning the narration of his spiritual conflicts, certain of his early mental experiences. In childhood, so we learn, his 'cursing, swearing, lying, and blaspheming' were very marked faults. To quote his own words: "So settled and rooted was I in these things, that they became as a second Nature to me. The which, as I have with soberness considered since, did so

* Macaulay, for instance, in his *Miscellanies*, declares that, at a certain point, Bunyan's mind began to be 'fearfully disordered'; but he then proceeds, with a very undiscriminating analysis of the data, to define Bunyan's mental symptoms so that, if this analysis were sound, they would make up a case of what we should now define as 'hallucinatory delirium.' This Bunyan's disorder very certainly was not, in any fashion whatever. Taine, who, as psychologist, should have seen more clearly, is, in his way, (in the account of Bunyan in the *English Literature*,) almost equally confused as to Bunyan's true temperament and condition, and even imagines the calm and self-possessed art of *Pilgrim's Progress* to be the outcome of the 'inflamed brain' whose sufferings are depicted in the *Grace Abounding*. But the Bunyan of 1650 was not yet the Bunyan of the *Pilgrim's Progress* of 1675. Venables and Brown, well as they summarize the salient facts, fail to see their psychological significance. Froude also appears to go wholly astray in this respect.

offend the Lord, that even in my Childhood He did scare and affright me with fearful Dreams, and did terrify me with dreadful Visions. For often after I had spent this and the other day in sin, I have in my Bed been greatly afflicted, while asleep, with the apprehensions of Devils and wicked Spirits, who still, as I then thought, laboured to draw me away with them, of which I could never be rid." To these persistent nocturnal terrors there were added still other and evidently often waking troubles, 'thoughts of the Day of Judgment,' which gave him fears and 'distressed' his 'soul,' 'both night and day,' so that "I was often much cast down and afflicted . . . yet could I not let go my sins." These experiences came "when I was but a child, nine or ten years old." "Yea," he adds, "I was also then so overcome with despair of life and heaven, that I should often wish either that there had been no Hell, *or that I had been a Devil*—supposing they were only Tormentors; that if it must needs be that I went thither, I might be rather a Tormentor, than tormented myself." Of such early sufferings we have several accounts besides the foregoing summary statements.

Childhood experiences of this sort have to be estimated as important in direct proportion to their depth and in inverse proportion to their dependence upon the suggestions to which a given child is subjected. These dreams were, plainly, in some instances very elaborate and detailed. Bunyan's later youthful ignorance, so freely confessed, concerning all theological matters indicates, however, that these fears and this despair were no part of any very coherent system of childish thoughts on religious topics. The content of his 'terrible dreams' was of course derived from what he heard at church and elsewhere; but a sufficient basis, in these suggested ideas, for such marked trouble seems very improbable. That the nocturnal terrors and the despair were in part primary symptoms of nervous irritability, one can thus hardly doubt. As to the depth of the experiences themselves, the very fact of Bunyan's careful report of them is, under the circumstances, convincing. For his Autobiography is, as has just been noted, extremely reticent as to all matters that he does not consider essential parts of the tale of God's dealings with his soul.

In youth, at what seems to have been the healthiest period of his life, these dreams left him, and were "soon forgot . . . as if they never had been." And now began the wilful and sinful time which Bunyan later so unsparingly condemns. That his sins did not include unchastity or drunkenness seems, as aforesaid, clear to all his recent biographers, and for good reasons too, into which I need not here enter. Bunyan was now a very active and daring lad, who, in his almost complete ignorance, as Froude and others have observed, had no other way of expressing his genius than by "inventing lies to amuse his companions, and swearing they were true" (Froude's expression), and by showing extraordinary ingenuity as the chief swearer and wild talker of the village, so that even 'very loose and ungodly' wretches, as Bunyan tells us, were shocked by the flood of bad language in which this still unconscious poet was moved to voice his latent powers. These offences, and the still worse crime of playing tip-cat on Sundays, abide later in Bunyan's memory as evidences of the depth of his lost condition during these days. Meanwhile, despite the vulgarity of his surroundings and the restless waywardness of his life, Bunyan would otherwise appear to have been, on the whole, an exceptionally pure-minded youth. His early education, obtained in a local school, was extremely meagre.

His boyish marriage must have involved serious responsibilities. He and his young wife had at first not 'so much household stuff as a Dish or Spoon' between them. But the wife, 'whose Father was counted godly,' had, as her inheritance from this now dead father, two religious books, which Bunyan read with her, yet, so far as he was concerned, without 'conviction.' But ere long these books and his wife's speech 'did beget within me some desires to religion,' and for a while Bunyan attended church busily, 'still retaining my wicked life,' but already feeling some doubtful concern as to his own salvation, and much admiration for the formal side of church worship. A sermon against Sabbath-breaking brought him his first 'conviction.' After service and dinner, that day, when his full stomach had made him already cheerfully forget his transient remorse, he went, as usually on Sunday afternoons, to play his game of cat. But having struck the cat one blow from the hole, "just as I was about to strike it a second time, a Voice did suddenly dart from Heaven into my Soul, which said, *Wilt thou leave thy sins and go to Heaven, or have thy sins and go to Hell?* At this," he goes on, "I was put to an exceeding maze. Wherefore, leaving my Cat upon the ground, I looked up to Heaven, and was as if I had, with the Eyes of my understanding, seen the Lord Jesus looking down upon me, as being very hotly displeased with me, and as if he did severely threaten me." The result of this sudden internal vision, of which he said nothing to his comrades, was an immediate sense of his general sinfulness, an overwhelming despair, which kept him standing 'in the midst of my Play, before all that were then present,' until, with a swift dialectic characteristic of all his later experiences, he had reasoned out the conclusion that it was now too late, since he had sinned so much, and that the only hope was to go back to sin, and take his fill of present sweets. "I can but be damned, and if I must be so, I had as good be damned for many sins as damned for few." He thereupon went on with the game, and in the immediately subsequent days swore, played, and 'went on in sin with great greediness of mind.'

The automatic internal vision seen with 'the eyes of the understanding,' but seen more or less suddenly, with extraordinary detail and with strong emotional accompaniment, appears henceforth as a frequent incident in Bunyan's inner life, and later became, of course, the main source of his peculiar artistic power. He was plainly always a good visualizer. But this automatic organization of his images was an added characteristic of the man, and an invaluable one. This 'power of vision' remained, as the *Pilgrim's Progress* itself shows, late in life; and without it our 'dreamer's' genius could not be conceived. In his times of depression these visions, in later days, took on the shading of his mood; but in themselves they were of course signs, not of depression, but of poetic power. Apart from other and serious causes of disturbance they plainly never approached near to any hallucinatory degree; and Bunyan always describes them so as to distinguish them clearly from hallucinations, even when his condition, as described, is one of great agitation.

Shortly after this time the reproof of a neighbor again startled Bunyan from his reckless ways, and he resolved to begin in earnest the work of reform. The result was a period of a year (or probably somewhat less), during which he undertook nothing less than a systematic course of conscientious self-suppression. He 'left' his swearing at once, and in a way that astonished himself. He gave up his games as vain practices; after a long struggle he even abandoned dancing. He read the Bible; he lived a life of reform that astonished his neighbors; "for this my conversion was as great as for Tom of Bethlem to become a sober man." Inhibition of all outwardly suspicious deeds became the one rule of his life. He still wholly lacked what he later regarded as true piety, and he indulged in some spiritual pride in view of the approbation of his neighbors; but he cultivated a painful scrupulosity. We can well conceive how the material cares that beset this very poor but now married youth, and this sudden change from a careless life, of numerous relaxations, to an existence wherein every act was a matter of scruple, and wherein the opinions of all his neighbors were now so much taken into account, must have involved a considerable strain. The immediate consequences were characteristic of the whole case.

(*To be continued.*)

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (I.)

BY PROFESSOR HUGO MÜNSTERBERG.

A. Memory.

(With the assistance of Mr. J. Bigham.)

The experimental study of memory, important both for psychology and for pedagogics, is as yet only begun. The only experiments we have are those of Ebbinghaus, which cover simply the question of the influence of repetition and of the time-interval, made with but one material (syllables), and with only one subject*. Many other questions arise if we wish to understand the mechanism and the conditions of reproduction and memory. We began our systematic study of these questions by an investigation the purpose of which was to determine the action of disparate senses in recollection, especially to discover whether the different senses act at the same time independently, or help, or hinder each other. Collateral questions as to the influence of various modes of presentation, of various content, etc., could be answered by the same experiments. Studies on the reproduction of more complicated content, on the influence of the filling of time-interval, etc., will be reported later.

The simple apparatus used in the experiments consists of several series of ten small squares of paper of different colors, and of several series of black numbers, each one mounted upon a square white card of the same size as the colored squares. For audible presentations the names of the colors or numbers were spoken by the conductor of the work, while for visible presentations those colors and numbers were exposed upon a black background. The subjects were supplied with several corresponding series of colored squares ($3\frac{1}{2}$ cm) and

* The experiments on memory by G. E. Müller and F. Schumann (Zeitsch. f. Psychol. Nov. 1893), published while this article is in press, have in like manner no direct relation to the present investigation.

mounted numbers. After learning the presented series the subjects arranged these small numbers and colored papers in the order of the given series as these were recollected. The purpose in using numbers and colors was to secure a presentation-content as free from associations as possible. The length of the series consisted of either 10 or 20 presentations. Two sets of digits and colors were used, so that the same presentation might be given twice in a series even of ten presentations. The content of each series might be entirely audible, entirely visible, or partly audible and partly visible; it could consist wholly of numbers, wholly of colors, or of numbers and colors arranged in groups, in pairs, or in alternation. Thirty-two different kinds of series were employed: 20 successive audible numbers, 20 successive audible colors, and 10 successive colors alternating with 10 numbers (for instance: red, 6, gray, 2, green, 0, yellow, 7, etc.), 10 successive audible numbers followed by 10 successive audible colors, or the reverse, etc., 20 successive visible numbers, 20 simultaneous visible numbers, 20 successive visible colors, 20 simultaneous visible colors, 10 successive visible colors alternating with 10 successive visible numbers, 10 visible colors alternating with 10 visible numbers all presented simultaneously, etc., etc., 10 successive colors audible followed by 10 successive numbers visible, 10 numbers visible followed by 10 colors audible, 10 numbers visible alternating with 10 colors audible, 10 successive colors visible and 10 numbers audible presented in pairs (number given while color is seen), 10 successive colors visible and their names heard at the same time, etc.—altogether, 32 combinations in perfectly symmetrical arrangement. The time taken for learning each single presentation was 2 seconds, therefore 40 seconds for a series of 20 presentations, seen or heard, simultaneously or successively presented. The subjects saw or heard the series only once; the recollecting was done immediately at the conclusion of each single series. The subjects were cautioned against the use of mnemonic devices, and were informed beforehand of the length, the character, the content, and the mode of presentation of each series. Five subjects of the average age of 24 years took part, all making the same experiments,—each man working fifty hours, during the winter of 1892–93. Every day a few short series were used as preparatory practice; these were not recorded. Provision was made for resting the subjects. To avoid any disturbance by mere training, each person made only two experiments at a time with each of the 32 series. All the results which are to be compared came, therefore, under the same conditions of practice, training, and fatigue. The errors were recorded solely as displacements of the single presentations in each series; blue-red instead of red-blue were therefore two errors, so that in a series of 20 presentations 20 errors were possible.

We shall first consider the 32 kinds of series only as audible, visible, or mixed, and disregard all other elements in the experiments. We add for each of the five subjects the presentations of all the visible series and the errors made in them. The pure visible series offered 2140 presentations (colors or numbers); in these series A. P. had 365, Bu. 413, E. P. 479, etc., errors. If we take the percentages and calculate them in the same way for the pure audible series, the result is:

Visible series—per cent of error: Bi. 18.7%, Bu. 19.3%, A. P. 17.1%, E. P. 22.4%, W. 25.1%—average. 20.5%.

Audible series: Bi. 34.1%, Bu. 31.4%, A. P. 25.1%, E. P. 35.9%, W. 31.6%—average, 31.6%.

With all the subjects the visual memory excels strongly the aural when they act independently. When sight and hearing act together as in the mixed series (excluding of course those series in which numbers or colors are seen and their own names heard at the same time), we have the following data:

Mixed series: Bi. 44.4%, Bu. 46.9%, A. P. 26.5%, E. P. 41.9%, W. 38.9%—average, 39.3%.

The memory for mixed series is therefore much weaker than for visible, and with one exception (A. P.) also weaker than for audible presentations. *When the two senses act together in recollection, they hinder each other.* But a special analysis of the mixed series can be made, to secure comparative tables of the audible and visible presentations which are variously combined in them. In the mixed series the percentages of errors in visible presentations are: Bi. 45.2%, Bu. 50.2%, A. P.

26.7%, E. P. 41.2%, W. 41.7%—average, 41.0%. In audible presentations: Bi. 43.5%, Bu. 43.5%, A. P. 26.2%, E. P. 42.5%, W. 36.0%—average, 38.3%. Within the mixed series the aural memory is with one exception stronger than the visual, i.e., we have the interesting result, that in the united action of the senses of sight and hearing, their relative strength is just the reverse of what it is when they act independently. *When isolated the visual memory surpasses by far the aural; when combined the aural excels the visual.*

If we disregard the difference of audible and visible modes of presentation, we may consider the contents of our series as: (1) simple contents (series consisting entirely of numbers or of colors); (2) grouped contents (a group of colors followed by a group of numbers or the reverse); (3) alternate contents (a single color followed by a single number); (4) paired contents (a number and a color presented together); (5) doubled contents (a color or a number visible and its name audible at the same time). Beginning with the simple content, the errors are for numbers: Bi. 15.2%, Bu. 16.2%, A. P. 16.5%, E. P. 23.8%, W. 18.1%—average, 18.0%. Colors: Bi. 26.6%, Bu. 27.2%, A. P. 24.1%, E. P. 33.7%, W. 30.6%—average, 28.4%. The memory for numbers was accordingly much stronger than for colors.

With the grouped content there is a greater difficulty than in recollecting the simple content. Error for numbers in the grouped content: Bi. 26.5%, Bu. 34.7%, A. P. 15.7%, E. P. 18.1%, W. 20.4%—average, 23.1%. For colors: Bi. 43.1%, Bu. 36.1%, A. P. 13.8%, E. P. 35.8%, W. 30.4%—average, 31.8%.

With the alternate content the various series yield the following data: For numbers: Bi. 46.1%, Bu. 37.8%, A. P. 21.1%, E. P. 21.7%, W. 32.8%—average, 31.9%. For colors: Bi. 50.6% Bu. 60.0%, A. P. 39.4%, E. P. 48.9%, W. 38.3%—average 47.4%.

With the paired content the error for numbers is: Bi. 41.2%, Bu. 35.6%, A. P. 18.7%, E. P. 33.1%, W. 39.4%—average, 33.6%. For colors: Bi. 56.8%, Bu. 63.1%, A. P. 55.0%, E. P. 48.1%, W. 53.7%—average, 55.3%.

The general result of these four contents therefore is *that the memory is impeded by a closer combination of different contents.* The more closely numbers and colors are united in presentation, the weaker the memory. For 20 numbers alone or 20 colors alone the average error was 23.2%; for 10 numbers followed by 10 colors or the reverse, 27.4%; for 10 numbers alternating with 10 colors, 39.6%; for 10 pairs of numbers and colors, 44.5%. In all groups the error for numbers is smaller than for colors; this difference increases with the increasing closeness of the two contents.

The doubled content may finally be compared with those series in which the same number of figures or colors was only audible or only visible. The errors are these:—Ten numbers heard: Bi. 6.2%, Bu. 4.3%, A. P. 17.5%, E. P. 23.5%, W. 18.9%—average, 14.1%. Ten numbers seen: Bi. 5.3%, Bu. 11.2%, A. P. 13.7%, E. P. 15.9%, W. 6.3%—average, 10.5%. Ten numbers seen and heard at the same time: Bi. 6.2%, Bu. 2.5%, A. P. 2.5%, E. P. 7.5%, W. 1.2%—average, 3.9%. Ten colors heard: Bi. 25.0%, Bu. 30.3%, A. P. 26.4%, E. P. 32.5%, W. 32.1%—average, 29.3%. Ten colors seen: Bi. 15.3%, Bu. 17.8%, A. P. 13.6%, E. P. 20.9%, W. 21.9%—average, 17.9%. Ten colors seen and heard at the same time: Bi. 5.0%, Bu. 8.7%, A. P. 0.0%, E. P. 7.5%, W. 3.7%—average, 4.9%. *A series of presentations offered to two senses at the same time is much more easily reproduced than if given only to sight or only to hearing.*

We consider finally the differences of simultaneous and successive presentations. As all the audible series had to be successive, the pure visible series only give a basis for comparison. Half of the visible series were successive, the other half simultaneous, both halves corresponding in every respect except in the manner of presentation. The time was the same, as 20 simultaneous presentations were to be looked at during 40 seconds and each of the 20 successive ones for 2 seconds each. The error was for simultaneous series: Bi. 13.6%, Bu. 9.8%, A. P. 15.3%, E. P. 17.9%, W. 15.0%—average, 14.3%. For successive series: Bi. 18.5%, Bu. 23.3%, A. P. 16.6%, E. P. 24.1%, W. 21.6%—average, 20.8%. With each observer the memory was stronger for simultaneous than for successive presentations.

B. The Intensifying Effect of Attention.

(With the assistance of Mr. N. Kozaki.)

It is usually held that when the attention is directed to objects of sense, its effect is not only to increase the clearness and liveliness of the impressions, and strengthen the resulting associations in consciousness, but also to intensify the impressions themselves. The majority of psychologists, influenced partly by the experiences of daily life and partly by theoretical considerations, have acceded to this popular view. But Fechner long ago pointed out that a piece of gray paper does not appear lighter, nor the ticking of a clock louder, however much we direct the attention to them. And more recently Stumpf has shown in his *Tonpsychologie* (I. 71, II. 291) that the ordinary views are by no means self-evident. James (*Princ. of Psychology*, I. 426) comes to the same conclusion as Stumpf, and closes his discussion of the matter with the remark: "The subject is one which would well repay exact experiment." An account of such experiments I shall now give.

The only experiments on the subject (those of Helmholtz and Stumpf) concern the bringing into prominence of one from among several simultaneous impressions. It is clear that the interpretations here may differ. Our problem was to arrange the experiments in such a manner that the intensities of two impressions of moderate strength could be compared, and at the same time the attention be directed toward one and away from the other. In this way we examined intensities produced by light, sound, and the lifting of weights, and also the distances between visible points, the distances serving as measures for the intensity of the sensations produced by the movement of the eyes. The method always employed for diverting the attention was as follows: the subject was directed to give his attention fully to the adding of numbers, which in the case of the optical impressions were read to him, and in the case of the auditory impressions were read by him. The adding took place before and during the time the stimulus was present. Since the order of the stimuli to be compared is of great influence upon the judgment, two sorts of experiments were arranged for each series. In one case, the attention was directed to the first stimulus, while the second was perceived with diverted attention; and in the other case, the attention was directed to the second stimulus, while the first was perceived with diverted attention. In order to discover from these series the influence of the attention, independently of other conditions, both series must be compared with the results of experiments in which the attention was either directed to both stimuli, or turned away from both. If we designate attention to the first stimulus by A, and that to the second by A', and, correspondingly, the inattention by I and I', we have then for each sense and for the same magnitude experiments with $A—A'$, $A—I'$, $I—A'$, and $I—I'$. $A—A'$, as well as $I—I'$, give the constant error resulting from position, although with a different mean variation. It appears, however, that the results in the two series are different; the overestimating of the second stimulus in the case of $A—A'$ being much more marked than in the case of $I—I'$. Accordingly, we should compare those series only in which the judgment is made under the same conditions. That is, $A—A'$ ought to be compared only with $I—A'$, and in the same way $I—I'$ only with $A—I'$. And from such comparisons the direction of the changing influence of attention must appear. Obviously the actual numbers are valid merely for the relations of these stimuli chosen arbitrarily, and only their relative value, considered as plus or minus, comes in question.

The optical distances were given by an apparatus consisting of a black cloth surface, 80 cm square, upon which were two white points. The vertical distance between these points could be changed by a screw upon the back of the screen, and the exact distance moved could be accurately read (*Beiträge*, Heft IV.). At the beginning of each experiment the white points were covered by a strip of black paper. This was swung to one side, leaving the points visible for 3 sec.; the strip was then returned to its place for 6 sec., during which time the distance of the points was changed; and finally the points were again made visible for 3 sec. The normal distance which was exposed first in one half of the series and second in the remainder was 30 cm. This was compared with 27.5, 28, 28.5, 29, 29.5, 30, 30.5, 31, 31.5, 32, and 32.5 cm, the various pairs being given in irregular order but symmetrically in each series. The judgment was then noted, i.e., whether the second length appeared greater, equal to, or less than the first. For the light-stimulus a gray was used, produced by a black rotating disk with a white sector. The disk was hidden behind a black screen, then shown for 3 sec.; then it was covered for 8 sec. while the sector was changed, and finally shown again for 3 sec. The normal size of the white sector was 90°, and this was compared with 65°, 70°, 75°, 80°, 85°, 90°, 95°, 100°, 105°, 110°, and 115°. The striking of a metallic ball upon an ebony plate served for the sound. The ball was held by an electro-magnet and fell at the breaking of the current. The time between the two sounds was 5 sec. The normal height of the fall, 50 cm, was compared with 35, 40, 45, 50, 55, 60, and 65 cm. A signal preceded the sound, and simultaneously with this the adding of the numbers began as they were read. The weight was given by lifting a funnel-shaped vessel, held between the thumb and first finger. The elbow rested upon the table, and the weight was raised without movement of the wrist. Weights were put into the funnel in such a way that they could be easily changed. The funnel was supported from below, and the hand lifted it and after 4 sec. lowered it again. After a pause of 6 sec. it was again raised for 4 sec., but with the weight changed. A weight of 300 gm, including, of course, the weight of the vessel, was compared with weights varying from 250 gm to 350 gm with successive intervals of 10 gm.

From only two of the five subjects did we get complete series with weights (1280 experiments with each); from two in the distance experiments (440 with each); from four with the lights (440 with each); and from two with sounds (280 with each). The calculation was made by dividing the judgments into correct and false; the false being those of overestimation (i.e., smaller taken for equal, or smaller and equal for greater), and those of underestimation (i.e., equal taken for smaller, or greater for equal or smaller). Then the per cent was calculated, and the preponderance of the overestimation over the underestimation (or *vice versa*) was determined. The number of the overestimations, or underestimations, depends upon the sensitiveness and upon the attention at the time at which the judgment is given. The preponderance of the one over the other gives the constant error. The question therefore is, how the constant error changes when calculated first for *A—A′* and then for *I—A′*, or first for *A—I′* and then for *I—I′*. The absolute numbers are of course dependent upon the chosen gradations of the stimulus, and consequently have no value of their own. On the other hand, the direction of the change ought to bring out clearly the law of the relation. The results are as follows:

Distances.—First person. *A—A′*. The second stimulus was correctly judged, 72.2%; overestimated, 18.8%; underestimated, 9%. Preponderance of overestimation, 9.8%.

I—A′. Second stimulus correctly judged, 63.7%; overestimated, 21%; underestimated, 15.3%. Preponderance of overestimation, 5.7%. If *A—A′* gives 9.8% and *I—A′* only 5.7%, for the preponderance of overestimation, the difference means that the second stimulus is overestimated more frequently by 4.1%, or that the first stimulus is underestimated by that amount, when the attention is turned to the first stimulus. The directing of the attention, therefore, causes the first stimulus to appear weaker.

The same result followed for *I—I′* and *A—I′*. In the case of *I—I′* the second stimulus was correctly estimated 61%; overestimated, 22.5%; underestimated, 16.5%. Preponderance of overestimation, 6%. In the case of *A—I′*. correctly judged, 69.4%; overestimated, 21.2%; underestimated, 9.4%. Preponderance of overestimation, 11.8%. The overestimation of the second stimulus, or the underestimation of the first stimulus, occurs therefore more frequently (11.8%, — 6%, i.e., by 5.8%) when the attention is directed to the first stimulus.

Second person. *A—A′*, preponderance of overestimation, — 3.2% That is, with this person the underestimation of the second stimulus preponderates. *I—A′*, preponderance of overestimation, — 11%. Difference, 7.8%, by which the first stimulus is less often overestimated, when the attention is turned to it.

I—I', preponderance of overestimation, — 2.4%; *A—I'*, + 3.5%. Difference, 5.9%. For both persons, therefore, the distances, when attentively observed, appear smaller than those perceived in a state of inattention. This is true for both classes of judgments.

Sound.—First person. *A—A'*, preponderance of overestimation, 16.2%; *I—A'*, 9.4%. Again, therefore, the attention causes the first stimulus to appear smaller. *I—I'*, 2.5%; *A—I'*, 5.8; that is, the effect of the attention here also is to weaken the first stimulus.

Second person. *A—A'*, 4.7%; *I—A'*, 4.0%; *I—I'*, 1.6%; *A—I'*, 3.2%. The differences are here exceedingly small, but in each case in the direction that shows the weakening effect of the attention.

Light.—I. *A—A'*, 0.6%; *I—A'*, 5.7%; *I—I'*, 5.5%; *A—I'*, — 2.7%. Here the effect of the attention is an intensifying one in both cases. The same occurs in a much less degree with a second subject.

II. *A—A'*, 3.8%; *I—A'*, 4.4%; *I—I'*, 1.5%; *A—I'*, 1%. But in the case of two other subjects there is again a marked decrease due to the attention.

III. *A—A'*, 16.2%; *I—A'*, 9.7%; *I—I'*, 4.8%; *A—I'*, 8.9%.

IV. *A—A'*, 14.7%; *I—A'*, 10%; *I—I'*, 7.5%; *A—I'*, 12.2%.

Finally, the diminishing effect of the attention comes out most strongly in the experiments with weights.

Weights.—I. *A—A'*, 10.8%; *I—A'*, 2%; *I—I'*, 3.7%; *A—I'*, 8.5%.

II. *A—A'*, 4.3%; *I—A'*, — 7.6%; *I—I'*, 2.7%; *A—I'*, 8.3%.

The unexpected result is reached, therefore, *that all stimuli appear relatively less when the attention is from the outset directed to them.* The light experiments with two persons form the only exception. Now it has often been urged that the changes in the brightness of a gray disk do not always run subjectively parallel to the physical changes. Physically speaking, the increase of darkness is a decrease of light. Psychologically speaking, the increase of darkness may be looked upon as an increase of a positive quality, just as well as the increase of light. If we grant, now, that these two subjects were inclined to perceive the changes in this sense, the intensity of the darkness is with them lessened by the attention, and thus the single exception vanishes. But even if we disregard these results, those for distances, sounds, and weights remain fully in agreement with each other. The attention causes the intensities to appear smaller. The explanation seems to me to rest in the fact that we must always judge intensities relatively, the standard being in our muscular tensions. The interesting experiments of G. E. Müller have shown how much we underestimate weights if, in consequence of a particular preparatory motor adjustment, we lift them with too great a tension of the muscles. Exactly this ought to occur in cases in which the attention is previously prepared. We judge the intensities relatively to our sensations of tension. Let these, however, be previously strengthened by expectant attention and the stimulus will appear weaker than if the stimulus itself were to arouse reflexly all the corresponding muscular tensions. We interpret our retinal images as spatial magnitudes on the basis of our feelings of convergence and accommodation. In the same way the feeling of intensity comes to represent the intensity of a stimulus only through its relation to the subjective sense of strain. If we purposely strengthen the subjective strain simultaneously with the strains aroused by the stimulus, the stimulus will indeed appear stronger, because we interpret the tension as the result of the stimulus. In the same manner, if we wish, we can make certain beats of a rhythm appear stronger by a voluntary accentuation. If, on the other hand, the tension precede the stimulus as an element in the preparatory adjustment of the attention; and if consequently it be interpreted by consciousness, from the outset, as a subjective function, the increase of the tension aroused by the stimulus can appear only slight, the ratio of the two intensities has become reversed, and accordingly the stimulus is slightly underestimated.

C. A Psychometric Investigation of the Psycho-physic Law.

(With the assistance of Mr. W. T. Bush.)

The theory of the judging and comparing of differences of sensation has thus far rested chiefly upon the study of just perceptible and just imperceptible differences. But recently the conviction has arisen more and more that as soon as the deductions from such experiments step beyond the mere establishing of a threshold of excitation, they come upon presuppositions, which are at least open to discussion. Sensations are not magnitudes which can be added. Weaker sensations are not contained in stronger. Differences of sensation, then, should appear equal to us only when they arouse in us equal feelings of difference; which feelings depend upon the degree of the similarity of the sensations, and upon other circumstances. I have already tried to show that it is probable that we measure and compare these feelings of difference subjectively by means of sensations of tension. The question arises, how far we also possess objective means for measuring the differences of sensation. The degree of the subjective difference is obviously identical with the relative ease of discrimination. If, then, we possessed a measure for the ease with which we discriminate between two stimuli, we should have a measure of their subjective difference. In time-measurements we do have exactly this. We ought to designate as equal the differences between two pairs of intensities of stimuli, if equal times are necessary to distinguish them. It is clear that this method, which frees us from just perceptible differences, has at the same time the advantage that it is applicable to the study of differences of quality, as well as of differences of intensity. If the psycho-physic law be true, the time necessary to discriminate between weights of 100 and 200 grams must be just as long as the discrimination-time for 200 and 400 grams.

That group of stimuli, with which we began the study of this relation, consisted of lines of various lengths. It is well known that in this field the threshold of excitation follows Weber's law within wide limits. The problem, then, was to compare stimuli whose differences were always clearly perceptible. To eliminate individual differences, the measurement of the time was effected by the method of chain-reaction, the fundamental principles and advantages of which I have elsewhere described (*Beitr. z. exp. Psych.*, Heft IV. p. 40). A chain of persons so reacted that the first person gave the stimulus to the second, and the second through his reaction gave the stimulus to the third, and so on until the last person gave the stimulus again to the first. The occurrence of the first stimulus and that of the last reaction accomplished respectively the opening and closing of the current in a Hipp's chronoscope, and so the time was measured which elapsed between the first stimulus and the last reaction. If we are dealing with reactions after choice, as in this case, it is clear that the time occupied is the sum of the discrimination-times of all the persons, with the exception of the first. The first person knows the stimulus, and therefore has no choice to make at the end, but only a simple reaction. If we subtract the simple reaction-time of this first person from the total time, and divide the remainder by the number of other persons, we obtain the average discrimination-time, whose relative value must change with the difficulty of the discrimination. For optical stimuli this method is of course not applicable without the use of complicated apparatus. The instruments which we employed permitted the use of five different optical stimuli for any desired number of persons. Each subject had upon the table before him a black metal plate 50 cm square. This was inclined a little away from the subject and had in its centre a round opening 6 cm in diameter. The eyes were opposite this window and at a constant distance from it. At the same time, the five fingers of the right hand rested upon a set of five electric keys, made invisible by a box. If one of these five keys was pressed down by the first person, one of the five pictures appeared immediately in the window of the second instrument. This was accomplished by five electro-magnets, on the back of the plate. If the armature of one of these was drawn down, a lever was released at the end of which there was a circular frame the same size as the window. As soon as this lever was released, it was thrown forward by a strong spring so that the frame stopped exactly behind the window, and the paper in the frame—upon which might be pictures, or colors, or words, or, as in this case, black horizontal lines upon a white ground—became visible to the subject. The five levers and the five frames (all of hard rubber), and the five electro-magnets, corresponding to the five electric keys, were so symmetrically arranged that every lever had exactly the same distance to move. To obviate the possibility of observing the direction from which the lever came,—which would facilitate the discrimination of the optical stimuli,—the window was covered by a black shutter of hard rubber, which was opened in two halves, through the action of electro-magnets and springs, as soon as one of the keys was pressed down. The same current that carried the frame to the window opened the shutter in front of it, and thus the recognition of direction was rendered impossible. One electric battery was sufficient for the whole apparatus, and it was only for the chronoscope that a second battery was necessary.

We made use of three only of the five frames and worked with six instruments; the total time thus always included the reaction-time of the first subject, Mr. Bush, and the discrimination-time as well as the choice-time of the other five. In the first three frames of each instrument there were—to choose a single example—white disks with lines 5, 10, and 15 mm long. Each subject had upon the table before him a corresponding disk with a line 10 mm long, and was expected to look at this until immediately before the time when the line to be judged came to the window of the instrument. This line was 5, 10, or 15 mm, according to the key which his neighbor had pressed; and a judgment was to be given as quickly as possible as to whether the line were shorter, equal, or longer than the standard line seen immediately before. Then as quickly as possible the corresponding key was to be pressed, the first if shorter, the second if equal, and the third if longer. Since the working of the instrument, as well as the choice between the three fingers and the movements themselves, remained the same in all the experiments, and since all the magnitudes experimented upon were clearly distinguishable, the differences in time were in direct relation to the differences in discrimination.

To come more closely to the question of the pscho-physic law, we were obliged to choose such intensities of stimulus as were multiples of certain simple differences. For such fundamental differences we chose first, 2.5—5—7.5 mm, secondly, 4—5—6 mm, and thirdly, 4.5—5—5.5 mm. From each of these relations four multiples were investigated; i.e. 4—5—6, 8—10—12, 12—15—18, 16—20—24. In this way we obtained twelve groups of experiments. Each group contained four series, and each series fifteen experiments, which followed one another in a wholly irregular arrangement and included five greater, five equal, and five smaller stimuli. In each of the twelve groups, therefore, there are twenty cases of each kind. And since in every case the choice-times of five persons were measured at the same time, we have 3600 discrimination- and choice-times, which were scattered throughout the entire winter of 1892–93, and were conducted with all possible regard to practice, fatigue, etc.

The averages must be especially calculated from each set of twenty cases; first, because the ratio, 4 : 5, is not the same as 5 : 6, and therefore the discrimination will also be different; and secondly, because the equal judgments must not be confounded with the greater or the less.

If now 0.15 sec. be subtracted, as representing the reaction-time of Bush, and the remainder be divided by five, so that every number represents the average of a hundred single reactions, we have the following results: ($\sigma = 0.001$ sec.).

First Group.

	2.5—5—7.5 mm	
Smaller: 512σ	Equal: 574σ	Greater: 530σ
	5—10—15 mm	
Smaller: 491σ	Equal: 551σ	Greater: 527σ
	7.5—15—22.5 mm	
Smaller: 460σ	Equal: 539σ	Greater: 505σ
	10—20—30 mm	
Smaller: 443σ	Equal: 519σ	Greater: 505σ

Second Group.

	4—5—6 mm	
Smaller: 572σ	Equal: 737σ	Greater: 685σ
	8—10—12 mm	
Smaller: 557σ	Equal: 733σ	Greater: 637σ
	12—15—18 mm	
Smaller: 526σ	Equal: 710σ	Greater: 600σ
	16—20—24 mm	
Smaller: 534σ	Equal: 647σ	Greater: 578σ

Third Group.

	4.5—5—5.5 mm	
Smaller: 792σ	Equal: 856σ	Greater: 836σ
	9—10—11 mm	
Smaller: 756σ	Equal: 844σ	Greater: 775σ
	18—20—22 mm	
Smaller: 698σ	Equal: 778σ	Greater: 760σ
	27—30—33 mm	
Smaller: 682σ	Equal: 772σ	Greater: 740σ

All the figures are of course relatively large, since they include the discrimination-time for three magnitudes and the will-time for three fingers. It is especially noticeable that the time in the equal cases is without exception longer than in the greater and smaller cases. This fact, which was also often subjectively noticed, rests evidently upon the consideration that, in the cases of the longer and shorter judgments, there was a summation of two similarly directed judgments. The longest line is longer than the middle one, and much longer than the shortest. This latter judgment strengthens the former, and so the correct reaction is accelerated. In the case of the middle line, however, the two opposite judgments are mutually conflicting. The estimating of the longer line, also, is shown no less regularly to take longer than the estimating of the shorter. The reason lies evidently in the fact that all the relations of the lines represent multiples of three numbers, which have equal absolute differences: and, consequently, the relative difference between the longer and the middle lines is less in each case than the relative difference between the middle and the shorter lines. To discriminate between 5 and 6 must therefore take longer than to discriminate between 5 and 4. This leads us to the psycho-physic law, which claims that equal subjective differences are correlated with equal objective relations of stimuli. There is no doubt that this law is corroborated for line-lengths by our psychometric investigation. The four series of each group maintained the same relations of stimuli. A wide-reaching constancy in the corresponding numbers cannot be disputed. Group I., containing the relations 2.5—5—7.5, multiplied by one, two, three, and four, gives for the shortest line intermediate values from 443σ to 512σ; for the middle line, from 519σ to 574σ; and for the longer line, from 505σ to 530σ. Group II., containing the relation 4—5—6, multiplied likewise by one, two, three, and four, gives for the smaller line intermediate values from 534σ to 572σ; for the middle line, from 647σ to 737σ; and for the longer line, from 578σ to 685σ. Finally, Group III., with the relation 4.5—5—5.5, multiplied by one, two, four, and six, gives for the shorter line 682—792σ; for the middle line, 772—856σ; and for the longer line, 740—836σ. The limits between which the intermediate values of the three groups vary do not overlap; the figures for the shorter line being, in Group I., 443—512σ; in Group II., 534—572σ; in Group III., 682—792σ; and for the other categories equally marked differences. The approximate validity of the psycho-physic law for optical distances admits of proof, therefore, by psychometric methods.

But at the same time it is evident that this validity is only approximate, and that there exists a perfectly regular variation from the law, in that the subjective difference increases, i.e., the difficulty of the discrimination decreases, with the increasing length of the line. In every group we see that the times become smaller, the higher the number by which the fundamental relation is multiplied. While 792—856—836σ are the times that correspond to the numbers 4.5—5—5.5 mm, only 682—772—740σ, i.e. about a tenth of a second less, is needed with 27—30—33 mm lines, six times as long. For our subjective discrimination, therefore, *the stronger effect of the* *relative differences of stimuli is constantly influenced by the weaker effect of the absolute differences of stimuli.* The numbers show clearly how superior the psychometric method is to the other psycho-physic methods, for the finer analysis of the processes in an act of discrimination; and that too without regard to the fact that we are freed from the necessity of using the just perceptible differences in a way that is theoretically questionable. Similar psychometric investigations with weights, sounds, and lights are in progress, but are not yet completed.

D. Optical Time-content.

(With the assistance of Mr. A. R. T. Wylie.)

The discussion on the time-sense, which has been carried on in recent years with more warmth than politeness, has unfortunately not led thus far to much agreement either as to results or as to explanations. On the other hand agreement has happily been reached in the statement of the question. It is not our task to follow the earlier investigators and pile up numbers about the comparison of times, without regard to the subjective means by which we judge the magnitude of the times; but rather to study these subjective means, and so to arrange the experiments that the analysis of the characteristic psychical factors is made possible. We are also agreed that the results of experiments thus far made are not sufficient for the support of the common theories. They must be supplemented in various directions. Another point is also universally granted: that the judgment of longer time-lengths, as hours, days, years, which rests upon the manifoldness of the presentations, and is indirect throughout, must be clearly distinguished from the comparison of lesser time-lengths, e.g., parts of a minute. In the latter, it is true, the indirect time-consciousness is not wholly wanting; but the direct time-sense stands foremost. Our problem is to study the mechanism of this direct time-sense.

Farther this again is universally admitted, viz., that the judgment of the shortest times, say periods shorter than two seconds, does not take place under the same conditions as the judgment of longer time-lengths, say five, ten, or twenty seconds. The study of these two processes is accordingly to be kept distinct. The investigation of such longer time-lengths is theoretically the more important, since it allows a wider latitude to the change of outer conditions, as well as to self-observation. With this in mind I prepared the investigations on time-content which I published a short time ago (*Beiträge zur exp. Psych.*, Heft IV. p. 89). The experiments gave the general result, that time-intervals of from eight to twelve seconds, marked off and filled with auditory impressions, can, with the most varied content, be compared with sufficient accuracy to show that the judgment does not depend upon the number of separate presentations in the interval; while on the other hand these presentations do exercise a constant influence upon the estimate of the time. It was shown, namely, that those lengths were constantly underestimated, whose contents highly engrossed the interest of the observer. Words appeared shorter than noises, verses shorter than the strokes of a pendulum, chords shorter than simple tones, and sentences shorter than strings of nonsense-syllables. These results corresponded throughout with those theoretical views which I had formed earlier, on the basis of the self-observation of my subjects, the analysis of the results of others, and experiments of my own. And all the more recent researches substantiate these views much more strongly than the experimenters themselves are inclined to admit. I mention here only one point drawn from these theoretical considerations. The subjective measure for such time-lengths seems to me to lie in sensations peripherally aroused by muscular activity, especially by the strains and relaxations which take place in the various groups of muscles conditioned upon bodily reactions to changing intensities of stimuli. Such reactions occur in the functions of breathing, in the voluntary movements of the eyes, limbs, etc. We can therefore compare intervals with some certainty, even when the number of outer stimuli filling them is quite varied. On the other hand we lose that standard of comparison as soon as our attention is fully directed to those outer stimuli and thus withdrawn from our muscular sensations. This is plain from the fact that at the start all subjects are quite helpless in the comparison of intervals with different contents. The influence of practice consists just in this,—that one learns to divide the attention between the presentations of the outer stimuli and the bodily sensations. The more strongly these stimuli absorb the attention, the more must the bodily sensations retreat into the background of consciousness, even with experienced observers, and the shorter must the lapse of time appear. The less interesting the stimuli, the more obtrusive the bodily sensations and the longer the apparent time. It is clear that the results mentioned correspond perfectly with these theoretical deductions.

I then proposed to continue the research by investigating the influence of the time-content, when the marking off and filling of the time-intervals was accomplished by optical in place of auditory stimuli. Such optical experiments upon the time-sense have thus far never been made, if we disregard earlier investigations on the comparison of rapidities.

The method of investigation was as follows: a Ludwig kymograph, with drum placed vertically, was so set that every point moved exactly one centimeter in a second. Between the drum and the subjects there was a screen of black paper, in which an opening 2 cm square was cut. Immediately behind this opening moved the black paper which covered the drum. On a level with the opening there were fastened pieces of paper of any desired color or degree of brightness, of uniform surface or covered with print. These passed by the square opening at the rate of one centimeter a second, and the lengths to be compared followed one another immediately with no intervening period. Suppose for example that yellow and red papers are fastened to the drum. The subject sees first a black, then a yellow, then a red, and finally again a black square, and judges whether the red seems to occupy a longer, shorter, or equal time, as compared with the yellow, in passing by the opening in the screen. If letters or numbers were employed as stimuli, the white papers on which they were printed served as the lengths for comparison. The numbers and letters were arranged singly, or in groups, at such irregular intervals that their number gave the subject no clue whatever. The question is really therefore one of comparing spatial extents, in which, since the lengths pass by with equal rapidity, our time-sense can give the only standard for judgment. The lengths to be compared were so chosen that in every experiment one length was 10 cm, and the other 7, 8, 9, 10, 11, 12, or 13 cm. Every full series comprised twenty experiments, in which 10 was compared four times with 9, 10, and 11 respectively, and twice with 7, 8, 12, and 13. And, further, in ten cases 10 cm served for the first, and in ten cases for the second length. These twenty possibilities followed irregularly in every series. Then in the next series the order of the first series was reversed. If at the start yellow was the first and blue the second length, in the succeeding twenty experiments blue was the first and yellow the second. Then the same forty experiments were repeated. The experiments were, therefore, distributed with perfect symmetry, exactly as in the case of my auditory experiments, which have been already described. Meumann's criticism of my method has only the more strongly convinced me of its correctness.

Obviously the time-order exercises a great influence. A strong tendency was shown to overestimate the second length. Since, however, the influence of the position does not here concern us, we have only to eliminate it by reckoning together the results of the symmetrical experiments in both positions. If in one hundred experiments yellow was first and blue second, and in another hundred blue was first and yellow second, we have to ask how often in two hundred experiments the yellow appeared longer than the blue. We find that the one content was objectively longer than the other forty times in every one hundred experiments, equal to it twenty times, and shorter forty times. If we halve the equal judgments and assign them equally to both sides, fifty per cent would constantly appear longer and fifty per cent shorter—if the quality of the content were without influence—since the influence of position has been eliminated. The actual result may now be given.

Six subjects took part, and each one was given more or less preliminary practice; for here as in other investigations some practice proved necessary. The good experiments were as follows:

I. Yellow and light green were compared. The results fluctuate throughout and show no decided tendency. Manifestly both colors make equal claims upon the attention.

II. Brilliant yellow and dark red. A decided tendency in each person is clearly manifest, but it varies with the individuals. In one hundred cases yellow is estimated longer, as follows: with B., 62.3%; H., 67.8%; J., 42.6%; K., 46%; P., 43.8%; W., 39.5%; that is, two persons overestimated and four underestimated. The tendency is in every case so strong that there may be a question here as to the individually different attitudes of the attention. In all the following groups, however, the tendency is perfectly uniform.

III. A continuous color is compared with a series of from six to ten strips of different colors and of varying widths. The many-colored band appears regularly shorter than the continuous color. In each one hundred experiments the one continuous color appeared of longer duration: for B., 59.5%; H., 63.8%; J., 51.2%; K., 54%; P., 65.3%; W., 58.7%—average, 58.7%. Here it appears to me unquestionable that the changing colors attract the attention more than the monotonous color.

IV. A continuous color is compared with a series of from six to fifteen letters on a white background. The letters appear shorter. The color is held to be of longer duration, as follows: with B., 51.2%; H., 55%; J., 49.1%; K., 52.3%; P., 50.8%; W., 62.3%—average 53.5%. The underestimating of the letters is therefore somewhat less. Evidently, single letters, moving by so slowly, have far less tendency to catch the attention than bright changing colors. But the result gains especial interest from comparison with the following group.

V. The same optical impressions as in group IV. But the subjects were obliged now to take care to keep the letters in memory, and write them down after each experiment. The result is that the color appears longer than the letters, as follows: with B., 62.3%; H., 71.5%; J., 53.2%; K., 56.3%; P., 58%; W., 54.9%—average, 59.4%. The more marked straining of the attention leads, therefore, at once to a more marked underestimation of the time. This reaches its culmination in the next group.

VI. A continuous color is compared with a series of numbers between one and forty—these to be added as they pass by, and the sum noted. The color appears longer: for B., 75.6%; H., 54.3%; J., 52.8%; K., 69.3%; P., 56.2%; W., 68.7%—average, 62.8%.

With the use of optical stimuli I arrive, therefore, at exactly the same result as with auditory stimuli, viz., *that, irrespective of the number of the presentations, the times appear shorter the more the given optical time-content attracts the attention, and thus diverts it from the observation of the accompanying subjective phenomena produced by bodily changes.*

I limit myself here to the experimental results. What the subjects have said about their subjective experiences in reference to subjective rhythmical strains, respiration, and eye-movements I shall give at another time in a theoretical discussion of the time-sense.

E. A Stereoscope without Mirrors or Prisms.

(With Plate I.)

The stereoscope, as every one knows, is an instrument by means of which the two eyes are made to see different pictures, and yet to converge at a point corresponding to the apparent distance of the object seen. If these two pictures correspond to the views that a person's right and left eyes have respectively of a solid object, they unite, and the two flat pictures fuse into one solid figure. Up to the present, the problem of the stereoscope has been solved either by the use of mirrors (Wheatstone, Helmholtz, Duboscq, etc.), or prisms (Brewster, etc.). Without mirrors or prisms the converging eyes would be obliged to see the same picture always.

But this is a fact for simultaneous impressions only, not for successive impressions; which, as far as I know, have never been considered available for stereoscopic purposes. As the stroboscopic disks and the zoötrope demonstrate, the subjective visual impression considerably outlasts its objective stimulus. If, then, we suppose that for the stereoscopic union it is not the simultaneity of the visual stimuli that is necessary, but only the simultaneity of the two visual impressions, the effect must also be produced when the two pictures are presented to the two eyes in rapidly-changing succession, in such a way that the impression upon the left eye is still effective when the right eye is stimulated, and *vice versa.* But in this case it is evident that no mirror or prism is necessary, since now the eyes can rest in their natural position of convergence; and the two pictures appear successively at the same spot—provided only that care be taken that each picture be always accessible to the corresponding eye, and to that eye only.

The easiest way of accomplishing this is by using rotating disks. Stroboscopic disks preceded the ordinary stroboscopic drum of to-day; and disks, with slits for the right and left eyes respectively, and separated from one another by equal angles, have also been made use of for other optical purposes (Sanford, Dvorak). The only thing which I found necessary, then, was to fasten such a disk upon the same axis with, and at a certain distance from, another disk, so that the pictures were displayed alternately to the right and left eyes, the pictures being placed at angular distances corresponding to the slits, and at equal radial distances from the middle point. The slits must be as narrow as possible, in order that the pictures may be seen unmoved; and must be as numerous as possible, in order that the pictures may be seen clearly. A trial gave the expected effect with surprising vividness.

The form of apparatus used by me is as follows (see Plate I.): a strongly built color-mixer, or a centrifugal machine placed vertically, is provided with a steel axle 40 cm long, in such a way that one half of the axle is in front and the other half behind the machine. On both the front and the back of the axle is a brass screw, which holds securely a card-board disk. In order that the distance of the disk could be changed at will, I used perforated brass disks with collar-attachments which can be slid along the rod and secured at any desired point. The disk to be fastened in front is made of heavy black card-board, and has a radius of 25 cm. The disk has twelve slits, six inner and six outer. The inner ends of the former are 10 cm from the centre and the slits are 5 cm long: i.e., the outer ends are

15 cm from the centre. At 18 cm from the centre the outer slits begin and they end at 23 cm. The inner and outer slits alternate regularly at angular distances of 30°. The slits are bounded by radii, so that their widths increase from within outwards. The outer slits are 8 mm wide at their outer ends, the inner slits 5 mm. If, now, one sits before the disk in such a way that the eyes are on a level with the shaft, one eye is always obscured while the other sees through one of the slits. If one sits in front of the left half of the disk, the inner slits correspond to the right and the outer slits to the left eye. And if the propelling wheel be set in motion, so that the disk makes, say, five revolutions in a second, each eye must look thirty times a second upon the second disk behind. This second disk, which is fastened to the rear half of the shaft, is, in its simplest form, a circle of white card-board, of 25 cm radius, upon which, at intervals of 30°, are drawn twelve figures whose middle points are 16.5 cm from the centre of the disk. Six of these twelve figures are for the right and six for the left eye, the two kinds alternating regularly. One disk has, for example, the well-known outline of a truncated pyramid, with which the result is that the smaller inner square is six times on the right side and six times on the left side of the larger square. On the other hand, the middle points of all the twelve larger squares are at exactly equal distances from the edge of the disk, so that during rotation they completely cover one another. If now the rear disk be so secured that any one picture for the right eye stands opposite any inner slit, the pictures for the left eye are then, of course, opposite the outer slits. If the shaft be now set in rotation, the eyes can converge readily upon the second disk, which is perhaps 20 cm distant, and then the right and the left eyes look upon the same spot of the background of the rapidly changing pictures. Subjectively, however, we believe that we are continually observing the solid object. The pyramid stands out in relief, as with the best stereoscopes. If one looks in turn through the right side of the disk, one sees the pyramid hollow; since now the outer slits correspond to the right eye, and the right eye, therefore, sees the picture intended for the left eye. For the same reason the succeeding and preceding pyramids upon the left

side also appear hollow, while only that one which stands exactly opposite comes out in relief. Of course pictures for this disk-stereoscope can be printed just as simply as for the zoötrope, etc. Instead of changing the disks each time, we use another variety of disk, which is so provided with slits that the desired pictures can be easily inserted. Instead of the twelve pictures, it is more convenient to make use of two only, as in the case of the stereoscope. The disk with the twelve slits may also be used for the two pictures, if the rear disk be rotated exactly six times as rapidly as the one in front; an arrangement which is technically inconvenient. On the other hand, the twelve slits can be reduced in number, if only the pictures give a sufficiently strong impression. Eight slits with eight pictures generally give strong effects, but two slits with two pictures separated by 180° prove successful only with such clear impressions as, say, white stereometric drawings upon a black ground.

Obviously other forms of movement can also be chosen in order to present the two pictures successively; for example, both pictures may be glued back to back and turned about a horizontal or vertical axis. It depends only upon whether a rotation of 180° takes place between the view of the left eye and that of the right. A very convenient further arrangement is this: a black paper cylinder, of say 20 cm diameter and 15 cm in height, is fastened to a vertical wooden disk and rotated like a color disk upon a centrifugal machine. If two slits are made in this for the two eyes, something like 120° from one another, and the pictures fastened to the inner side of the cylinder opposite the slits and equally distant from the base of the cylinder, so that they cover one another at rotation, the stereoscopic effect is well produced. The slits ought not to be 180° apart, since the pictures would then partly cover up the opposite slits. So it is evident that unlimited variations of the same principle are possible.

Among the practical advantages of this stereoscope I may mention as most important that it admits of an immediate union of the stereoscopic with the zoötropic effects. A dozen figures or more can be easily printed upon a disk just as they are now printed upon a single strip for the zoötrope;

and in such a way that the right eye's views of the phases of movement alternate with those of the left. The effect then is that the solid object is seen in movement. For the illustration of physical apparatus and machines in action, of animals in motion, of physiological and pathological forms of movement in man, and so on, unusually clear representations may be secured. A further advantage is the unlimited size of the stereoscopic pictures to be combined, and the circumstance that for each eye the distance of the picture from it can be chosen at will. Since the rotating wheel for mixing colors is very common in schools and elsewhere, it would require but a longer axle and some paper disks to add to the pedagogical equipment a stereoscope both easy to understand and easy to operate, having also the advantage that it is at the same time stroboscopic and strobo-stereoscopic. But the application of this simple apparatus to theoretical studies appears to me essentially more important. All the questions not only of stereoscopic vision, but also of binocular vision in general, the question of the rivalry of the visual fields, the questions of binocular color-mixing, of contrasts, of lustre, etc., can be studied here from a new point of view. By being able not only to give different pictures to the two eyes, but also to give at the same time different stimuli to each eye successively; by being able to vary at will the time between the stimulation of the two eyes; by being able to present a picture more frequently to one eye than to the other, etc., there arise a multitude of new and interesting problems.

SHORTER CONTRIBUTIONS.

ARITHMETIC BY SMELL.

BY FRANCIS GALTON, F.R.S.,
London.

It seems worth while to put a few simple experiments on record, which I made for my own satisfaction a few months ago, in order to assure myself that arithmetic may be performed by the sole medium of imaginary smells, just as by imaginary figures or sounds. I had first to familiarize myself with a variety of scents, for which purpose the following arrangement was provided. Each scent was poured profusely upon cotton wool, loosely packed in a brass tube $\frac{3}{4}$ inch in outside diameter, which had a nozzle at one of its ends. The other wide-open end of the brass tube was pushed into a tightly fitting piece of caoutchouc tubing, $4\frac{1}{2}$ inches long, and the opposite end of the tubing was stopped with a cork. Whenever the tubing is grasped by the hand, a whiff of scented air is forced through the nozzle; when the grasp is relaxed, fresh air enters through the nozzle and passing through the wool becomes quickly impregnated with scent. The apparatus is then ready to be used again. Whiffs of scented air may thus be sent out four or five times in moderately quick succession and be almost equally odorous throughout. In using the apparatus, I begin by breathing out slowly through the nose, to prevent any scent from being prematurely perceived; in the mean time the nozzle is brought below the nostrils. Then I simultaneously give a sudden grasp and a sudden sniff up. A separate apparatus is used for each scent. They are made as alike as possible, and are scarcely distinguishable; nevertheless it is well to operate with the eyes shut. The scents chiefly used were peppermint, camphor, carbolic acid, ammonia, and aniseed. I taught myself to associate two whiffs of peppermint with one whiff of camphor; three of peppermint with one of carbolic acid, and so on. Next, I practised at some small sums in addition; at first with the scents themselves, and afterwards altogether with the imagination of them. There was not the slightest difficulty in banishing all visual and auditory images from the mind, leaving nothing in the consciousness besides real or imaginary scents. In this way, without, it is true, becoming very apt at the process, I convinced myself of the possibility of doing sums in simple addition with considerable speed and accuracy solely by means of imaginary scents. Further than this I did not go, so far as addition was concerned. It seemed a serious waste of time to continue the experiments further, because their difficulty and complexity rapidly increased. There were also provoking lapses of memory. For instance, at the present moment, having discontinued the experiments for three months, I find my old lessons almost wholly forgotten. Few persons appreciate the severity of the task imposed on children in making them learn the simple multiplication table, with its 81 pairs of values each associated with a third value. No wonder that they puzzle over it for months, notwithstanding the remarkable receptivity of their fresh brains. I did not attempt multiplication by smell.

Subtraction succeeded as well as addition. I did not go so far as to associate separate scents with the attitudes of mind severally appropriate to subtraction and addition, but determined by my ordinary mental processes which attitude to assume, before isolating myself in the world of scents.

Few experiments were made with taste. Salt, sugar, citric acid, and quinine seemed suitable for the purpose, and there appeared to be little difficulty in carrying on the experiments to a sufficient extent to show that arithmetic by taste was as feasible as arithmetic by smell.

THE PSYCHOLOGY OF INFANT LANGUAGE.

BY PROFESSOR JOHN DEWEY,

University of Michigan.

In his interesting and valuable article on *The Language of Childhood*,* Mr. Tracy undertakes, upon a basis of 5400 words used by at least twenty different children, to determine the relative frequency of the various parts of speech. Before making some remarks, I wish first to submit my own mite for the further use of students. A refers to a boy; B to a girl, 20 months younger.†

A at 19 *mos. old.*

Parts of Speech.		Per cent.
Nouns . . .	68	60
Verbs . . .	24	21
Adjectives .	13	11
Adverbs . .	4	3
Interjections	6	5
Total . .	115	100

Pronouns, prepositions, conjunctions, none.

B ‡ *at* 18 *mos. old.*

Parts of Speech.		Per cent.
Nouns . . .	76	53
Verbs . . .	40	28
Adjectives .	2	1
Adverbs . .	9	6
Interjections	7	5
Pronouns .	8	6
Conjunctions	2	1
Total . .	144	100

Prepositions, none.

For purposes of comparison, I append the per cents reached by Mr. Tracy by averaging all his results:

Nouns	60
Verbs	20
Adjectives	9
Adverbs	5
Pronouns	2
Prepositions	2
Interjections	1.7
Conjunctions	0.3
	100.0

* Am. Jour. Psychol., vol. VI., No. 1, reprinted in *The Psychology of Childhood*, Boston, Heath & Co., 1893.

† The presence of other children in the family should always, I think, form part of the data with reference to a child's vocabulary. At least, it is one of the old wives' saws on this matter that the presence of other children both hastens and extends a vocabulary.

‡ A's vocabulary was kept continuously; B's vocabulary was taken from words actually used within a period of five or six days; a number of words contained in her vocabulary four months previously do not appear at all.

I wish to remark (1) concerning the relative frequency of verbs, and (2) concerning the different rates of distribution in different children:

1. Mr. Tracy notes that since the relative frequency of verbs in the language is but 11 per cent, the child, *comparatively* speaking, uses verbs with 1.81 the ease with which he uses nouns, and makes some judicious remarks concerning the prevalence of concepts of activity in the child mind. I think he could make his case much stronger. Mr. Tracy, I take it, has classified his words according to the sense which they have to an adult, and I have followed that principle in my own table.* In a sense, however, this is as artificial as Mr. Tracy notes that it is to put knife under *k* instead of under *n*, because *we spell* it with a *k*. The psychological classification is to class the word according to what it means to a child, not to an adult with his grammatical forms all differentiated.

Such a classification would in all probability increase immensely the percentage of verbs. It is true that such a method demands much more care in observation, and opens the way to the very variable error of interpretation; but the greater certainty of the method followed above is after all only seeming—it does not express the *child's* vocabulary, but our interpretation of it according to a fixed but highly conventional standard. It is out of the question to redistribute the language of A and B, given above; but I subjoin the vocabulary of a child in his twefth month where contemporaneous observation makes me reasonably sure of what the child means:

See there; bye-bye; bottle; papa; mamma; grandma; Freddy; burn; fall; water; down; door; no, no; stop; thank you; boo (peek-a-boo); daw (used when he sees anything which he wants given to him)—17 in all.

Of the above, only the four proper nouns are, psychologically speaking, names of objects. Water is a verb as well as

* Phrases like 'all light,' 'all dark,' 'all gone,' 'out' (for 'go out'), etc., I have treated as verbs. It is obvious that they might be considered either as interjections or as adjectives. The relatively larger per cent of verbs in my table may be due to this classification.

a noun; door is *always* accompanied by gestures of reaching, and an attempt to swing the door back and fro; 'daw' is apparently a request, an expression of expectation of something good to eat and the name of a thing all together; bottle certainly has adjectival and verbal implications as well as nominal. At present I should regard it as a complex, 'nominal-adjectival-verbal,' the emphasis being on the noun, while six weeks previously it was, say, 'verbal-adjectival-nominal.' 'Stop'; 'no, no'; 'burn'; 'see there', etc., are equally interjections and verbs. 'Thank you' is at times a request for something, and is almost invariably said when giving an article to any one else. We have then a graded and continuous series, so far as *sense* is concerned, the proper names (23 per cent) at one end, and the interjectional forms 'no, no', 'peek-a-boo', at the other. These have a verbal coloring, however. Between these classes are a nominal-adjectival-verbal-interjectional complex, the verbal-interjectional meaning prevailing on the whole, the adjectival in all cases subordinate.* The tendency to apply the same term to a large number of objects ('ball' to ball, orange, moon, lamp-globe, etc.) can be understood, I think, only if we keep in mind the extent to which the formal noun, 'ball,' has really an active sense. 'Ball' is 'to throw' just as much as it is the round thing. I do not believe that the child either confuses the moon with his ball, or abstracts the roundness of it; the roundness suggests to him something which he has thrown, so that the moon is something to throw—if he could only get hold of it.

What I would suggest, then, along the line of a study of the distribution of vocabulary into parts of speech is such observation and record as would note carefully the original sense to the child of his words, and the gradual *differentiation*

* The fact that interjections fail so late, as a rule, in aphasia, taken with the highly immediate and emotional character of child-life, indicates the defective character of a method of classification which reduces the percentage of interjections to 1.7. The philologist's objections to making interjections a primitive form of speech, however sound grammatically, seem to me to rest upon attaching a limited, technical sense to the concept *interjection*, which is without ground psychologically. In the infant mind (whether race or child) the emotional state and the tendency to react aroused by an object *must*, I should say, be fused, and both precede any clear recognition of the 'object' as such, or of any objective quality.

of the original protoplasmic verbal-nominal-interjectional form (as it seems to me), until words assume their present rigidity.

2. No one can examine the statistics given without being struck by the great differences in different children. F, in Mr. Tracy's tables, has 15 per cent interjections; while K, with a vocabulary of 250 words, has none at all. F has 11 per cent adverbs; while K has but 2 per cent; in my own table, A has 4, while B has 9 per cent. So in my two, A has 11 per cent adjectives; B, 1 per cent; while Mr. Tracy's vary from a maximum of 13 to a minimum of 3 per cent. I believe the tendency in all psychological investigation, at present, is to attempt to get a *uniform* mathematical statement, eliminating individual differences; for pedagogical and ethical purposes, at least, it is these differences which are, finally, most important. And on strictly psychological grounds the varying ratio of adverbs and pronouns on one side and nouns and adjectives on the other must denote a very different psychological attitude—different methods of attaching interest and distributing attention. Observation of different mental traits as connected with these linguistic differences would not only add to the *terra incognita*, individual psychology (and it would seem that all psychology must be finally individual), but throw great light upon the psychology of language. How vague and formal at present our answers, for example, when we are asked to what psychological state and need an adverb corresponds!

WORK AT THE YALE LABORATORY.

BY E. W. SCRIPTURE.

The first year in the life of a laboratory is one of incredible difficulties and incessant labor in getting matters arranged. Nevertheless, we have been able to carry on several investigations and bring them to successful conclusion and publication.*

The most extensive investigation was that by C. B. Bliss on reaction-time and attention. The graphic method was developed so that records absolutely accurate to thousandths of

* *Studies from the Yale Psychological Laboratory*, 1892–1893, edited by E. W. Scripture, New Haven, 1893.

a second can be made and counted with less trouble than chronoscope records. The vibrating line receives a spark-record at the instant of the stimulus and another at the instant of reaction. The single waves, .01 sec., are counted and the odd tenths obtained by the eye. The necessary arrangement of the currents led to the invention of a multiple key. The reaction-time to sound was measured with and without disturbances of attention. When the attention was distracted by a steady light the disturbance of the reaction-time to sound was very small; with an unsteady, moving light it was very great. When the attention was disturbed by a steady sound, e.g., a tone, no disturbance resulted in the reaction to sound; with an intermittent sound, e.g., a metronome, it was very marked. In making these experiments some unexpected results were obtained, showing that the reaction-time to a sound heard in both ears is shorter than when the sound is heard only in one ear, even after making allowance for the difference in intensity.

Dr. Bliss made the attempt to determine the relation of changes in reaction-time to various other mental disturbances. After each set of experiments records were made of anything worth noting that had passed in the mind. The conclusions from these introspective observations are:

1. Reaction-time is constantly affected by irregular disturbances a large part of which may be detected by introspection.

2. Introspection is not to be trusted in estimating results.

3. Reactions to the wrong signal, reactions before the signal is heard, and the reflex nature of reactions are not sufficient criteria to distinguish muscular from sensorial reactions.

4. There are at least six distinct kinds of voluntary attention: ideational attention, neural attention, feeling attention, muscular attention, preparatory attention, and inattention.

5. The involuntary attention is constantly changing.

Experiments were made showing the influence of various distractions and mental operations on the rate of voluntary tapping and on the steadiness with which a lever could be kept at a given place.

C. E. Seashore succeeded in making the first trustworthy measurements on the time required for altering the accommodation of the eye. He has established three important principles:

1. Within certain limits the accommodation-time varies with the distance between the points for which the eye is to be accommodated.

2. It takes longer to change the accommodation from near to far than from far to near, and this difference in time varies directly with the length of the accommodation-time.

3. For equal distances in the same range the accommodation-time is greatest for points near the eye and decreases with the distance of the points from the eye.

The investigations on reaction-time in relation to intensity and pitch, made by Dr. M. D. Slattery, lead to these conclusions:

1. The law that the reaction-time decreases with increasing intensity of stimulus does not hold good for the sense of hearing, i.e., the reaction-time to tones is nearly the same for all moderate intensities.

2. The longer time registered for very weak tones or noises by some observers is probably not due to any conscious change, but is caused by hesitation as to the actual hearing of the stimulus.

3. The reaction-time to tones decreases as the pitch rises.

4. The view held by Exner, von Kries, and Auerbach and rejected by Martius—namely, that about ten vibrations are necessary to the perception of a tone, no matter what its pitch —is sufficient to explain the differences in the reaction-times for different tones.

5. In the domain of tactile stimulation by electricity the reaction-time decreases with the increase in the intensity of the stimulus.

The experiments of J. A. Gilbert on the musical sensitiveness of school-children involved in the first place the invention of a new piece of apparatus, the tone-tester, which has proved an exceedingly convenient instrument for much demonstrational work on the psychological methods. The sensitiveness to differences in pitch increases with age; at first rapidly, then very slowly. The least perceptible difference at 6 years is $\frac{12}{32}$ of a tone, at 19 years it is $\frac{2}{32}$. At 10 years and at 15 years the sensitiveness suddenly falls off.

A new reaction-key, designed to avoid the objections to the usual telegraph-key, is described in an article on the time of voluntary movement. Being a new instrument it opened up new methods of solution; one of these was the measurement of the time of voluntary movement. The time of flexion of the finger was found to decrease as the distance of movement increased from 5 mm to 20 mm; the time of extension, however, increased. This is explained by J. M. Moore as the result of the favorable leverage for the flexor muscles at the smaller distances.

An article on drawing a straight line makes the attempt to apply experimental methods to pedagogical problems. The average errors for various positions, inclinations, grasps of the pencil, etc., were determined.

The equipment of the workshop in the laboratory has proved an excellent investment. The room is the one most used in the building and is often quite overcrowded. To have accomplished without a workshop the amount of work actually done in the laboratory would have cost more than the two hundred dollars spent in its equipment; thus at the beginning of the second year we are in possession of a well-equipped workshop which has already paid for itself. The employment of a mechanic has furnished the opportunity for the invention of several pieces of apparatus. The multiple key has been much improved in a later model. A novel clock contact makes platinum contact in the middle of the arc of swing. A new drum built with the durability of a piece of machinery is run either by hand or by motor.

DISCUSSION.

PROFESSOR WUNDT AND FEELINGS OF INNERVATION.

A note to page 432 of Vol. I. of the fourth edition of Wundt's *Physiologische Psychologie* quotes an opinion of mine and corrects it in a manner that seems to demand a word of reply. When the external rectus-muscle of a man's eye (say the left eye) is wholly or partly paralyzed, objects lying in the left half of the field of view appear to that left eye to lie farther to the left than they really are. In Prof. Wundt's earlier writings he agreed with Von Graefe and others in explaining this phenomenon by the man's consciousness of the excessive leftward innervation which he must employ in turning his diseased eye towards the object. The existence of feelings of innervation was attacked presently by Bastian, Ferrier, and others, and this particular supposed case of it was explained away by G. E. Müller and myself. We pointed out that the true cause of the object's false leftward location was rather to be found in the inward squint of the right eye when the left one vainly or successfully turns to look at the object. The leftward innervation is indeed increased, but there is no need of assuming the *feeling* of it to be increased, when the feeling of its *results* in the turning of the *right* eyeball (even when its lid is closed or it is screened from the object) explains sufficiently why the man should think himself looking farther to the left with both eyes than he really is.

Professor Wundt, in the third edition of his book, definitively abandoned the theory of feelings of innervation. In the present fourth edition he adds to what he has to say upon the subject some novel remarks of detail. *Inter alia* he says that it is impossible to explain the false location of the object in the case before us by the position of the sound, or right, eye. It seems to me, however, that he has failed to understand correctly the facts and his authorities for them, and that Müller's and my explanation stands as firm as it did before.

Wundt says (p. 424): "To the movement of the sound eye the false localization cannot be ascribed, for the images seen by the two eyes are distinct, and only that of the lame eye is falsely placed." In the note to p. 432 he quotes a passage from Alfred Graefe and thereupon makes the remark that more particularly provokes the present note from me. I must leave Graefe's passage in the transparency and elegance of the original: "*Die Richtung in welcher sich das* (*dem, paralytischen Auge angehörende*) *Scheinbild von dem* (*vom normalen Auge herrührenden*) *wahren Bilde entfernt, liegt stets in der nach aussen projicirten Wirkungsbahn des gelähmten Muskels, d.h. in der Ebene, welche die Sehlinie um die Drehungsaxe desselben beschreibt.*" Wundt's remark hereupon is: "When therefore W. James (Psychology, ii. 506) and others aver that the displacement of the false image comes from the movement of the normal eye, they would seem to ascribe to this latter the marvellous capacity of a simultaneous twofold localization, first a normal one coming from the said eye's real position, and second an abnormal one, corresponding to the position which the paralytic eye is striving to reach."

What meaning the special quotation from Graefe may have for Prof. Wundt's mind I cannot tell, but the rest of Graefe's text, and the facts themselves are so simple that one wonders how there can be two opinions about them. The case Wundt is considering is apparently that in which both eyes are open, the object lies or moves towards the left, and the sound right eye turns to it and sees it where it is, whilst the lame left eye fails to rotate so as to fixate it and consequently gets its image on the nasal half of the retina or, in other words, sees the object in indirect vision to the left of the point at which it directly looks. During all this there is a convergent squint, the right eye being turned in and looking farther to the left than does the left eye.* The question now is: *where* do the two images appear? The left eye's image must in any case appear to the left of the right eye's image, because whilst the latter falls on the right fovea, the former falls on the nasal half of the left retina. But where does the right eye's image appear? In its real place, or thereabouts, according to all accounts. *Thus the position of the right eye is what determines a place, to the left of which the left eye's image is falsely referred.* There is no question of any twofold localization here by the right or normal eye. That eye sees in the direction of its own line of sight, of which direction it would appear to be made conscious by its feelings of rotation. The left eye also has feelings of rotation, but

* For simplicity's sake I omit the variation in which the left eye succeeds in rotating so as to fixate the object, whilst the right eye turns violently in, and, fixating a point leftward of the object, gets the image of the latter on the nasal half of its retina, or sees it in indirect vision to the right of the spot which it fixates. The principles of explanation are here the same.

they would appear to be overpowered by those of the right eye, first because the actual rotations of the latter eye are the stronger, and second because (as a host of similar pathological examples show) we are liable (until trained by contrary experience) to suppose, when we have intended a movement, that the movement has taken place. The patient intends to move both his eyes considerably to the left. He does so move his right eye only; but failing, in the novelty of the whole experience, to discriminate in his orbital feelings just what new and strange things have occurred, he thinks he has performed the entire movement as usual in spite of the fact that he has not. He sees double; he locates the left eye's image according to the fatal laws of retinal projection; and he gets a strong vertigo as the result of the unusual behavior of the field of view. How Professor Wundt himself would explain the wrong localization by the left eye without invoking either the right eye's position or the feelings of innervation in which he formerly believed, he does not deign to say.

The point is a minute one, certainly in itself not worthy of notice; and the existence or non-existence of feelings of innervation is an alternative on which, so far as I can see at present, no general theoretic consequences seem to hinge. I should consequently not have been stirred to write this note were it not that Professor Wundt's peculiar manner of revising his opinions is objectionable from the point of view of literary ethics, and is beginning, I fancy, to arouse in other readers besides myself an irritation to which it is but just that some expression should be given.

First, it would seem better, in issuing revised editions of works as weighty as those of this author, to name explicitly in the new prefaces the pages where modifications of doctrine are to be found. No one ought to be forced to read a thousand pages merely to ascertain what an author's newest formulations are. Second, it would seem well, in parts of the text where a change of view has occurred, to announce that fact explicitly in the text. And third, it would be fair, if one cited authors already identified with the new view, to cite them so as to award to them some degree of credit. In this overburdened age the reader has a right to clearness on every point. But Prof. Wundt's new prefaces contain no reference by pages to what is revised; his text habitually lacks any indication that his thought may once have been different from what it is; and his citations are almost always by way of discrediting the predecessors quoted and clearing their opinions out of the way. No one, I think, who should be introduced to Psychology by Wundt's third edition could come to any other conclusion than that Bastian, Ferrier, and others were adherents of a foolish theory of innervation-feelings to which Wundt himself now and ever stood opposed. In the fourth edition Münsterberg, one of the most original opponents of innervation-feelings, is quoted only once and then actually so as to make the reader think that he might most naturally have got his views from Wundt himself (see p. 431, note).

The mania for a plausible smoothness, the shrinking from an appearance of fallibility, seem in fact in Wundt's later writings to be driven so far as seriously to neutralize the clearness and value of the work. A thinker so learned, so intelligent, before whose encyclopædic capacity an entire generation bows down with cordial admiration, ought to be above such foibles. Not in such ways were the best parts of the reputation of a Fechner, a Mill, a Darwin, made.

William James.

Harvard University.

MR. JAMES WARD ON MODERN PSYCHOLOGY.

Mr. Ward's noteworthy article in *Mind* for January last, under the title '*Modern' Psychology: a Reflexion*, may be described as a critique of the fundamental conceptions on which Prof. Münsterberg and others of the younger physiological psychologists base their experimental work. It was in his brilliant little book, *Die Willenshandlung* (1888), that Prof. Münsterberg first advanced the view that all mental states, emotions and volitions as well as cognitive states, are simply complexes of sensations, that is, of elements each essentially similar to blue, hot, sour.* This view was briefly but severely criticised by Prof. Wundt in his article 'Zur Lehre von den Gemüthsbewegungen' (1890), who designated it as intellectualism, that is to say, the ignoring of any but cognitive elements.† It is against the same view that Mr. Ward's criticisms are directed; and though he prefers to call it presentationism, he is as outspoken in his condemnation of it as Prof. Wundt, whom he claims as an ally.

Now I observe that Mr. Ward uses the word presentationism in two senses, a narrow sense and a broad sense, which he equally condemns. In the narrow sense he means by presentationism the doctrine that all mental states may be resolved into sensations. In the broad sense he means by it the doctrine that psychology has to do solely with conscious events. And what I shall try to show is, that it is only in the narrow sense that Prof. Wundt agrees with him in condemning presentationism; whereas, if the word be taken in the broad sense,

* *Willenshandlung*, p. 62. Cf. *Beiträge zur exp. Psych.*, I. p. 28.

† Philos. Studien, VI. 3 (1890), pp. 387–8.

Prof. Wundt becomes himself a presentationist; in fact, if Mr. Ward realized how truly he is one, we should find him denouncing Prof. Wundt as the greatest and most dangerous of presentationists, instead of claiming him as an ally. This seemingly personal issue will be found, I believe, to involve an important question of psychological principle.

A word, first, in regard to presentationism in the narrow sense, the sense which Prof. Wundt and Mr. Ward agree in condemning. Here I will only say that I am inclined to sympathize with Prof. Münsterberg's critics. He himself admits that every sensation is accompanied by feeling as its inseparable subjective aspect. Every sensation, so the doctrine runs, has a quality and an intensity which are its objective aspect, and which represent the nature and strength of the stimulus, and an emotional tone, which expresses the attitude of the organism towards this stimulus. But it seems to me that the accompanying feeling gets but scanty justice when described as emotional tone. The description overlooks the fact that the feeling has an active as well as a passive side. For though retrospectively and with reference to the sensation it attends we call it pleasure or pain, yet prospectively and with reference to the changes it effects in consciousness it deserves the name of impulse or will. But this is not the question I wish to discuss, which is that of presentationism in the broad sense—or rather the doctrine I find Mr. Ward attacking under that not very appropriate name.

Presentationism in the broad sense is the doctrine, not that "all the elements of psychical life are primarily and ultimately cognitive elements," which is presentationism in the narrow sense, but that all the elements of psychical life are facts of conscious experience, and that "psychology has to do solely with conscious processes and events." This doctrine admits feelings and attention as distinct from sensations and ideas, and also a self which has these feelings and exerts this attention. But it holds that the feelings are facts of conscious experience, that the attention is a fact of conscious experience, that the self is a fact of conscious experience. The very being of feelings, attention, and self, as much as of sensations and ideas, consists in their being facts of conscious experience; if they were not such facts, we should never know anything about them. And since this is so, psychology may restrict itself to the facts of conscious experience, and trouble itself as little with the question of a soul, or of a subject not given in consciousness and without influence upon the course of conscious events, as physics does with the question of material substance. Its proper task is to study the empirical facts and to trace out their connections as physics and chemistry do those of material phenomena. Psychology may, in short, be an empirical or natural science, the science of conscious processes and events.

Now I think it would be safe to say that this general conception of the facts of mind and the duties of psychology with reference to them is shared by practically all of the younger men who take an interest in neurology and in experimental work, many of whom are very far from being presentationists in the narrow sense. I think further that there can be no doubt of Prof. Wundt's entire sympathy with this conception—and no doubt, for that matter, that it is largely from him that the younger men have learned it.* But, for Mr. Ward, a psychology which recognizes only conscious processes and events still falls under the reproach of presentationism. Such a psychology may succeed in explaining nine tenths of the facts, but when it comes to the other tenth, to the subject and its activities, it inevitably breaks down. And so I should like to consider, very briefly, whether Mr. Ward's account of the subject is the only possible account, or whether 'modern' psychology can offer one which shall be adequate to all the facts. I believe not only that it can do so, but that it can offer an account which will not be open to a very serious objection lying against Mr. Ward's, an objection admitted to be such by Mr. Ward himself.

Mr. Ward's account of the subject may be summarized in an introspective observation and three inferences.† First the introspective observation. The facts of mind cannot be properly expressed by saying, 'There are feelings, ideas, volitions,' but only by saying, '*I have* feelings, ideas, volitions,' or more briefly, 'I feel, I know, I will.' Every mental state, in other words, involves a subject by whom it is known or felt or willed. Now the three inferences. First, the subject must be conceived as distinct from the state which it knows or feels or wills. Second, it must be conceived as different in kind from all ideas or feelings or possibilities of such. Third, since all knowledge implies a subject which knows, all feeling a subject which feels, it follows that this subject, just because it is the subject, cannot itself be directly known or felt.

I imagine that the plain man, who began by acquiescing in Mr. Ward's judicious words, will be brought up with a start by this last conclusion. What! he will exclaim, we have no direct knowledge of

* Cf. Philos. Studien, VI. 3 (1890), p. 391.

† Mr. Ward is of course in no way responsible for the form in which I have stated his doctrine. His own best statement of it is that given in his valuable article *Psychology*, in the 9th ed. of the Enc. Brit., p. 39.

the subject at all? Then how do we ever come to know that there is such a thing? And Mr. Ward acknowledges that the plain man's question is a very difficult one to answer. In fact, he frankly admits, as I have said, that the difficulty of answering it forms a serious objection to his doctrine.

The difficulty is not lightened when Mr. Ward proceeds to draw the further consequence that feelings and volitions, being subjective facts as compared with sensations and ideas, cannot be given in experience any more than the subject. We do not know them directly, we only know *of* them by their effects. If we could know them directly, they would be cognitive states, not feelings and volitions. And here the plain man asks again, If we are not directly conscious of our feelings and volitions, how do we ever learn that there are such things?

Now the 'modern' psychologist surely has common sense on his side when he protests that pleasures and pains and desires and resolves are facts of conscious experience, and that the self too is a fact of conscious experience. But Mr. Ward immediately points out to him that there cannot be an experience without some one who experiences, a feeling without some one who feels; and that consequently, if the self is felt, there must be another hidden self which feels it. Feeling, he insists, implies a subject which is not itself felt.

It seems to me that the 'modern' psychologist's cue at this point is to turn upon Mr. Ward and demand his warrant for the assumption that the subject is not itself felt. This is the very essence of Mr. Ward's doctrine. The doctrine must either be accepted, or the assumption challenged. And I think that the 'modern' psychologist may challenge it with a courageous heart. For what is the source of this assumption? It is an inference from the introspective observation with which we started, the familiar fact of 'I know, I feel, I will.' But is there anything in this fact to justify the inference that the 'I' is not felt? Must we not rather say that the 'I' and the 'know,' the 'I' and the 'feel,' the 'I' and the 'will,' are equally facts of conscious experience? And if an inference is to be drawn, must it not be that all feeling involves a subject which is *also* felt, rather than that all feeling implies a subject which is *not* felt? But if this is the true account of the matter, how comes Mr. Ward to draw his inference that the subject is not felt? I believe that he is led to do so by a preconceived theory of consciousness, a theory not so much extracted from the facts as superinduced upon them.

There are two theories of consciousness. The first conceives it after the analogy of the eye, which sees other objects but cannot see itself. The other conceives it as analogous to light, which in illuminating other objects illuminates itself also.*

Mr. Ward's is the eye-theory. His principal argument in its favor is that the relation of knowledge logically implies two terms, a knower and a known, and that the knower must needs be distinct from the known, and therefore itself unknown. I should admit that this is true in a sense, but deny that it justifies the eye-theory of consciousness. I should hold that, though the knower is not known, it is nevertheless always experienced or felt, and should rely on concrete examples of knowledge to prove this. In representative knowledge, for instance in memory, that which knows is a present cognitive state, which of course is experienced. And in presentative knowledge, or attention, the self is not prevented from knowing, in the sense of attending, by the fact that it is itself dimly experienced. Mr. Ward's fallacy may therefore be said to lie in applying to conscious experience or feeling an analysis which holds good only for knowledge. As for the other argument, if it is another, that mental states are phenomena, or appearances, and must therefore appear to something, I should reply that it begs the question. For whether mental states are in the proper sense of the term phenomena is precisely the question.

The great objection to the eye-theory is, however, the difficulty already mentioned in regard to the knowledge of the subject. If the subject is not directly experienced or felt, it is impossible to understand how we ever learn of its existence. To my mind this is not merely an objection : it is a refutation.

The difficulty referred to is sometimes evaded in the following way. Though the subject cannot know itself at the moment when it knows, it is assumed that it can turn and know itself the moment after. Prof. James's line of argument on this point is so instructive that I cannot forbear reproducing it here.† After getting happily rid of the soul, and identifying the knower as the passing state, he finds that this state, just because it knows, cannot also be an object of knowledge. Thus the present moment of consciousness, instead of being the lightest in the series of mental states, becomes the darkest. Indeed, it is not an empirical fact at all. Only after it is gone have we any knowledge of it. As it vanishes it becomes the object of a new unknown state. It thus appears that we have no direct evidence of the existence of mental states while they last; they are not verifiable facts. For once, and once only, one is tempted to regret that Mr. James is such a devout reader of Mr. Shadworth Hodgson.

* Cf. Wundt, *Logik*, II. 502 ff.

† *Principles of Psychology*, I. p. 304.

Such difficulties and absurdities as these are the inevitable outcome of the eye-theory. In Mr. Ward's account, the self, the real efficient one, loses its status in consciousness and becomes a quasi-transcendent entity, little better than the soul in disguise. Our knowledge of it is a sort of standing miracle. We are cut off from ourselves: an impenetrable curtain is drawn between us and that which should be nearest and most familiar. In Mr. James's, we are told that the lamp of consciousness is dark while it burns, luminous only after it has gone out; that we cannot see it while it is present, but may do so as it begins to be absent; in short, that we may remember and reflect upon that which we have never experienced. The only escape from these absurdities and difficulties lies in breaking with the eye-theory, and putting the light-theory in its place. But can the light-theory explain the self and its activity in attention? This is the question that now calls for an answer.

An attempt to solve the problem of the self and its activity in accordance with the light-theory, the only such attempt in fact with which I am acquainted, is Prof. Wundt's much-maligned theory of apperception. This theory has been strangely misunderstood. It has oftenest been regarded as a form of the eye-theory, or even as a crude relapse into the old faculty psychology. Yet there is not the slightest ground for either of these assumptions, and I think it may be shown that the theory is perfectly consistent with the conception of psychology as a science dealing solely with conscious processes and events. In fact, one of the greatest merits of Prof. Wundt's psychologizing seems to me to be the consistency with which he holds to this conception. It is usual for psychologists to say that they have not been able to understand the theory of apperception. I believe that this is true both of Prof. Münsterberg who combats it, and of Mr. Ward who appeals to it against presentationism. Prof. Münsterberg combats it because he supposes it to contain non-empirical elements; whereas his own impersonal subject-Ego is a non-empirical element of the most obvious kind, a relic of the eye-theory, which might to the advantage of his psychology be altogether dropped.* Mr. Ward, on the other hand, supposes that the theory of apperception is a form of the eye-theory, like his own doctrine of the subject, while in truth it is the legitimate outcome of the light-theory.

The theory of apperception, then, is an attempt to solve the problem of the self and its activity in terms of the light-theory. According to Prof. Wundt, we have not two selves, an unknown subject and an

*Cf. *Beiträge zur exp. Psych.*, I. p. 38, and esp. p. 55: "So wenig wie die Netzhaut sich selber sehen kann. . . ."

objective self-consciousness,* or an impersonal subject-Ego and a personal object-Ego,† or a 'self as knower' and a 'self as known'.‡ We have but one, the self which is a fact of every one's conscious experience, the empirical self. It is the empirical self which attends —that is, determines what shall be attended to. But what shall be attended to depends on the contents of our minds, on the totality of our latent ideas—on these, however, not merely as ideas, but as accompanied by feeling—in other words, on our likes and dislikes, on our interests. These constitute the very essence of ourselves. The self may therefore be defined as the manifestation in consciousness of our latent ideas, of our likes and dislikes, of our interests, in so far as these are operative in determining what shall be attended to.§ According to Prof. Wundt, they are manifested in a state of fusion, under the form of a 'total feeling,' which occupies the background of consciousness.‖

Not only does the self, as thus conceived, determine what shall be attended to, but it does so actively; and we may rightly speak of its activity, and call it an agent. For the self is a relatively permanent fact; it has much the same permanence that belongs to a material object. And if we are justified in speaking of the sun as active when it melts wax, or of the wind when it moves the sails of a windmill, or of the windmill when it grinds corn, then to the same extent we are justified in speaking of the self as active when it attends.¶ Indeed, it is only so far as the latent ideas are active in attention that they are called the self.**

It will be evident that there is a wide difference between the relation of the self to the idea attended to and that of the eye to the object it sees; for the eye can catch no glimpse of itself, whereas the self which attends, though not in the focus, is yet in the fringe. Thus Prof. Wundt, after insisting that the facts of mind are one and all conscious events, proceeds: "Above all the percipient subject is not an independent spectator standing over against its own ideas, as under the treacherous figure of external sense-perception it is here represented, but forms an inseparable constituent of the psychic process itself." †† The reader may judge from this passage how far Mr. Ward is justified in claiming Prof. Wundt as an ally.

If the above account of the self is correct, there is evidently no

* Cf. Dr. Ward's article. † Cf. Münsterberg, *Beiträge zur exp. Psych.*, I. p. 55.

‡ Cf. James, *Psychology*, Briefer Course, pp. 176 and 195.

§ *Vorlesungen über Menschen u. Thierseele*, 2d ed. (1892), pp. 248–9, 269.

‖ Philos. Studien, VI. 3 (1890), pp. 392–393.

¶ *Vorlesungen*, p. 245. ** Ibid., p. 269.

†† Philos. Studien, VI. 3 (1890), p. 389.

ground for denying that feelings and volitions are facts of conscious experience because they are subjective. Nor is there any ground for denying this because they are cognitive states. For a state is cognitive, not because it is known, but because it knows; a memory, for instance, is cognitive because it knows a past experience. But cognitive states, like all others, are in their first intention facts of conscious experience; therein lies their immediate being. Mr. Ward's denial, in short, that we consciously experience our feelings and volitions is in reality, what he says it will seem, an extravagant paradox.*

If it be asked whether we can attend to our feelings, I should answer that we attend to them in attending to the cognitive states of which they are the subjective side. And the same remark applies to attention. Although what we ordinarily mean, when we speak of attending to attention, is that we remember states just past and attend discriminatively to the aspect of attention in them. This would, in fact, be my solution of Mr. Ward's puzzle about 'consciousness of consciousness' or reflection.

If, finally, it be asked whether we remember our feelings, I should say that we undoubtedly do as a matter of fact, but always in connection with the cognitive states of which they are the subjective side. I do not mean by this that past feelings are remembered by means of present feelings. Such an assumption would indeed make feelings cognitive. What I mean is that every experience may leave behind a representative idea of itself, an idea which will be cognitive of the emotional aspect as well as of the cognitive aspect of the experience, but which will itself be attended by a new feeling.

Sensations and ideas, then, feelings, acts of will, and the self as well, are all facts of conscious experience, and in this they have their being. The self with its feelings and activities is consubstantial with all other mental facts, and is known in the same identical way.

A chief point in the Kantian philosophy is the proof that we have no direct and intuitive knowledge of other beings than ourselves, but can know them only representatively, through the medium of our own conscious states. This doctrine seems to interpose an opaque barrier between us and the things we would know, and to make anything like a full and satisfactory knowledge of them forever impossible. Is our idea of an intuitive knowledge of things, then, nowhere realized? Are we as ignorant of ourselves as we are of other beings? Do "all the great realities escape us"? It seems to me that the Kantian doctrine requires a complement. Representative knowledge holds for

* Enc. Brit., 9th ed., art. *Psychology*, p. 44.

other beings, but it does not hold for ourselves. In conscious experience we have the realization of the demand which in the case of other beings was judged unattainable, the demand to know things as they are in themselves. Conscious experience is not less than knowledge, but more. In it we immediately grasp ourselves and know ourselves for what we truly are. Conscious experience is, in brief, the one point at which we come into immediate contact with reality.* It seems to me therefore that the philosopher is not less concerned than the psychologist to hold fast to the principle that there is nothing in consciousness of which we are not conscious, nothing in experience which is not experienced.

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* Cf. Paulsen, *Einleitung in die Philosophie* (1892), p. 377.

PSYCHOLOGICAL LITERATURE.

EDUCATIONAL.

The Science of Education: Its General Principles deduced from its Aim. By JOHANN FRIEDRICH HERBART. Translated from the German, with a Biographical Introduction, by HENRY M. and EMMIE FELKIN. pp. 268. Boston: D. C. Heath & Co. 1893. Price $1.00.

Apperception: A Monograph on Psychology and Pedagogy. By Dr. KARL LANGE, Director of the Higher-Burgher School, Plauen, Germany. Translated by members of the HERBART CLUB, and Edited by CHARLES DE GARMO. pp. 279. Boston: D. C. Heath & Co. 1893. Price $1.00.

The interest in Herbart's psychology is to-day based almost wholly upon its applications in education. This interest is not only wide-spread, but growing. In America it has reached very respectable proportions. In England it is gathering force. Even in France it is discernible. The reason for this is not far to seek. Ever since the conviction was established that sound educational theory must find its justification and support in psychology, there has been a searching for light. Bain and Herbert Spencer contributed something, and Sully a great deal; yet Herbart contributes still more. It is probably true, as De Garmo has said, that no other system of psychology assigns to the teacher so important a function as Herbart's, and no other is so well adapted for practical application in the work of education. Proof of this is to be found by measuring the success of its use in the elementary schools of Germany, and, under the stimulus of Frick, in some secondary schools as well.

The two volumes under notice are translations of material that has been found suggestive and useful in Germany by followers of Herbart. The first shows us Herbart himself, and the second analyzes what has come to be regarded as his main doctrine, that of apperception. In both works the purely psychological element is subordinated to the educational applications. They belong therefore to the field of applied or, as it is sometimes called, educational psychology. It may be said at the outset that both translations seem to be unusually well done, and that the thought has lost nothing—indeed at some points it has distinctly gained—by transference to the English.

Herbart cherished the individual and would not crush him under the weight of the mass. The individual's development, particularly on his active side, is constantly before Herbart as the educational aim. Government (*Regierung*), instruction (*Unterricht*), and discipline (*Zucht*) are the three educational stages or moments. They co-operate to produce intelligent character. In the discussion of each he combines the practical with the theoretical and formulates rules of procedure in dealing with the growing mind. If some of the analyses yield results that are almost too precise and if portions of the praxis are so formal and well-fitted as to appear mechanical, the blame must be laid on the psychological presuppositions on which they rest. For many of these are mechanical to the last degree. Yet the suggestiveness of the treatment given by Herbart is not to be denied or minimized. It has done and is doing an important work in the schoolroom.

Lange's exposition of apperception is much more psychological, though of course his eye is always fixed on educational practice. The assimilation of a perception is apperception, and in the process of being assimilated the perception gains in clearness. In a brief historical sketch, Lange shows that the idea of apperception is not an artificial one invented for the purpose of giving a new and more learned appearance to well-known educational maxims; but that, on the contrary, it has been under discussion since Leibnitz. Its meaning for Herbart is contrasted with the use of the term by Kant, and with the later theories of Lazarus, Steinthal, and Wundt. Admirable accuracy and precision characterize this portion of the book.

On the purely educational phases of these books this is not the place to enlarge. My purpose has been simply to call attention to them as indicating a tendency in the educational applications of psychology.

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THE NERVOUS SYSTEM.

1. *Zahl und Vertheilung der markhaltigen Fasern im Froschrückenmark.* J. GAULE. Leipzig, 1889.
2. *Développement des Éléments de Système Nerveux Cérébro-Spinal.* W. VIGNAL. Paris, 1889.
3. *The Structure and Combination of the Histological Elements of the*

Central Nervous System. F. NANSEN. Bergens Museums. Aarsberetning, 1891.

4. *Ueber einige neuere Forschungen im Gebiete der Anatomie des Centralnervensystems.* W. WALDEYER. Leipzig, 1891.
5. *Die Funktionen der Ganglienzellen des Halsmarkes.* O. KAISER. Leiden, 1891.
6. *A Microscopical Study of Changes due to Activity in Nerve-cells.* C. F. HODGE. Journal of Morphology, VII. 1892.
7. *Nuevo concepto de la Histologia de los Centros Nerviosos.* RAMÓN Y CAJAL. Revista de Ciencias Medicas de Barcelona, XVIII. Nums. 16, 20, 22, and 23. 1892.
8. *The Structures and Functions of the Brain and Spinal Cord.* V. HORSLEY. Fullerian Lectures for 1891. London, 1892.
9. *A Physiological, Histological, and Clinical Study of the Degeneration and Regeneration in Peripheral Nerve-fibres, after Severance of their Connections with the Nerve-centres.* W. H. HOWELL. Journal of Physiology, XIII. No. 5. 1892.
10. *Notes on the Arrangement of some Motor Fibres in the Lumbosacral Plexus.* Plates 20, 21, 22, 23. C. S. SHERRINGTON. Journal of Physiology, XIII. No. 6. 1892.
11. *Der feinere Bau des Nervensystems im Lichte neuester Forschungen.* M. LENHOSSEK. Berlin, 1893.
12. *The Nerve-cell considered as the Basis of Neurology.* E. A. SCHÄFER. Brain. 1893.
13. *The Arrangement of the Sympathetic Nervous System, based chiefly upon Observations upon Pilo-motor Nerves.* J. N. LANGLEY. Journal of Physiology, XV. No. 3. 1893.

In a recent paper Prof. Schäfer (12) has made some very useful suggestions concerning the nomenclature. We look upon the so-called nerve-fibres as outgrowths of the nerve-cell. Prof. Schäfer suggests that, since in all other cases we make the term *cell* cover not only the principal mass of cytoplasm with its nucleus, but also all the processes of this mass, it would be advantageous to be consistent in this case and, in describing the elements of the nervous system, to use the term cell in an inclusive sense.

In 1891 Prof. Waldeyer (4) in his admirable review of the recent studies in the anatomy of the central nervous system proposed the term *neuron* for the nerve-cell and its processes. This term was so convenient that many have already accepted it; but there is more to be said in favor of Schäfer's suggestion, which we here adopt. The processes of the cell are by him designated as *neurons*—so far as they form the axis-cylinder of the nerve-fibres,—or *dendrons*—so far as they form the protoplasmic prolongations. The examination of nerve-cells has been most successfully carried on by the methods of Nissl, Ehrlich, Weigert, and Golgi. Improvements in the technique have increased the number of processes which can be demonstrated in connection with the cell-body. The exact meaning of these processes is still open for discussion. The method of Golgi shows that in a typical cell, such as one from the human cerebral cortex, there are differences of form and outline which clearly mark the dendrons from the neuron. But as we descend the vertebrate scale, the two sorts of processes come to look much more alike under this treatment. This at once suggests that in the lower vertebrates the dendrons may be more similar to the neuron physiologically also (7). The older histology taught that many nerve-cells had but a single neuron which was unbranched and the function of which was to carry impulses from the cell-body to their destination. It also recognized bipolar nerve-cells which had two neurons, such as are found in the spinal ganglia of the fish, but the effort was not made to reduce these apparently divergent forms to a common type.

In the spinal ganglia of mammals we find a cell which during its development is at first dineuric (i.e., has two neurons), and later by the fusion of the two for a short distance into a common stem becomes mononeuric. This stem so formed, may be looked on as having two branches, along one of which nervous impulses arrive and by the other of which they leave the cell, each branch being continued as a portion of the single neuron which unites both with the cell-body. There is no reason that these two branches should be of the same size, and thus it is possible that all apparently mononeuric nerve-cells receive the incoming impulses by way of one or more of these branches, so called (11). Thus it is possible to reduce all nerve-cells to one type. It seems necessary that we should have some such pathway into the cell-body as well as one out of it. In some cases the surface of the nerve-cell is covered by minute disks which appear to be the enlarged termination of fine fibrils surrounding it. This arrangement suggests that impulses arriving by that fibre may in this way pass to the cell-body.

For the most part the neurons from elsewhere break up into fine brushes in the neighborhood of the cells among which they end; and just how the impulse passes from one nerve-cell to another is thus a matter of much speculation. The mass of evidence—not all—is to the effect that under ordinary conditions we do not have continuity between one nerve-cell and another, and, that being the case, the difficulty is to show how the nervous impulse crosses the gap. But the other alternative is always open to us, namely, to question the reality of the gap. The nerve-cell of the day, then, has a cell-body and processes which may be more or less different. When well differentiated these processes are assumed to be of two sorts. The dendrons are contrasted with the neuric processes, and of the latter there may be one or more. The length of the neuric process may range from a small fraction of a millimeter to more than a meter. It ends, however, always in a brush (7). The cell-body, the neuron, and the brush-like termination form the three important subdivisions.

One chief difference between cells lies in the length of the neuron. When this is very short, the brush is formed close to the cell-body and within the central system, and such cells appear to be interpolated in the pathways of the surrounding neurons. Wide study of the nervous system in different forms shows that the general arrangement of the elements is that of a series of cells so placed that from one cell-body a neuron passes to the neighborhood of another cell-body and there terminates in its brush, and so the units repeat themselves to the end.

When a group of cell-bodies is located at a point where incoming neurons terminate, each neuron may have connection with a number of cells, and in this way the impulse arriving at any point may pass on by either a number of paths or by one of the several paths open to it. Here we reach a fundamental fact, the explanation for which is obscure. It is plain that the possible pathways for an impulse within the nervous system are very numerous indeed. For example, any voluntary muscle can be contracted in response to any possible stimulus from any part of the sensorium. The arrangement which permits of this must be highly complicated. In view of this complication, it has even been suggested that the cell-body is, so to speak, a bystander, and that the physiological reactions go on in the branches of the neurons, the cell-body itself acting simply as a nutritive centre (3); but the idea has little to support it. Indeed, the size of the nerve-element has, without doubt, some connection with the energy it can put forth. Where a group of nerve-cells controls a limb, it is found to be the larger cells that are associated with the more proximal and more bulky muscles, which are at the same time those that do the most work. As the result of exercise, it is the nerve-centres that first run down, and muscular contractions cease because in some way the nervous impulse has changed. The size of cells increases so long as general growth continues; and in those animals in which growth is practically indefinite the range may be very great. In man, we consider that the formation of nerve-cells ceases some time before birth, so that the individual comes into existence with marked limitations regarding the possible number of elements composing the nervous system (1). The existence of this numerical limitation seems to offer difficulties in explaining the marvellous capacity for development, through exercise and training, which is so peculiar a character of the central nervous system. These difficulties are lessened, however, when we remember that by no means all these possible nerve-elements undergo further development.

Vignal's (2) study of the development of the nerve-cell shows in a given species of animals more and more cells that are fully formed and characterized with advancing age; and Kaiser (5), from the study of the spinal cord, shows that in man between birth and maturity the number of well-developed nerve-cells is doubled. There is no evidence that these cells are newly formed, but there is some evidence that they are from the first present *in petto* and developed in serial order.

Concerning the undeveloped nerve-cells between birth and maturity, we have much to learn; and, curiously enough, the current histological methods bring out these structures but poorly. There is another standpoint from which size is of interest. It has long been recognized that in man the male was possessed of the heavier central nervous system. This has been interpreted as indicating the mental superiority of the male. It is not our object to enter into a controversy in which there are so many vested interests, but to point out simply that among males the taller individuals also have a decidedly greater brain-weight than the shorter ones, and that another curious weight-relation exists, of such a nature that, when we compare the brains of the two sexes,—first from persons on the limits of intelligence, and second from those with just enough brain to keep alive, though idiotic,—we find in both cases the average weights less for the female than for the male. In these instances we may fairly assume that lack of complexity in structure is at the basis of the intellectual condition; and if such were the case, we could most easily explain the differences in the gross weight of the brains as due to differences in the size of the constituent elements. This, however, leaves the size of the elements still to be explained. Possibly when the experiments of Hodge (6) shall have been more extensively applied, we may get assistance from the comparison of the way in which small and large cells change as the result of exercise. Hodge finds that fatigued cells have changed their shape and grown smaller. Whether this change in the shape of the cell and its nucleus is simply the physical result of the shrinkage, or whether it is a remnant of the capacity for contraction, is a point that would bear examination.

Schäfer (12) suggests that the rhythmic manner in which central nerve-cells can be shown to discharge may be the result of a series of contractions which, if the axis-cylinder of the neurons be considered as made up of a series of tubules, might be supposed to give rise to variations in the surface-tension of the fluid substance within these tubules. The passage of this wave of varying surface-tension would then coincide with the passage of the nervous impulse. If this were the case, it should be possible to obtain both reinforcement and interference of two impulses. The tubular character of the cell-contents has been contended for by Nansen (3), but it is hardly probable that the structure of the nerve-cell is unique; and if this tubular character is genuine, indications of it should be found elsewhere. Again, the physiology of the nervous system is complicated by the fact of multiple innervation, in the sense that afferent and efferent fibres from more than one centre are distributed to the same region. The return of motor functions after injuries to the cerebral hemispheres or to the spinal nerves, the reactions of the pilo-motor nerves, all point to the fact that there is a preferred path for the impulses under normal conditions; but this may be by no means the only path (8, 9, 10, 13).

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APHASIA.

Ein Fall von Seelenblindheit und Hemianopsie mit Sectionsbefund. HERMANN WILBRAND. Deut. Zeitschr. f. Nervenheilkunde, II. 5 u. 6. 1892.

Ein Fall von aphasischen Symptomen, Hemianopsie, amnestischer Farbenblindheit und Seelenlähmung. E. BLEULER. Archiv für Psychiatrie, XXV. 32. 1893.

Die Bedeutung der Aphasie für die Musikvorstellung. R. WALLASCHEK. Zeitsch. für Psychol., VI. 8. Sept. 1893.

The study of the different forms of disturbances of speech, and of the various processes of disease in the brain which are capable of producing them, has thrown much light upon the normal action of the brain in thought. And as new forms of aphasia are discovered and the defects which they imply in mental processes are more closely analyzed it is quite clear that considerable insight can be gained into the mechanism of thinking. Before proceeding to review some recent observations upon aphasia it may be well to present a brief summary of the facts so far as they are already determined which are accepted regarding the physical basis of speech.

It is now known that as the physical basis of any word, be it noun or verb, there is a series of mental images acquired through different senses, located in different regions of the gray cortex of the brain, and joined together in a unit by a series of association-tracts which pass in the white matter under the cortex. The word "concept" long used by psychologists to denote congeries of mental images making up an idea conveyed by a single word may be adopted by the pathologist to indicate this collection of mental images. To be complete, such a concept must have all its parts intact and the connections between those parts also intact.

If we take such a word as 'book' or 'rose' as an example and watch the process going on in the mind of a child as it acquires a primary knowledge of some particular book or rose, we at once see that this particular concept has a limited extent, consisting of the mental images of the object (1) as seen (form, color), (2) as felt (shape, size, temperature, hardness), (3) as smelt, (4) as tasted, if the object has. odor or taste, (5) as heard, if the object is audible. These five mental pictures comprise the concept of the object, and the separate mental images are associated with all the others, so that when one arises in memory it inevitably recalls all the rest.

If we take as another example the verbs 'to run,' 'to sew,' we call up to the mind memory-pictures of some individual in action or of some act of our own with its attendant sensations; and thus as the basis of any verb, as well as of any noun, we must think of a congeries of mental images closely associated with one another, forming a mental picture.

So far our analysis of the basis of concepts would apply equally well to a child who had not learned to speak, to a deaf-and-dumb person, or to a healthy man. In the man, however, who can talk, there has been added to this original concept a "word-concept" quite similar in its parts to the other and consisting of an image of the word (1) as heard, (2) as seen in print or writing, (3) as pronounced by muscular effort, or (4) as produced in writing by movements of the hand. Each of these separate word-images is joined with the others, and each part of this word-concept is also connected with every part of the mental concept which the word enables us to convey to others. Each one of these various mental images is known to have a separate location in the brain. Thus it becomes evident that the psychological term "concept" (German *Begriff*) stands, not for a simple thing, but for a very complex thing, having as its physical basis an activity not only of widely distant gray cortical areas of the brain, in each of which a separate memory-picture is located, but also of long or short association-tracts, running in every conceivable direction between these various areas. If there is such complexity in the physical basis of so simple a concept as a particular book or rose or as a simple act like running or sewing, it is evident that the physical basis of more complex concepts such as the class books or roses or the complex acts implied by the verbs 'to educate,' 'to civilize,' is very difficult to fathom. It becomes equally evident that for the conduct of thought or the wholesome action of the mind in dealing with concepts, be they simple or complex, an integrity of the entire brain is necessary both in its gray cortical extent and in the white subjacent association-tracts.

Small lesions of the gray cortex in various parts may injure these concepts by depriving them of some constituent mental image, and small lesions in the white matter may disturb the use of these concepts by dissociating images which should be closely bound. Hence it is evident that lesions in various parts of the cerebral hemispheres will produce disturbance in the use of our mental concepts, and this may be manifest either as an inability to recognize by any sense places or objects, the conditions known as mind-blindness, mind-deafness, and mind-paralysis; or as a condition of aphasia, of the form of word-blindness, or word-deafness, or inability to speak or to write.

The localization of the various lesions producing such conditions is a subject apart from our present purpose; it is only to be said that from the condition present a very precise conclusion as to the part of the brain affected can be reached.

But while the various well-marked forms of aphasia indicate large lesions in various situations, many particular cases of aphasia cannot be assigned to any one of these forms, but present very interesting features. When, for example, a person loses the power of reading, yet can copy any word which he sees, and in the act of copying becomes conscious of the meaning of the word which he is writing, being thus able to read by means of his muscular sense, it is evident that he has not lost the visual memory-pictures, but that it has been possible to awaken them in consciousness only in an unusual manner. This may be likened to a break in a railroad which compels a passenger to reach his destination by a roundabout route with one or more changes of cars, instead of by the direct road. It is the study of these partial forms of aphasia which is now exciting interest. It may be stated that they are usually produced by small areas of disease in the subcortical white matter of the brain—in the association-tracts, which cut off the connection between various mental images forming the concept.

Wilbrand has described a most interesting case of mind-blindness with word-blindness in which a small region of disease affecting chiefly the association-tracts in one occipital lobe of the brain was found.

His analysis of the defects of speech and thought produced by the break in these association-tracts is too elaborate to be reproduced here. He shows, however, that the area of the brain concerned in the reception of visual impressions and that in which the visual memories are stored are distinct from one another, but are closely joined by associating fibres. If these fibres are broken, the sight of an object no longer brings up its memory and there is no conscious recognition of a thing when it is seen. But the recollection of an object may occur not only by means of seeing it but also by a train of ideas or by hearing its name. Hence the break in the one association-tract does not necessarily prevent the individual from recollecting the object or calling its memory-picture up to consciousness when it is spoken of or when the train of thought leads to it. On the other hand, a break in the associtionartract joining the visual memory-picture of the object with the memory-picture of its name results in an inability to call to mind the object when its name is heard or to name an object which is recognized when seen.

Bleuler, in a very carefully written article upon a case of amnesic aphasia with word-blindness and mind-blindness, discusses many of the processes of association and their defects. He calls attention to the fact that it is rare for an entire concept to be obliterated from the mind, because its parts, being widely scattered through various regions of the cortex, are not all involved in a single lesion. And it is easy to call up to the mind any concept in various ways, as numerous associations are capable of arousing some one of its parts and thus leading to the whole. On the other hand, it is common to find a single part affected by disease, e.g., names of objects. But this amnesic aphasia has its degrees, from the physiological inability to recall a name, up to complete loss of memory. Even in the latter there is in many cases rather a hindrance in finding the words than a loss of words ; the acoustic word-picture is not lost, for when the word is heard it is recognized. Bleuler asks, Why is the way open from the ear through the word-memory to the concept, when it is closed from the concept to the word-memory ? Why will a man call to mind a person whose name he hears, yet be unable to name a person about whom he has a knowledge perhaps quite complete ? The difference appears to lie in the course taken by the associating impulse : in the one case from a small region of the cortex outward to a general centre or rather collection of centres, in the other case from this collection of centres to a particular area of the cortex. The first he likens to the way taken by a fish down a brook and river into the sea, easy to follow down the current. The second he likens to the difficulty the fish would find in coming from the sea to the particular little brook from which it started. Or, to take another illustration, it is easy to get from a particular town into a neighboring country, but much more difficult to reach that town from any part of the country. If the associations between various parts of the concept are not entirely intact—if the concept is injured—the normal easy flow of associating processes to one focus in the word-hearing memory is interfered with, and there results a difficulty in calling the word to mind. Thus amnesic aphasia may occur when lesions are purely subcortical and the word-memory is actually intact. In daily life the word easily evokes the idea, but ideas are more difficult to state in words. In the first case we go from the particular to the general, in the latter from the general to the particular. The keenest subjective mental analysis therefore is inferior to the study of such cases of disease in throwing light upon the actual physical mechanism of thought.

Wallaschek has shown that disturbances in musical expression and appreciation, quite similar to those in speech and its understanding, have been observed. These may or may not be attended by aphasia. He distinguishes several forms of this condition, which he names amusia, viz.: I. Disturbance in singing: (*a*) motor amusia, in which the person can no longer produce a note or tune; (*b*) sensory amusia or tone-deafness, in which the person cannot recognize various tones or tunes; (*c*) paramusia, in which the effort to sing is unsuccessful because wrong notes are struck; (*d*) musical amnesia, in which a person can sing with or after another but cannot sing alone correctly. Thus a well-known Wagner-opera singer who was suffering from incipient softening of the brain demanded of the manager that some one in the wings should whistle the notes he was to sing, as without help of this kind he could no longer remember the notes or their succession. This occurred prior to any disturbance of speech. Other singers have suddenly lost for a time all memory of music. II. Disturbance in writing music may occur independently of loss of the power of writing words (musical agraphia). One person has been seen who could write music when he had lost the power of writing letters. III. A loss of ability to read music has been observed in one patient (musical alexia). IV. Inability to perform on a musical instrument formerly used has been noticed when other movements were preserved (musical amimia).

Numerous interesting examples of these defects are given in the article. The reviewer can confirm some of its statements. A patient now under his observation with total loss of power of speaking, the understanding of speech being preserved, is being successfully taught to sing in a high pitch words which he cannot be taught to say. In many cases of aphasia the condition of amusia is also present, but their separate occurrence proves conclusively that the two functions are performed by distinct parts of the brain. This is confirmed by the fact that infants may learn to sing a melody before they learn to talk. Wallaschek further calls attention to the numerous association processes which music awakens. Few can hear a melody without keeping time by some movement or by silently humming the tune to themselves. For some the association of music with motion is very close and musical memories may often be of a motor kind. Persons may commit to memory the succession of finger positions on a piano which are necessary to play a piece and be then able to render it when their knowledge of music is too slight to allow them to play by ear. Some can only reproduce a melody of which they know the words, the latter awakening the former ; others, while able to recall the words in the act of singing, cannot do so without the tune. The author attempts to group persons into types—'visuels,' 'auditifs,' and 'moteurs'—after Ribot, in respect to their predominant kind of musical memory. And he intimates that the various theories as to the origin of music, some regarding it as a method of speech, others as a method of dramatic action, others as a method of emotional expression, may be traced to the individual variations of the theorizers. It is possible that the old controversy regarding the 'universal' in logic—which divided the schoolmen into nominalists, conceptualists, and realists—may be similarly solved. The article closes by an attempt to show that a musical person differs from a non-musical person rather in the superior number and activity of his association-processes than in any distinct quality of mind.

It is evident that in the later studies of aphasia the importance of a study of the processes of association is being appreciated.

M. A. S.

HYSTERIA, PARAMNESIA.

On the Psychical Nature of Hysterical Unilateral Amblyopia and Sensitivo-sensorial Hemianæsthesia. Professor BERNHEIM. Brain, Parts LXI. and LXII. 181–190. 1893.

Taking the case of a youth of 19, the upper left half of whose body was completely anæsthetic, Bernheim defends the view that the insensibility in such hysterical cases is not real, but only mental, just like that produced by suggestion in hypnotic subjects. The boy's left eye, used alone, appeared both dim-sighted and color-blind, but varied so from day to day that one was led to believe 'that the imagination played a part in the results." Experiments with a prism and with Snellen's apparatus (red and green letters used as objects and eyes armed with red and green glasses) proved that under these conditions, both eyes being used together, each of them saw its own proper letters distinctly. Moreover, under certain unusual experimental conditions employed for testing the acuteness of sight, the left eye used alone was able to read large print easily at a distance of five metres, but not within that distance. The mind, disturbed by the new conditions, says Prof. Bernheim, omitted to inhibit the sensations received, and the auto-suggestion arose, to see at the distance of five paces. Once surprised into seeing, the eye for a while kept up the habit under these conditions. The patient moreover called hot cold, black white, and *vice versa*—results only to be explained by perverted imagination. He had no resident sensations in his left arm, and with eyes closed could not find his left hand with his right one. The suggestion that the one was a magnet attracting the other enabled them to find each other; but on this being explained aloud to a clinical audience, the effect ceased, and the youth relapsed into the old symptoms. His left ear was stone-deaf, but could be surprised by artifice into hearing. When he discovered the artifice he over-corrected himself and then appeared deaf in his right ear also. All these results, which suggest shamming, and would make most examining physicians unhesitatingly condemn such a patient for an impostor, are explained by Bernheim as effects of 'auto-suggestion,' analogous in all points to the systematized anæsthesias which suggestion produces in hypnotic subjects, and in which it is now well proved that the patient must by one part of his consciousness pick out and recognize the object which it is the duty of the other part of his consciousness to ignore. Bernheim insists that all hysteric anæsthesias are of this type, and that the contraction of the field of view, for example, which they so generally present is a symptom which it is impossible that ignorant patients could deliberately conspire with such unanimity to simulate. Simulation is present, if you like, but it is unwitting and involuntary, and the patient's own consciousness is its first victim.—One is reminded of Mr. Myers's phrase that hysteria is a 'disease of the hypnotic stratum.'

W. J.

Des Paramnésies. A. LALANDE. Revue Philosophique, XXXVI. 485. Nov. 1893.

This name has been given to the very common illusion of feeling as if one had already undergone the experience which may be passing, already been with just these people, in just this place, saying just these things, etc. Usually a strong emotion accompanies the sense of recognition. M. Lalande says that of 100 persons interrogated, 30 know the phenomenon, and that the completeness of detail in the recognition makes it impossible to confound it with a mere judgment of imperfectly discerned resemblance between the present and a real past situation. He also notes, and gives several cases of, the asserted fact that the recognition of the situation sometimes goes so far as to lead to a correct expectation of what the next-following details are to be. The phenomenon is too wide-spread to be considered pathological. Both sexes and all ages and temperaments present it. Passing to its explanation Lalande rejects Wigan's theory of the non-synchronous action of the hemispheres, and Anjel's of a tardy act of *perception*, which, when accomplished, looks back on the just previous *sensation* as if it were another perception remotely past. He himself gives a first explanation similar to Anjel's, by assuming momentary absences of mind with an exaggerated impression of the duration of the lapse, and a return to the object, which thus appears to be seen after a considerable interval and for the second time. But admitting the insufficiency of this theory, he proposes another more original one based on subliminal or 'unconscious' telepathic perception. This perception, if *made conscious* by the succeeding mental state, might give rise to the sense of a previous experience repeated: "Thus, I am walking with a friend, and he thinks of something which he is going to say. A telepathic sensation occurs in me, and I perceive directly the interior thought by which he has thought his phrase. But this perception, to which I am unaccustomed, remains unconscious unless the phrase is actually pronounced. If, on the contrary, he pronounces his phrase, the auditory sensation may awaken in the obscure recesses of my mind the identical perception which I have just received; I shall seem then to recognize it, or rather I shall really recognize it. The only illusion is that of projecting my remembrance into a more or less remote past, in order to account for the confused character which comes only from its origin." This theory, says its author, would also account for the element of prevision. "In any case," he adds, "the key to paramnesia must be sought in the existence of a double perception, unconscious at first, then conscious." W. J.

THE PERCEPTION OF LIGHT AND COLOR.

On a Photometric Method which is independent of Color. OGDEN N. ROOD. Am. Jour. of Science, 3d Ser. XLVI. 173–176. 1893.

Prof. Rood has rendered a very important service to those who are interested in theories of color-vision by devising a simple and certain method for determining the brightness of colored papers. The only methods that have yet been devised consist in simply putting the question to consciousness, does this or that surface seem to be the brighter? Prof. Rood's plan consists in mixing the papers to be compared upon the color-wheel, and observing whether the sensation of flickering is produced or not; that sensation diminishes in intensity the more nearly the papers are alike in brightness, and ceases only when (as tested by the ordinary means) the difference in brightness is reduced to $\frac{1}{50}$ of the total light. To test the method, the brightness-values of six disks (three pairs of complementary colors) were obtained by it; the complementary colors were then mixed in the proportion necessary to produce gray, the brightness of the resulting gray was calculated, and compared with its brightness as observed by the ordinary method. The differences were found to be very slight.

There is this interesting psychological difference between this method and the direct one. When one endeavors to decide by simple inspection which is the brighter of two colors widely different in tone but not in brightness, a disagreeable feeling of uncertainty and embarrassment is experienced (*cf. Physiol. Optik*, p. 428). Helmholtz says: "I must personally repeatedly declare that I can hardly trust myself to form a judgment upon the equality of heterochrome brightnesses. . . . I have always the sense-impression that it is not a question of the comparison of one quantity, but of the combined effect of two,—brightness and color-glow (Farbengluth),—for which I do not know how to form any simple sum, and which I also cannot scientifically define" (p. 440).* Upon Prof. Rood's method, on the other hand, the moment of deciding that the two brightnesses are equal would seem to be a moment of certainty and agreeableness,—"no shock is experienced and the colors are seen to mingle in a soft streaky way."—As regards the accuracy of the determination by the old method,

* It will be noticed that this is a most important statement, on the part of Helmholtz, for those who believe that what we speak of as the brightness of a color is, in fact, a sort of combined voluminousness of sensation due in part to amount of specific color-process and in part to amount of accompanying white-process.

Brodhun (who is green-blind, and skilled in such observations) found his mean error to be:

For comparison of red with red	....	3%
" " " blue with blue	....	3.8%
" " " red with blue	....	5.8%

(Since Brodhun and Ritter [red-blind] showed superior facility in these comparisons, it was at first supposed that that might be due to their color-blindness, but they have both since been somewhat surpassed by Frl. Elise Köttgen [*Helmholtz Festgruss*, p. 337]). It will be interesting to know how great is the mean error by the method of Prof. Rood. As regards the least difference perceptible by flickering between like colors (grays), it is very exactly the same as that found by König and Brodhun, for all colors and for white, at moderate intensities, but it would appear that, by the flicker-method, this is a difference which can be detected without trouble by untrained observers, and, what is most important, it would seem to be as readily observed between unlike colors as between like ones.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

1. *A New Theory of Light Sensation.* CHRISTINE LADD FRANKLIN. Proceedings of the International Congress of Experimental Psychology, 103–108. London, 1892. Also reprinted in the Johns Hopkins University Circulars, XII. 108–110 (June 1893), and in Science, XXII. 18, 19 (July 14, 1893).
2. *Eine neue Theorie der Lichtempfindungen.* Same author. Zeitschrift für Psychologie, IV. 211–221. 1892.
3. *Theory of Color Sensation.* Same author. Science, XXII. 80, 81. 1893.
4. *Color Vision.* Same author. Science, XXII. 135. 1893.
5. *On Theories of Light Sensation.* Same author. Mind, New Series, II. 473–489. 1893.

Workers on the psycho-physiology of vision are indebted to Mrs. Franklin for the clear light in which she has set their problem. The second and fifth of her papers are the most important, the first being an abstract of matter more fully presented in the second, the third a reply to a criticism, and the fourth a notice of priority. A statement of the shortcomings of current theories naturally paves the way for the proposal of a new one. The theory of Helmholtz is at fault in that it disregards sensation in making white a mixture. While a combination of red, green, and blue lights does appear white, it is illegitimate to infer that the white sensation is a compound of the red, green, and blue sensations. The theory is strained and artificial in its account of total color-blindness. Its explanation of complementary colored after-images with closed eyes by the 'self-light' of the retina is inadequate. Its explanation of simultaneous contrast has been proved to be wrong. Against Hering it is urged that his conception of reciprocal processes in the retina, both of which are attended by sensation (yellow, for example, being caused by a katabolic change and blue by an anabolic change in a single photochemical substance), is wholly unparalleled in the physiology of other sense-organs, sensation, so far as known, attending katabolism only; further, that the impossibility of color-intensity apart from color-whiteness (saturation) to which his theory leads is something not readily conceivable; that his explanation of simultaneous contrast is a mere translation of the facts into the terms of his theory without much demonstrative weight. Against this theory also the author brings the experimental objection that a gray resulting from complementary red and green and an exactly similar gray from complementary blue and yellow do not remain exactly matched when the total illumination is varied, as they must do on Hering's theory. The experiment was made, however, with the color-top and can hardly be regarded as conclusive. Hering himself denies the conclusiveness of similar experiments with spectral colors, unless made under special conditions, and asserts that when the experiment is properly made the matched grays remain matched at all intensities (*Pflüger's Archiv*, LIV. 299 ff., 1893). The theory of Donders is briefly criticised in the first paper above, and a criticism of Ebbinghaus' new theory is promised in the next number of *Mind.*

The author's theory, which is regarded, indeed, rather as an indication of the direction that such a theory should take than as a developed scheme, is in outline as follows. There are in the eye two sorts of photochemical substance: one, decomposed by all kinds of light, which gives, by the action of its decomposition-product on the nervous organs of the retina, the sensations of the black-gray-white series; the other, decomposed in particular ways by red, green, and blue lights, which gives these colors and their mixtures. When all three of these decompositions take place at once, the decomposition-product is the same as that resulting from the breaking up of the black-gray-white substance and accordingly gives the same sensation. Complementary pairs of colors give white because they cause all three of the decompositions at once; red and blue-green, for example, give white because red causes the partial decomposition corresponding to that color, and blue-green the decomposition corresponding to the other two primary colors. The theory without great difficulty also accounts for the cases in which colors appear colorless,—e.g., at the extremes of intensity, when the retinal area is very small, when the eye is color-blind, etc.,—but on these the author lays little stress. The account that the theory gives of complementary colored after-images with closed eyes and of simultaneous contrast is regarded as following so unavoidably from the theory as to be a justification of it. When the eye is stimulated for a time with light of a given color (e.g., red) the corresponding decomposition takes place and the decomposition-product is used up in causing the red sensation. The molecules thus partially decomposed are in an extremely unstable condition, and in going to pieces set free the decomposition-products that give blue and green, whence the blue-green after-image. In simultaneous contrast, where, for example, a red field induces a tinge of blue-green upon an adjacent gray field, the effect on the color-molecules is the same as that just mentioned, but in this case the mutilated molecules (or their decomposition-product) are carried out over the retinal area corresponding to the gray field by the rapid retinal circulation. The theory can easily be made to fit also, with the distribution of the rods and cones, the saturation differences of spectral colors, the retinal areas sensitive to different colors, and the unmixed appearance of yellow.

Difficulties and objections naturally suggest themselves with every new theory. What, for example, is this theory to do with black, especially with black in simultaneous contrast? And granting that the retinal circulation is rapid enough for the use made of it in explaining simultaneous contrasts (which seems doubtful to the reviewer), how is the reversal of colors which is found in the after-image of the contrasting fields to be accounted for? Finally, as the author herself observes, if Hillebrand's demonstration of Hering's theories of complementary colors stands, it is fatal to her theory, at least in its present form.

Some of the advantages of the theory can be inferred from what has already been said. It has three primary colors, an independent white, and need make no unusual physiological assumptions. Its chief advantage, perhaps, and one that it shares with the theory of Donders, is its use of partial decompositions in the photochemical substance.

EDMUND C. SANFORD.

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THE MUSCULAR SENSE.

Über den Muskelsinn bei Blinden. PAUL HOCHEISEN. Zeitschrift f. Psychol., V. 239–282. 1893.

The author shares the views of Goldscheider, under whose direction the experiments were made, as to the peripheral sources of the muscle-sense. Since the threshold of excursion for just perceptible active movements hardly differs from that for passive, we can use the threshold-values found for passive movements as measure for sensitiveness to active movements, hence, together with the extensive discriminative sensibility of the skin, as measure for the fineness of muscle-sense in blind persons.

Experiments were carried out on Goldscheider's apparatus for measuring movements. Three joints of each hand were investigated, both as to extension and as to contraction. An average of 100–150 trials was made for each determination, a total of more than 6500 experiments for determining movement-thresholds in the blind, with about 3000 more on seeing persons for comparison. Each determination of the skin-sensibility in different parts was the result of 50 trials with varying distance of the points. The conclusions arrived at were as follows:

It makes no difference for the fineness of sensation whether an individual was born blind or became so later. Blind persons who have cultivated the sense of touch show an objectively provable refinement of sensibility to passive movements, and thus of the muscle-sense; this refinement is, however, not very great and not present in all blind persons. The cause of the refinement is a psychical one, due to the fact that, through improvement of the attention and through practice in interpreting sensory signs, sensations of intensity usually indistinguishable are raised above the threshold. Children have a finer sensibility for movements than adults. Little difference can be found between right and left sides in case of movement-sensations. The discriminative sensibility of the skin is increased in slight degree, cannot always be clearly proved, and is due to practice.

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EXPERIMENTAL.

Minor Studies from the Psychological Laboratory of Clark University, I. Am. Jour. Psychology, v. 294–389. Apr. 1893.

1. THADDEUS L. BOLTON : *On the Discrimination of Groups of Rapid Clicks.*
2. MARY WHITON CALKINS : *Statistics of Dreams.*
3. F. B. DRESSLAR : *On the Pressure-sense of the Drum of the Ear and 'Facial Vision.'*
4. J. F. REIGART and E. C. SANFORD : *On Reaction-times when the Stimulus is applied to the Reacting-hand.*
5. JOHN A. BERGSTRÖM : *Experiments upon Physiological Memory by means of the Interference of Associations.*
6. JAMES H. LEUBA : *A New Instrument for Weber's Law; with Indications of a Law of Sense-memory.*
7. EDMUND C. SANFORD : *A New Pendulum Chronograph.*

Dr. Sanford explains in a brief preface that these studies were planned and carried out primarily as practice in research by his students. The studies are not for that reason either unimportant or untrustworthy, for the contributors have had good training, some of them are of independent strength, and all of them have enjoyed the intimate co-operation of Dr. Sanford. The title 'Minor Studies' is a very modest one, for it would be easy to find Doctors' Theses and other pretentious articles less significant and less thorough than some of these. For one thing, if for no other, these studies must receive the highest praise. One cannot find a line of verbal padding. Essential psychological explanations are put as concisely as are the statements of fact. This is a thing as excellent as it is rare.

1. The chief specific question considered in Mr. Bolton's paper is: "Can the presence of one click more or one click less be recognized when successive groups of different numbers of clicks are given, and under what conditions as to rate and number?"

The results with three subjects show that such differences can be recognized in a majority of the observations (57%–95%),—with two of these subjects in more than 75% of the observations,—provided that the rate be not more than about 133 per second for groups of 10 or more clicks, and not more than 153 per second for groups of 9 or less clicks. The fact is observed that the errors of judgment are fewer when the standard number of clicks is compared with a smaller group than when the standard is compared with a larger group. An attempt to measure the difference between the two cases showed that an increase of two clicks is recognized more accurately than is a decrease of one click. For possible explanation of the difference in the two cases the following is proposed: "Our memory-image of the first actually increases in length between the first and second set of clicks. When the lesser group follows the standard, the second seems decidedly less than the first; but when the same or a greater number follows, the difference is not so great and more difficulty in discriminating is experienced." For explanation of the lengthening of the memory-image see, below, Mr. Leuba's 'Indications of a Law of Sense-memory.'

It is thought improbable "that any clicks are lost in the perception of a rapid group, at least up to 153 per second," since groups of 8 and 9 are distinguished at that rate. It is also thought improbable that the number of clicks in the group affects the impression of discreteness. The paper concludes with a brief statement by Dr. Sanford of the work of Dietze, Hall and Jastrow, and F. Schumann in the same field.

2. The 'Statistics of Dreams' is based upon "the accurate record of two people, from notes made by themselves during the night and supplemented by careful study and recollection on the following day." The observers were S., a man of thirty-two (taking records 46 nights, 170 dreams); and C., the writer, a woman of twenty-eight (taking records 55 nights, 205 dreams). A study of the results shows the following main results:

"Most of our dreams occur during the light morning sleep," but "the sleep of the middle of the night is in no sense a dreamless sleep." In a majority of the dreams, some connection with waking life could be traced, but "the influence of the time of the dream upon the degree to which it is associated with the waking experience" could not be accurately calculated. An attempted classification of the dreams according to their vividness showed that vivid dreams occur most frequently, but not always, in the morning. A small per cent of the dreams were classified as presentative,—visual, auditory, tactual, temperature, gustatory, and organic sensations appearing in the list, auditory sensations most frequently. The influence of actually present stimuli in occasioning such dreams is pointed out. The so-called 'representation dreams' were classified with respect to their representation elements. In this table visual elements greatly predominate. Word-dreams (spoken or heard) are very frequent, especially with the observer C., who reports dreams in five languages.

The connection between waking and dream life was studied by noting the presence and frequency in the dreams of perception, accurate and illusory memory, imagination, thought (contrary to Spitta), emotions of many sorts, and volition involving deliberation and decision (contrary to a common opinion). There is noted further the presence in the dreams of persons, places, and other things from waking experience, together with the number and character of these. It is observed that many but not all (contrary to Délage) of these are of trivial importance. The connection of the dream with the waking life is put thus: "The dream will reproduce in general the persons, places, and events of recent sense-perception or of very vivid imagination—not the objects of ordinary imagination, of thought, of emotion, or of will, so far as these are not also perceived objects. Furthermore, thoughts, emotions, experiences, and personal relations that mean most to us are generally extremely complex and depend for their reproduction on the integrity of very many lines of association. When a number of these are put temporarily out of function by sleep, it is next to impossible to bring about these complicated mental states, though less complicated ones can be reproduced with tolerable completeness."

Differences between waking and dream life are pointed out,—as the isolation of the dreamer, partial or complete loss of personal identity, and various types of paramnesia, leading to various degrees of absurdity in the dream.

The writer questions the popular belief as to the swiftness of dreams, explaining the facts as resulting from a quickening of the memory-time, which makes what was dreamed in a long period seem to have been dreamed in a short one. This explanation does not, however, account for all such dreams, for in some cases the total time of the sleep is reported very short. The paper concludes with a brief consideration of prevision in dreams, with suggestions in explanation. The paper is by no means the statistical skeleton which its title and this statement of its contents might indicate, but is mature prose and good psychology. Even if it were not so, the collection of facts is very important. It is only to be wished that we had innumerable other collections as good.

3. Mr. Dresslar notes Prof. James' remark (*Princ. of Psychol.*, II. 140) that "the tympanic membrane is able to render sensible differences in pressure of the external atmosphere too slight to be felt as sound." Mr. Dresslar devised a simple apparatus for applying a measured pressure to the ear-drum and proved that "it is a mistake to ascribe to the tympanum a very great delicacy in the perception of pressure," and that accordingly Prof. James' explanation of the 'shut in' feeling sometimes called 'facial vision' is erroneous. Careful tests with three subjects verify the result that blindfolded subjects can distinguish in a majority of cases whether a frame brought near the side of

the face is open, solid, lattice, or wire. Temperature sensations were shown not to furnish the basis of judgment and the conclusion was reached that, after all, the basis of judgment is furnished by auditory sensations.

When a partial report of the foregoing result was made at the Philadelphia Meeting of the Am. Psychol. Assoc., Professor Ladd made the point that this is one of many cases of a kind of tact,—where we have fairly reliable knowledge without at all knowing through what means we obtain it.

4. The statement of Exner (Pflüger's Archiv, VII) that, contrary to his expectation, the reaction-time for electrical stimulation is longer by about 10σ when the stimulus is received by the reacting hand than when it is received by the other hand, was tested by careful repetitions of the experiment. The results show a slightly greater difference in the contrary direction.

5. Mr. Bergström took blank playing-cards, had abstract words or pictures of common objects printed upon these and so made up packs of eighty cards, ten kinds of cards to the pack. Having shuffled the cards so that two of a kind would not come together he directed his subject to sort the cards as rapidly as possible into ten piles, each pile to contain the eight cards of a given kind, and the order of the piles to be determined by a plan (see original) which prevented the subject from knowing the order in advance.

In the first set of experiments abstract words were used; two packs of eighty cards each, and alike in all respects except in the order of the cards, were placed before the subject. He was required to sort first the one and then the other pack, the order of the piles being in the two cases different and in both cases determined by the plan as aforesaid.

The results for five subjects show that the sorting of the first pack required from 122″ to 159″, while the sorting of the second pack required from 3.5″ to 18″ more time. That this increase of time was not due to fatigue is proved by the fact that when different words were used in the second pack, there was practically no increase of time.

To simplify the conditions, pictures of common objects were substituted for words. One pack was sorted and then after an interval of 3″, 15″, 30″, 60″, 120″, 240″, 480″, or 960″, a second pack was sorted in entirely different positions. Five subjects were tested, each at a given hour on successive (12 to 21) days. See the original for precautionary details. The results show that at each of the intervals the sorting of the second pack requires more time. The curve of recovery as the interval becomes longer is determined. It was again proved that fatigue is not the cause of the retardation. Mr. Bergström believes the cause to be interference of associations and that his method is 'a laboratory method of studying habits.' The paper is short and the apparatus is simple and inexpensive ; but the study deals with one of the most important problems of physiological psychology and does so with very considerable precision and success.

6. Mr. Leuba describes an apparatus devised by Dr. Sanford and modified by himself for testing Weber's law with lights, by the method of equal interval. For a detailed description of the apparatus (whose essential parts are a blackened tube and an episkotister) see the original. In using an apparatus copied after that of Dr. Sanford the reviewer found that the time during which the eye was directed into the dark tube made significant differences in the observation of faint lights. Whether this point was regarded in Mr. Leuba's work does not appear. The results obtained by him "do not show the uniformity of ratio required" by Weber's law, but "the deviations are not extremely great" and the series of magnitudes is much more nearly a geometrical series than an arithmetical. Mr. Leuba compares a series of experiments in which the standard lights had to be held in memory with a series in which the standards were visible all the time, and finds "a striking difference in the ratios at the lower end of the scale." He explains this outcome as follows: "The image of a recent sensation tends to recall, by association, the united residual of all the past sensations of the same kind, and in so doing passes over in some degree to this sub-conscious resultant impression." Hence "there seems to be a natural tendency in us to shift the sensation held in memory towards the middle of the scale of intensities." This the author thinks to indicate a 'law of sense-memory,' which falls in general line with results and theories in other fields of psychology.

7. Dr. Sanford's Pendulum Chronograph is simple and inexpensive, and under favorable treatment the gross error is reported not greater than 3σ or 4σ for short intervals and one part in fifty for times of a second or longer, "a degree of accuracy that is sufficient for all practice work in psychological time-measurements and for many kinds of research." For details of construction see the original.

W. L. Bryan.

University of Indiana.

Zur Physiologie u. Pathologie des Lesens. By Dr. GOLDSCHEIDER and R. F. MÜLLER. Ztschrft. f. klin. Medicin, XXIII. 1, 1893.

Recherches sur la Succession des Phénomènes Psychologiques. B. BOURDON. Rev. Philos., XVIII. 225–260. 1893.

Goldscheider and Müller have made an important series of experiments to determine whether, in reading, we 'spell out' letter by letter, or grasp several letters at once. By means of a slit in a rapidly rotating disk a number of symbols or letters could be exposed to view for a short but accurately measurable time. An exposure of .01 second (slightly above the lower limit of visibility) was used; more could be distinguished with this than with longer exposures. The symbols used were lines, arcs, squares, etc., very heavily printed. Any arrangement of three different, or four similar, symbols was generally reproduced correctly at once. More trials were required as this number was increased, except when the arrangement was symmetrical or suggested some well-known figure. Similarly, with numerals or meaningless combinations of letters, four were usually seen at first, five after a second trial, etc. *Words of five letters were generally read at once*, but with longer words the results varied considerably: mistakes were often made and persisted in, rendering repeated trials necessary. Three words of four letters were placed one above the other and exposed for .03 second; between eight and ten letters were recognized at the first trial. Any well-known phrase of three words could be read at once, but in this case the omission or misprinting of certain letters passed unnoticed. The writers draw a distinction between 'determining' and 'indifferent' letters of words. "The initial letter is almost always one of the determining letters." A vowel, when not forming a syllable by itself, is generally less important than a consonant, e.g., 'Kl ngb ld' was read as 'Klangbild,' but ' lan bild' was not completed. [The writers are in error when they speak of "recognizing" (erkennen) 'Charité' from 'Ch té,' 'Object' from ' bj t,' etc., for these might be completed in other ways; e.g., 'Subject'.]

The results show that "the time of recognition is dependent on the number of letters and the degree of familiarity" of the word. Except with unfamiliar words, we do not 'spell' in ordinary reading, but observe certain letters, or parts of letters, here and there, and fill in the rest by association—generally of the word-sound, but often of the word-picture. This renders improbable the existence of any 'memory-cells' for calling up memory-pictures by association with apperceived elements, for the memory-picture must be called up before apperception occurs, if it is to hasten and extend the apperception, as it does. The writers remark that in speech, unlike reading, there is always a 'spelling out of words.' But may not the muscular co-ordinations for speech proceed by syllables, or even phrases, as well as by single letters?

Bourdon uttered single words (or letters) at intervals of about four seconds; the subjects were to write down each time the first thing occurring to them. 1. A letter or simple sound (a, b, ch, gn, etc.) was given, with which a word was to be associated. Compared with the chance resemblance (calculated) of any word-pairs, the pairs thus obtained showed considerable phonetic and remarkable syllabic resemblance. 2. When a letter was required as 'answer,' alphabetic contiguity gave the association as often as phonetic resemblance. 3. If a color was required, the pair (letter and color-name) usually showed phonetic resemblance. The writer also claims association between 'open' vowels and bright colors, between 'dull' sounds and dark colors, etc. 4. A word was given, another word 'answered.' Here the meaning generally determined the association, the phonetic resemblance bearing about the random average, with important individual variations.

Distinguishing between names of qualities, objects and acts, the two latter were generally associated each with a word of its own class, the first about equally with qualities and objects. In general the two phenomena were co-ordinate, although with objects the second was often subordinate. They were rarely heterogeneous, but exhibited difference oftener than likeness (e.g., red and blue). Combining all these categories, the answers of some persons come under fewer headings than those of others, though greater in number; this indicates a more logical mind. To prove this, the writer analyses several passages typical of the literary and scientific styles.

HOWARD C. WARREN.

PRINCETON.

EPISTEMOLOGICAL.

Idealism and Epistemology. H. JONES. Mind, N. S. No. 7, 289–306, and No. 8, 457–472. July and Oct. 1893.

Metaphysic and Psychology. JOHN WATSON. Phil. Rev., vol. II. 513–528. Sept. 1893.

The Meaning of Truth and Error. DICKINSON S. MILLER. Phil. Rev., vol. II. 408–425. July 1893.

Old and New in Philosophic Method. HENRY CALDERWOOD. Phil. Rev., vol. II. 641–651. Nov. 1893.

The first of Prof. Jones' two papers may be called a defence of Neo-Hegelian doctrine, and the second an attack upon the attempt to obtain Reality by inference from subjective states. The charge made against Hegelians and Neo-Hegelians, to wit, that they confound their thoughts of things with things, the Logic of Hegel with the system of the universe, is answered by the statement that their Epistemology is not at fault, for they have none, not recognizing two worlds to be brought together, but accepting ideas and things as manifestations of the one Reality and belonging to the same world. Yet Prof. Jones, while unwilling to call ideas 'existential realities,' admits that they have existence, and distinguishes between them as mere occurrences in consciousness and as having 'objective reference.' His position reminds one of Hamilton's distinction between the 'facts' of consciousness and the 'testimony' of the facts, and his opponent may still object that he appears to confound the idea and its 'reference' with the thing to which reference is made, assuming the latter given when the former is given. The second paper is largely an answer to Prof. Seth's article in the *Philosophical Review* (vol. I. pp. 504–517). The critical parts are more satisfactory than the constructive.

Prof. Watson's article is also written from the Neo-Hegelian point of view. It is a critique of Prof. Andrew Seth's position regarding the subject-matter of psychology, epistemology, and metaphysic, which allots to them respectively the self, the world, and God. Dr. Watson objects to regarding the world as known by inference from subjective states, and claims that one who does not start with 'reality' can never end with it. Mr. Seth's distinctions between psychology, epistemology, and metaphysic 'vanish away' when we bear in mind that "there is no consciousness of self apart from the consciousness of other selves and things, and no consciousness of the world apart from the consciousness of the single reality presupposed in both."

Mr. Miller's paper on the 'Meaning of Truth and Error' develops the consequences of the assumption that an idea can in no sense be said to reach out beyond itself and know an object beyond itself, whether this object be an external material thing or a past or future idea. It follows that truth and error must be found within the idea itself, must be gotten out of it by an analysis, for the mind is wholly shut up to its actual content at a single moment, the present. This doctrine, he maintains, is in no sense sceptical, permitting one to believe in an external world, a past and a future—it analyzes, but does not deny. The chief criticism to be made upon the article is that it does not distinguish between the psychological and the epistemological points of view, and even one who accepted its first assumption might desire to have some of its assertions pretty carefully restated.

Prof. Calderwood's paper is taken up with the problem of the significance for psychology and philosophy of the experimental and physiological psychology. He points out with justice that there must always be ultimate reference to introspection whatever method be employed, and he also calls attention to the fact that the results so far obtained cover rather a narrow field, stating that they "do not constitute a psychology, or doctrine of the soul. At best, they supply a view of the soul's relations with its environment, more especially with its own organism, as the vehicle of sensibility and of motion. But mental action is not included. The further we advance into the knowledge of mind, the further we travel from the borderland on which the results are obtained." Dr. Calderwood concludes that experimental psychology can contribute little to a knowledge of the more complex mental phenomena, and that men must come and go by 'the old pathways of introspection' as before. One cannot object to the statement that ultimate recourse must always be had to introspection, in some sense of that term, but God forbid that we always follow the old pathways of introspection as they have been followed in the past. And one cannot object to Dr. Calderwood's statement of the present limitations of experimental research, but it is still too early to prophesy what may or may not lie outside its territory in the future. The tone of Dr. Calderwood's discussion and his estimate of the outlook for experimental work are evidently determined by his view of the soul and its environment. Were he an adherent of, say, the doctrine of a parallelism of cerebral states and mental states, it is probable that he would write differently on this question. G. S. F.

ETHICAL.

On Certain Psychological Aspects of Moral Training. J. ROYCE. Intern. Journal of Ethics, III. 413–436. July 1893.

Moral Deficiencies as determining Intellectual Functions. G. SIMMEL. Journal of Ethics, III. 490–507. July 1893.

The Knowledge of Good and Evil. J. ROYCE. Intern. Journal of Ethics. Oct. 1893.

The problem proposed by Dr. Royce is to discover the mental factors, or the psychological side, of the fact ethically called conscience; to show, so far as psychology is concerned, why we have a sense that conscience is immutable and authoritative, while, at the same time, it is historically fallible and variable. The judgments of conscience reduce themselves to two types: one advises sympathy, devotion to a will beyond one's own; the other, justice, reasonableness, the regulation of life according to a consistent plan. Morality is the complete union of these two principles, and comes into play when these motives conflict either with opposing forces or with one another. Conscience, as thus defined, is our awareness of certain fundamental psychological tendencies — the tendency to imitate, leading to sympathy, and the tendency to form fixed habits, leading to regularity and consistency of conduct. The authoritative nature of conscience, its innate character, is due to the radical nature of these instincts. Its fallibility, its origin in experience, is due to conflict between these tendencies. Our imitativeness, our social suggestibility, act immediately, giving us generous impulses, but always tend to confuse our general plans. Our fixed habits, on the contrary, lend themselves to generality, but tend to become so fixed as to make us unmindful of the calls of sympathy.

The moral bearings of the discussion lie, of course, outside our scope. The psychological identification of imitation with immediacy of action, and of habits with reasonableness of action, seem to me, however, very questionable. Habit, as such, (apart from a need of changing it,) is, upon the whole, opposed to conscious reflection: and one could make out a very fair case for the hypothesis that imitation is one form of the law of habit, instead of a principle opposed to it. Dr. Royce, to be sure, makes much of the element of conflict, but only as affecting the ethical value, not as having an intrinsic psychological significance.

Simmel's extremely acute essay falls within our range so far only as it deals with the psychological and anthropological relation existing between knowledge and intellectual acuteness on one side, and activity denominated moral on the other. It is noteworthy for maintaining the anti-Socratic paradox that immorality is important to intellectual development in certain directions. It is a fundamental principle of modern psychology that an idea of an act is the first inclination to its execution; that, indeed, there is an organic psychophysical connection between a conception of an act and its performance. This granted, it follows that complete recognition of the act can be had only by following the idea to its consummation—that the act *is* the idea consummated. Hence immoral acts are the condition of our comprehension of immorality; the reproduction of the evil passion the only way to know it. This general principle is reinforced by considerations from criminal anthropology—the immoral man swims against the stream, and, hence, requires more strength, acuteness, etc., to succeed. Through lying, the mind grows wary, comprehensive, delicate and strong, acquires a good memory, quickness of invention, power of imagination, etc. A third psychological connection is found in the relation of the emotions to knowledge. Morality requires interest, sympathy with subject-matter; science requires indifference, approaching hard-heartedness—e.g., vivisection. Development of æsthetic power, regard for the picturesque, etc., requires also quiescence of altruistic emotions.

Dr. Royce's second article (considering the same problem as the foregoing, and, in part, a reply to it) attempts to reduce the apparent conflict between the demands of morality and those of intellectual progress to one special case of a more general law, psychological and even biological in character. This law is that every organic process is the combination in harmony of opposing tendencies: living tissue involves, as part of its own activity, phenomena which by themselves would mean death; every voluntary movement, action on the part of antagonist muscles; every nervous stimulation a corresponding inhibition; every virtuous act a known tendency to evil. In each case, the organic activity involves the reduction of an opposing tendency to a contributing factor in the activity itself. Applying this to the paradox of Simmel, it follows (1) that knowledge of evil does not require the actual evil-doing, but simply the presence of an evil tendency; and (2) that there is deep insight into the nature of the evil deed only so far as it is transcended; this being, apparently, a special case of the general law that we do not truly know any activity as long as we remain in it, but only when, by getting beyond it, we are able to turn back on it as an 'object' of reflection.* The same principle is involved in the fact that the attainment of virtue involves a constant approach to a condition where evil motives have no force and are ignored. Here the question is as to the relation of habit and consciousness. We are completely conscious only when the function concerned is learning, only when it is novel; mastered, or become habitual, it passes into unconsciousness. We cannot affirm from this, however, that consciousness is aiming at its own absolute extinction, for this unconscious function is the instrument for mastering wider situations and thus subserves a wider consciousness.

Dr. Royce would have made his case still stronger, psychologically, it seems to me, if he had not admitted that the evil tendency is evil *per se*. It is difficult to see how any organic process can be bad in and of itself. It is *in* entering into a larger activity, of which, therefore, it must become an inhibited factor, that it becomes bad as it would be in itself. Instead, then, of saying with Simmel that only the bad man can know evil, it is a psychological necessity to say that only the good man can know it—know it, that is, as evil. J. D.

* I say apparently, because, while this is implied, I do not quite know whether Dr. Royce expressly means it.—J. D.

NEW BOOKS.

Grundzüge der physiologischen Psychologie. WILHELM WUNDT. Vierte umgearbeitete Auflage. Engelmann, Leipzig, 1893. Pp. xv, 600, and xii, 684. M. 12 and 10. (The third edition is here enlarged by 181 pages.)

Grundriss der Psychologie auf experimenteller Grundlage dargestellt. OSWALD KÜLPE. Engelmann, Leipzig, 1893. M. 9.

Psychologie des Erkennens vom empirischen Standpunkte. GOSWIN K. UPHUES. Erster Band. Engelmann, Leipzig, 1893. M. 6.

L'Evolution Intellectuelle et Morale de l'Enfant. G. COMPAYRÉ. Hachette, Paris; 1893. Pp. xxiv, 371.

The Psychology of Childhood. F. TRACY. Heath, Boston, 1893. Pp. 94.

Appearance and Reality. A Metaphysical Essay. F. H. BRADLEY. (Library of Philosophy.) Swan Sonnenschein, London; Macmillan & Co., New York; 1893. Pp. xxiv, 558.

NOTES.

Prof. STUMPF has accepted a call to the Berlin chair of Philosophy, which will be vacated by Prof. Zeller on attaining his eightieth birthday.

According to *Nature*, Drs. G. DWELSHAUVERS and P. STROOBANT will have charge of a laboratory of 'Psychological Physics' in the University of Brussels; and Dr. W. H. RIVERS, of St. John's College, will conduct practical work in the Psycho-physical laboratory at Cambridge.

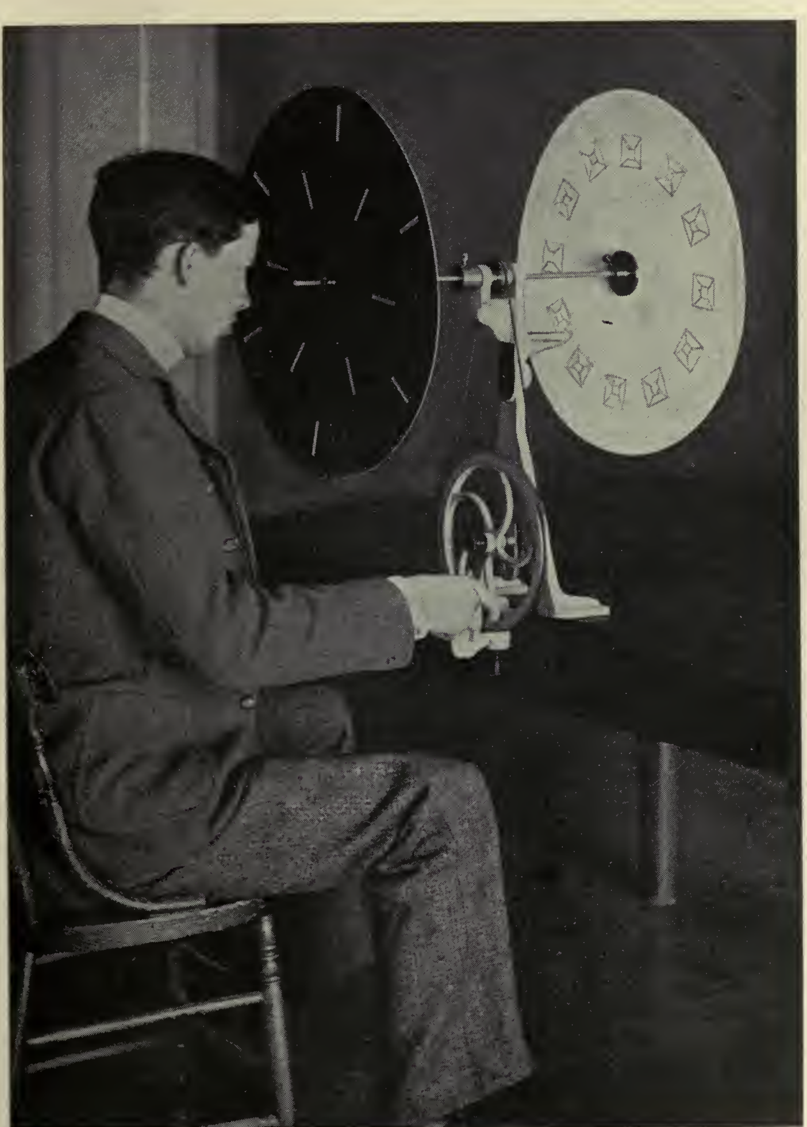
Prof. ALEXANDER, who takes the chair in Owens College, Manchester, vacated by the remova lof Prof. ADAMSON to Aberdeen, will offer opportunity for work in Experimental Psychology.

A. KIRSCHMANN, Ph.D., and F. TRACY, Ph.D., have been appointed Lecturers in Philosophy in the University of Toronto. Dr. Kirschmann will have charge of the Psychology, and will direct the laboratory.

JAMES R. ANGELL, A.M., has been appointed Instructor in Philosophy in the University of Minnesota, and offers laboratory courses in Experimental Psychology.

HOWARD C. WARREN, A.M., has been appointed Instructor in Experimental Psychology in Princeton College.

LIVINGSTON FARRAND, A.M., M.D., has been appointed Instructor in Physiological Psychology in Columbia College.



Vol. I. No. 2. March, 1894.

The Psychological Review.

THE PSYCHOLOGICAL STANDPOINT.

BY PROFESSOR GEORGE STUART FULLERTON,

University of Pennsylvania.

I.

The plain man who is innocent of special psychological culture, and has only the knowledge of his own mind and of other minds which is forced upon him by experience and the unavoidable half-conscious reflection upon experience to which one is led by every-day life, is still pretty well provided with psychological knowledge of a certain sort, and he uses with some skill the psychological methods in vogue in the schools. His thinking is by no means clear, it is sometimes quite inconsistent, it is limited to a narrow field. Still, I should hold it to be, in embryo, psychology as natural science, for it is psychology from the standpoint of the common understanding, and in thinking about minds and their relations to things the plain man thinks very much as he does when he is concerned with material objects. He believes that he has a mind, though he has no very distinct notion of what it is. He believes that this mind is intimately related to his body, which is a thing outside of his mind. He believes that through this body it is related to an external real world, from which it receives influences and which it can influence in return. In this external world he thinks he finds other bodies more or less like his own, and believes that there are, connected with them, other minds, as his mind is connected with his body. Further, he believes that, as he can express by actions of his body ideas or emotions in his mind, so the minds connected with other bodies can give expression to their ideas or emotions, and such an expression is to him the revelation of the contents of these other minds. He thus compares the mental states of other men with his own, and forms some general notions of the contents of minds and the ways in which they act, thus arriving at the beginning of a mental science. He may have done all this without even having heard of the science of psychology.

The handbooks of psychology describe at length the stages by which our plain man arrives at the knowledge of minds which he possesses, and describe it much better than the possessor himself could do. His thinking has been half-conscious and unanalytic; he has rather felt his way than seen it. And yet we may note that the psychologist, in obtaining the knowledge which has made it possible for him to write such a description, has followed no method not already applied by the unscientific subject of his description. Our subject has had recourse to introspection; he has made use of the objective method; he has not confined himself wholly to passive observation, but has sometimes experimented. Experience of his own conscious states has given him the key which is to make significant the expressions and actions of other men and of brutes. He has certainly observed these expressions and actions, and framed a more general notion of mind than he could have done by a mere examination of his own mental processes. And every time he has sought by persuasion or by any other means to produce a given mental state in another he has employed experiment, as does the *galopin* who rides on the back-platform of the bob-tailed car, at a personal inconvenience to himself, with the avowed purpose of getting the driver 'wild.' Of course, such introspection as we are discussing is blind and instinctive, such observation is loose and inaccurate, such experiment is undertaken for no scientific purpose, and sins in all sorts of ways against the canons of experimental investigation. Nevertheless, they remain introspection, observation, and experiment. The difference between the plain man and the psychologist does not lie in the fact that the latter uses any method peculiar to himself, esoteric and above the comprehension of the unlearned. It is simply a case of the difference everywhere found between the scientific and the unscientific, the man who applies methods carefully and seeks accurate and exhaustive knowledge of a subject, and the man who feels his way blindly, going only so far as he is impelled to go by immediate practical needs. The knowledge of mind gained by the plain man is loose and vague, more or less inconsistent, and very limited in extent. Yet it is, as far as it goes, a knowledge of mind, and does not differ in kind from that of the psychologist.

II.

Now that psychology is emerging gradually from that ill-defined medley which has passed by the name of philosophy, and is taking its place as a distinct discipline, it is coming, I think, to be generally accepted that the psychologist must occupy much the same standpoint as the ordinary man. I do not, of course, mean that he must be as loose and careless in his thinking, but that he must be scientific rather than metaphysical, accepting without question the assumptions upon which the natural sciences rest, and investigating the phenomena of mind much as they investigate material phenomena. The plain man becomes more careful and accurate when he becomes scientific, but he does not change his whole point of view. He is still in a real world of things about which he reasons as he did before. As a plain man he knew something about plants; as a botanist he knows much more, but he has passed through no intellectual revolution in becoming a botanist. There in the real world are real plants which he may examine, and upon which he may try experiments. The results obtained by the botanist are sufficiently intelligible to him, even if he be a very poor botanist himself. They are expressed in a language of which he has always known at least the rudiments. And the physiologist assumes an external world, in which are a number of organized bodies, forming a part of a real system of things. He seeks to obtain a general knowledge of the peculiar phenomena presented by these bodies, and to fix their relations to the rest of the system. Every one knows something about physiology, even if he has never heard the word pronounced. One may know very little indeed, but even in that case one's ignorance differs from that of the physiologist only in degree. The point of view is essentially the same. Lungs are lungs, and exist and function in a real external world among other real things, and the only problem is to discover how they conduct themselves there. Physiological truths do not lead one away from the ways of thinking to which one is accustomed in common life.

In the same way the psychologist assumes an external real world, the world of matter and motion. In this world there are organized bodies presenting certain peculiar phenomena which he regards as indications of mental action. He accepts a plurality of minds distinct from each other and from the system of material things, each standing in a peculiar and intimate relation to one body. Each mind knows directly its own states, and knows everything else by inference from those states, receiving messages along certain bodily channels and reacting along others. Upon this basis he strives to give an accurate account of the contents of minds and to trace the history of their development. He stands upon the same ground as the ordinary man, and, as has been said above, he follows the same methods in his investigations, making use of introspection, observation, and experiment. He applies the methods in a broader and more scientific way; he is clearer, and more exact and thorough; but he remains a student of 'natural science.' However he may modify, as a result of his studies, his views of minds and of their relation to a material world, he still holds to the existence of distinct individual minds in certain relations to such a world and through that to each other. He conceives each as shut up to its representations of things, and dependent upon messages conveyed to it from without, as does the disciple of Locke. Ideas are, to him, like images in a mirror, numerically distinct from the things which they represent, and of which they give information.

A psychologist is, it is true, also a man, and he may be a metaphysician or epistemologist as well as a psychologist. In such capacity he may have his own opinion about these psychological assumptions. He may be a natural dualist or a hypothetical realist, a materialist or an idealist. As a philosopher he is free to choose, but not as a psychologist. In the latter capacity he puts all this aside, and remains on the plane of the common understanding, the plane of natural science. There is, of course, much that is vague in the thinking of the man who rests wholly on the plane of natural science. The physicist may have no very clear notion of what he really means by matter and energy, and yet he may be a good physicist. He may experiment with ingenuity, and observe and record phenomena with accuracy. And the psychologist may have the vaguest of notions as to the whole connotation of the word 'mind,' or of the phrase 'a material world,' and yet he may be a good psychologist, and materially add to our knowledge of minds. If he has not carried on with some measure of success the sort of reflective thinking demanded in epistemology or metaphysics, he will probably mix from time to time with his psychology more or less crude material that is not strictly psychological. But this is on his part a work of supererogation. He has the right, as has the physicist, to work in his own field, and to make use of some concepts he has not completely analyzed.

III.

It must be admitted that the position regarding minds and their relation to an external world taken by the plain man and by the psychologist contains, when criticised from the standpoint of epistemology, that most serious of difficulties, a flat contradiction. It assumes that each mind has only its representative images of things, and not the things themselves. When it asks how a given mind comes to have a knowledge of an external thing, it concerns itself with the messages that have been conveyed to the mind, the materials, so to speak, out of which the image has been built up. It describes the process of building up such an image, and, distinguishing sharply between the image and the thing, maintains that the mind knows only so much about the thing as is contained in this image or in other images obtained in the same way. It admits that, given such an image in the absence of the thing (a hallucination), the mind will have absolutely no way of knowing the thing absent except by referring to its other experiences and assuming this one, as abnormal, to be a false representative, and without a corresponding reality behind it. In other words, it shuts the mind up to its own circle of consciousness, and makes the external world present to it only by proxy. The outer world, *as the mind knows it*, is a complex mental experience, built up out of mental elements, and not the real outer world at all. It is only something that stands for the real outer world. Thus the very idea 'outer' is, to the mind possessing it, only a something in consciousness—an inner representative of genuine externality, but, in itself, not external at all.

Yet all this rests upon reasoning in which it has been assumed that the mind is *not* shut up to its own experiences, but directly knows an external world of things. A man looks at his own body, the body of his neighbor, and some material object in front of which both are standing, and he seems to himself to be immediately conscious of all three. He grants his neighbor a knowledge of the object, reasoning as I have indicated in an earlier section, and distinguishes between this man's knowledge of the object and the object itself. The former he makes a representative of the latter, connects it in thought with the man's brain, and admits that it may even not wholly resemble the object as he sees it. He holds that the man is not directly conscious of the object itself, but infers it through the representative image. He then applies the same reasoning to himself, and concludes that he is himself not really conscious of the three objects with which he started, but only of representative images. Through such images he must infer the whole outer world —his own body, other men, other things.

But if he is not really conscious of his own body, the other man's body, and the real object, what becomes of his reasoning? Of what is the other man's image representative, and with what is it connected? Is it representative of an external object? The object which it has been assumed to represent is now seen to be an image in his own consciousness, and there is not a shadow of evidence that it represents any other. With what brain is it connected?

The brain belonging to that body which is under observation? That body, too, is now seen to be his own image and relegated to consciousness. And what do his own images represent and where are they? His image of the object cannot represent that object seen out there in front of his body. That object *is* his image, if he is shut up to images, and his body as perceived is another image in his consciousness with the object. The real object, the real body, are things to be inferred. They are not open to direct inspection. His image of the thing must not be referred to the brain which belongs to the body of whose existence he is directly aware. It must be referred to a brain in a totally different world. Where look for evidence that it is connected with any such brain in any such body? Yet evidence must be adduced for all this. The doctrine that there is an external world, and that it is mirrored by a number of minds which are shut up to their own representations of it, is not usually advanced as a gratuitous fiction. It is supposed to rest upon evidence. Is not one conscious of one's own mental experiences? Can one not observe the relations of these to the material world? Can one not arrive by analogical reasoning at some notion of the mental states of others, and apply one's results to one's self? The appeal is to experience, to observation and induction. And yet if the conclusion of the argument be true, the foundation upon which it rests is a delusion. If one be really shut up to one's own mental states, one has never observed their relations to material things, and never inferred from changes in material things the mental states of another. It is a strange argument that rests upon an assumption which its conclusion declares to be false.

The difficulty here pointed out is not assumed gratuitously. It is really inseparable from the psychological position both of the plain man and of the psychologist, though it is forced into greater prominence by the superior consistency and clearness of the latter. The plain man distinguishes in his loose fashion between a man's ideas of things and the things themselves, and he admits that if the ideas are not true representatives, their possessor will not truly know the things.

The psychologist makes more distinct the line of separation, and conceives the man's whole experience of an outer world as a mere copy of what is external, describing in detail the elements of which it is built up, and the process of its formation. Both hold, explicitly or implicitly, that we perceive directly the outer world, and that we do not so perceive it, but only infer it. The contradiction is there. It is imbedded in the very structure of the psychological position, the standpoint of common thought and natural science.* Psychology is not called upon to solve it, for it does not concern psychology. The psychologist has done and still does excellent work while simply disregarding it. It may safely be left to the epistemologist.

And the epistemologist, if he be wise, will not quarrel with the psychological standpoint. He will recognize its value as a basis for work of a certain kind, and he will object to the psychologist's mixing with his psychology reasonings which, however true and valuable in themselves, serve only to darken counsel when mingled injudiciously with other things. He may, as epistemologist, point out where the difficulty really lies, show why the psychologist's assumption does not lead to error, and indicate how the results obtained by him are true even for epistemology when restated in certain ways. But he will regard such discussions as more or less out of place in a text-book of psychology, and regret finding them there, much as he would regret finding

* I shall not here discuss at length the peculiar philosophical doctrine which attempts to hold to the psychological standpoint and remove the contradiction by declaring that both the ideas and the things are really given in experience. One need only read what has been written in support of it to be convinced that its adherents, after distinguishing between ideas and things, confound them completely. If consciousness testifies to anything clearly and unmistakably, it is to the fact that we do not under normal circumstances see things double. The inkstand in front of me I see. I see only one. I may call that one *idea* or *thing* as I please. I am certainly not conscious in looking at it of both a copy and an original. How would a hallucination be possible if, in addition to the image, there were immediately present to the mind in perception also the thing represented by the image? The absence of the thing would be remarked at once. The doctrine is bad as psychology, and bad as epistemology. It cannot afford to be really clear, and it wisely takes refuge in obscurity, making the phrase 'knowledge of things' unintelligible. I shall again touch upon this point later.

metaphysical reflections introduced to any great extent in a treatise on physics.

IV.

The psychologist should, then, frankly accept the standpoint of common thinking, the natural science standpoint, without attempting to make it consistent. He should accept without question an external world; should assume that his own ideas of things represent it, and can be proved by observation to represent it truly; should infer from the actions of other bodies ideas more or less like his own, which are representatives of external things as are his ideas.* He should then, in harmony with the psychological fiction that no one is directly conscious of external real things, assume that each mind is shut up to its own representations; that the world is mirrored in each consciousness, and that the pictures of it in different minds may differ. To him each mind's knowledge of the external world should mean the presence in it of such a picture—of such and such mental elements arranged in such and such ways. He can then set before himself the difficult but perfectly definite task of discovering just the elements present in a consciousness, and the method of their arrangement. He may describe the building up of a consciousness, and may relate everything in it to the system of real things in an intelligible way. His work is, in a real sense of the word, scientific, and resembles closely what scientific men are trying to do in other fields. It does not demand metaphysical reflection. And the best results are to be obtained in psychology, I feel sure, by holding firmly to this scientific standpoint. When it is abandoned, as it sometimes is even by men whom one would most naturally expect to be strictly 'scientific,' the resulting obscurity and confusion are positively depressing to a lover of clear thinking. Of course, until epistemology as well as psychology has done its perfect work all one's thinking will not be perfectly clear. The plain man and the scientist both employ, as has

* I, of course, do not limit a consciousness to such ideas with 'objective reference.'

been indicated above, some conceptions that they have not completely analyzed. Such obscurities in their thought need not prevent it from being, as common thought or as scientific reasoning, clear and effective. One may reason well and clearly about spaces and times without being a philosopher, provided one remain on the plane of the common understanding, and ask only for scientific clearness. But when one occupied with scientific reasonings abandons the scientific standpoint, he is apt to introduce a very different kind of obscurity, and to encumber his task with serious and needless difficulties.

One or two illustrations will serve to make more clear the point upon which I wish to insist here. Within the limits dictated by an article of this kind no thought can be developed at great length. But I hope to make plain in one or two instances that psychology is a loser when the psychologist abandons the psychological standpoint, and tries to be psychologist and epistemologist at the same time.

V.

The psychological standpoint assumes, as I have said, an external world and a number of minds or consciousnesses reflecting it. To it, knowledge of the external world or knowledge in any sense means the presence of such and such complexes in a consciousness. The psychologist sets himself the task of analyzing these, exhibiting their elements, and giving an account of their genesis and their relations to other things. Now, as all external things, in so far as they are to enter a consciousness and be known, must enter through their proxies, it is of the very first importance not to conceive of the contents of a consciousness in such a way as to make it inconceivable how they can act as representatives at all. For example, the external thing to be known is a chair; the representative of this in a particular consciousness is the idea of a chair. Since a chair has legs, a seat, and a back, and is by these and other marks distinguished from other objects, must there not be in the representative, in the idea itself, something to correspond to these? If the chair has color as well as form, must not both elements be in some way represented in the idea? Must not the arrangement of represented elements in the object have its representative in an arrangement of representing elements in the idea? If one is to have a true and complete knowledge of any external thing, must not every single element in the external object have its correlate in consciousness? Surely it is inconceivable that one and the same thing in consciousness should stand for half a dozen things outside, and yet give true knowledge of them or truly represent them. Is the external object complex and the representative idea simple, the complexity of the object is not represented at all. If we assume, as I think the psychologist must, that each man knows of things only as much as is represented in his ideas, we must conceive of his ideas as really capable of being representatives of complex things. We must make his consciousness complex.

I do not think I can better emphasize the point I am making than by referring to the treatment of consciousness contained in Professor James' able book, which every one has so lately been reading.* This paper is in no sense a general criticism of the book, to which I refer merely for the sake of illustration. The book is, however, so frank, and so positive in its statements, that one naturally turns to it rather than to another, when one wishes to find a doctrine 'writ large.'

Professor James gives as the irreducible data of psychology four things, which he emphasizes by numbering them and putting them in separate frames. These four things are the psychologist, the thought studied, the thought's object, and the psychologist's reality (vol. I. p. 184). Why he has selected as the fourth the psychologist's reality, it is a little hard to say, for he states immediately afterwards that the psychologist believes in the reality of numbers 2, 3, and 4, and he himself evidently assumes without question, throughout the book, the reality of 2 and 3. One is further puzzled to know why Professor James has omitted from his enumeration something else which he appears to distinguish from all the things mentioned above, and to treat as an irreducible datum.

* *Principles of Psychology*, N. Y. 1890.

This something, the most mysterious thing in the world (p. 216), is Knowledge. Knowledge is, it is true, spoken of in one passage as a 'particular quality' or 'cognitive function' of states of consciousness (p. 185), and it is asserted in another that 'the brain being struck, the knowledge is constituted by a new construction that occurs altogether *in* the mind' (p. 219). One who read such passages alone might be inclined to regard knowledge as 'a mode of being of ideas' or a kind of consciousness. But Professor James considers the view of knowledge which would make it "a mode of *being* of 'ideas'" as 'pitifully impotent' (p. 476); and distinguishes between thoughts (consciousness, p. 185) and knowledge, as follows: "Almost anyone will tell us that thought is a different sort of existence from things, because many sorts of thought are of no things—e.g., pleasures, pains, and emotions; others are of non-existent things—errors and fictions; others again of existent things, but in a form that is symbolic and does not resemble them—abstract ideas and concepts; whilst in the thoughts that do resemble the things they are 'of' (percepts, sensations), we can feel, alongside of the thing known, the thought of it going on as an altogther separate act and operation in the mind" (p. 297). When one reads these passages and others like them, one feels that Professor James must look upon knowledge as a something quite distinct from all that to which he refers under the various names of thoughts, feelings, mental states, and states of consciousness. He must use the word mind to indicate a something which contains both states of consciousness and knowledge, two elements differing in kind, irreducible and ultimate. Such a position, consistently held, involves one, it is true, in difficulties that must seem to the average man rather startling. If, for instance, when one perceives a chair, one directly knows the chair, and feels, alongside of this knowledge, the thought or percept, it would seem quite conceivable that one could know the chair in the absence of the percept, and the function of percepts in perceiving is not apparent. One might know all sorts of things without having any mental states at all. One would have only knowledge—no consciousness, no thoughts.

It is merely possible that Professor James has omitted knowledge from his list of irreducibles because he does not regard it as concerning psychology at all. We naturally think of everything that can be found in a mind as a legitimate object of psychological study, but, according to an explicit statement in the chapter on 'The Methods and Snares of Psychology,' this science is limited to the study of thoughts, or states of consciousness, and their relations to things. In summing up his chapter Professor James says, with the emphasis of italics: "These thoughts are the subjective data of which he (the psychologist) treats, and their relations to their objects, to the brain, and to the rest of the world constitute the subject-matter of psychologic science." If psychology be really limited to this field, and if knowledge be something different from thoughts, of course it does not concern the psychologist. Nevertheless, in spite of the above citation, I can scarcely believe that Professor James seriously means to thus turn his back upon knowledge. He certainly uses it more or less in his psychology. I can think of only one pretext under which he may introduce it. It may be made to fall within the limits he has indicated, if it be defined as a relation between thoughts and external objects. In some passages Professor James appears to so define it. And yet, if this be the definition of knowledge, it is hard to see how knowledge can be wholly *in* the mind, and once there 'may remain there, whatever becomes of the thing' (p. 219).

I confess I am puzzled to know what to do with knowledge as treated by Professor James, but, all things considered, I think it must be added to the above-mentioned list of irreducibles—or rather may be allowed to take the place left vacant by the psychologist, the thinker, who turns out later in the book (Chapter X.) to be a thought, or pulse of consciousness. (In speaking above of the possibility of knowledge without consciousness, the knower was assumed to be an irreducible.) With his conversion into a thought, we have still on our hands thoughts or states of consciousness, objects, and knowledge. The last I will leave at this point, frankly admitting that, with all Professor James' assistance, I can form no clear notion of what he makes knowledge, or even feel certain whether he puts it in the mind or outside of it. It appears to be distinct from consciousness and also distinct from objects known. States of consciousness are generally admitted to differ from one another, and so are objects, but whether we are to regard knowledges as differing, so that what is in one mind can be distinguished from what is in another simply by a difference in this element, I cannot guess. If there be no difference in this knowledge element, then the contents of different minds, or of the same mind at different times, must be distinguished and classified through differences in their other elements—they differ in containing different conscious states. Psychology, in so far as it is concerned in distinguishing and describing mental contents, may go on while ignoring knowledge.

To turn to something less mysterious and more familiar, conscious states—these are recognized in every psychology, and by psychology as natural science must, I think, be made to cover even what is meant by knowledge. It must regard a man's consciousness as all he gets of anything, and must conceive even his knowledge or belief that there is something beyond his consciousness as in itself a mental complex, a conscious state, which can be analyzed, and described, and the genesis of which can be traced. Since, to it, conscious states must serve as the representatives of all that is external, it must, as has been said, so think of them that it will not be impossible to conceive how they can serve as representatives at all. It may enter an energetic protest—and this is the point to which what I have said in the paragraphs preceding has been leading up—it may enter an energetic protest against Professor James' conception of consciousness as an unanalyzable, indivisible unit, a something in which no parts can be distinguished. How can such a consciousness represent any object not itself unanalyzable and indivisible? How can it represent two objects as two, or an extended thing as extended?

Professor James declares unintelligible the assumption that our mental states are composite in structure, made up of smaller states conjoined (p. 145). Consciousness he regards as 'an integral thing not made of parts' (p. 177); it is a 'single pulse of subjectivity' (p. 278), 'undivided' (p. 277), 'containing no manifold of coexisting ideas' (p. 278). Every 'pulse of thought' (state of consciousness at a given moment) is an 'indecomposable unity' (p. 371), 'an uncompounded psychic thing' (p. 179). Feelings cannot be put together to make a consciousness: "Take a hundred of them, shuffle them and pack them as close together as you can (whatever that may mean); still each remains the same feeling it always was, shut in its own skin, windowless, ignorant of what the other feelings are and mean. There would be a hundred-and-first feeling there, if, when a group or series of such feelings were set up, a consciousness *belonging to the group as such* should emerge. And this one-hundred-and-first feeling would be a totally new fact; the one hundred original feelings might, by a curious physical law, be a signal for its *creation*, when they came together; but they would have no substantial identity with it, nor it with them, and one could never deduce the one from the others, or (in any intelligible sense) say that they *evolved* it" (p. 160). A consciousness supposed to consist of a dozen feelings Professor James compares to a sentence consisting of a dozen words each of which is in the mind of a different man. There is nowhere a consciousness of the whole sentence (p. 160).

Conscious states are, therefore, simple, unanalyzable units. They may, then, differ from each other, and be distinguished as this state or that, but they cannot be analyzed and described, they can only be named. How do sensations differ from percepts, percepts from concepts, memories from fictions? As well ask how the sensation of redness differs from that of blueness. The psychologist may arrange in a given order a number of unanalyzable units, and state their relations to other things, but a descriptive psychology which busies itself with such units can be called descriptive only by way of courtesy. It cannot give any sort of an answer to the question, 'What are they like?' or, 'How do they differ from each other?' It cannot describe. Of course Professor James does not attempt a psychology upon any such basis. When he treats of the various mental states he talks of them as does any one else, making them composite, analyzable, and describable. It is because he has done so pretty consistently that he has written so good a book.

It is difficult to know, in view of the wealth of material at hand, just what to cite in illustration of the fact that Professor James does not treat consciousness as an indivisible, unanalyzable unit at all. One must, however, begin somewhere, and I shall refer to a few passages almost at random. In the opening sentences of his book Professor James does not hesitate to speak of the 'complexity' of mental phenomena (I. p. 1); a little further on he speaks of 'groups of sensations forming determinate *objects* or *things*' (I. p. 19); and on the next page he asks: 'What are perceptions but sensations grouped together?' In the same chapter he regards it as certain that consciousness is much more 'developed' in the hemispheres than it is anywhere else, and asserts that the development of will must be 'proportional to the possible complication of the consciousness' (p. 78). In the chapter on 'The Perception of Space' we find such statements as the following: "In the sensations of hearing, touch, sight, and pain we are accustomed to distinguish from among the other elements the element of voluminousness" (II. p. 134); "Now my first thesis is that this element, discernible in each and every sensation, though more developed in some than in others, is the original sensation of space" . . . (p. 135); "If a number of sensible extents are to be perceived alongside of each other and in definite order, they must appear as parts in a vaster sensible extent which can enter the mind simply and all at once" (p. 146); "Measurement implies a stuff to measure. Retinal sensations give the stuff; objective things form the yard-stick; motion does the measuring operation" (p. 267). These sentences stand out from their context in all the prominence given by italics. They have not been penned thoughtlessly. And yet one marvels to think they are intended to apply to a something in which no parts can be distinguished—to an indecomposable unity. How can such a unity have elements distinguishable from other elements, or parts which can be placed alongside of each other, or portions which can be measured? And how can one speak of what is present in imagination as a blurred picture or a sharp image, or describe it as an image surrounded by a 'fringe' composed of 'transitive' parts of consciousness, if all that is in consciousness be wholly without parts, an indecomposable unit (II. p. 49). Such partless images we might naturally suppose capable of representing only mathematical points, but we discover one of them to be the image of Professor James' absent friend. How Professor James can know of whom he is thinking is a mystery—a mystery worthy, I think, of a place beside 'knowledge' as one of the most mysterious things in all the world. What, too, shall we say to the percept, which is also indecomposable, when we find it to be the general law of perception that whilst part of what we perceive comes through our senses from the object before us, another part always comes out of our own head (II. p. 103)? or when we read: "Who can be sure, in his sensible perception of a chair, how much comes from the eye, and how much is supplied out of the previous knowledge of the mind?" (I. p. 191). And how can we regard an emotion as an indecomposable unit, when we happen upon the exclamation, "Who can enumerate all the distinct ingredients of such a complicated feeling as *anger?*" (I. p. 191).

We have, furthermore, repeated statements from our author to the effect that this indivisible something called consciousness can be split. "We shall find," he says, "in Chapter X, numerous proofs of the reality of this split-off condition of portions of consciousness" (I. p. 165). Again: "It must be admitted, therefore, that, in certain persons, at least, the total possible consciousness may be split into parts, which coexist but mutually ignore each other, and share the objects of knowledge between them" (p. 206); "How far this splitting up of the mind (consciousness) into separate consciousnesses may exist in each one of us is a problem" (p. 210); we "are forced to admit that a part of consciousness may sever its connections with other parts and yet continue to be" (p. 213); "Now although the *size* of a secondary self thus formed will depend upon the number of thoughts that are thus split off from the main consciousness, the *form* of it tends to personality, . . ." (p. 227). Are we really concerned here with an indecomposable unit? To conceive such a thing split into two or more parts of different sizes is indeed a difficult task. I can think of only one way in which such a consciousness as Professor James supposes can be divided. It is granted time-parts (p. 279), and the fragmentary consciousnesses may, perhaps, divide these among themselves. This interpretation does not, however, adjust itself to the text at all, and it would be doing Professor James an injustice to foist it upon him.

The chapter on 'The Consciousness of Self,' although it contains occasional lapses into the 'indecomposable unity' idea, may yet be said to treat consciousness with fair consistency as a complex and decomposable thing. It has 'elements' and 'parts.' "Compared with this element of the stream," says our author, "the other parts, even of the subjective life, seem transient external possessions, of which each in turn can be disowned, whilst that which disowns them remains" (p. 297). As this element is "more incessantly there than any other single element of the mental life, the other elements end by seeming to accrete round it and to belong to it" (p. 298). The self under discussion is never found in the stream of consciousness all alone (p. 299); it is strongly contrasted with all the other things consciousness contains (p. 302).

It is unnecessary, I think, to multiply citations. The few that I have given are sufficiently direct and unambiguous. One might quote from almost any chapter—from that, for example, on attention, where we find such phrases as: "It is the simplest possible case of two discrepant concepts simultaneously occupying the mind" (p. 410); or from that on conception, where an image held before the mental eye is distinguished from the vague consciousness which surrounds it (p. 473). But as my object is illustration and not criticism, I shall quote but one more passage, which we may take as an apology (Professor James gives several such) for the expressions used in the citations already given. It reads as follows: "For the ordinary 'analytic' psychology, each sensibly discernible element of the object imagined is represented by its own separate idea, and the total object is imagined by a 'cluster' or 'gang' of ideas. We have seen abundant reason to reject this view. An imagined object, however complex, is at any one moment thought in one idea, which is aware of all its qualities together. If I slip into the ordinary way of talking, and speak of various ideas 'combining,' the reader will understand that this is only for popularity and convenience, and he will not construe it into a concession to the atomistic theory in psychology" (II. p. 45).

Now far be it from me to insist that one may not, for convenience, speak with the vulgar, while thinking with the learned. It may be wise to do so on occasion. But it does not seem unreasonable to demand that the language one uses shall be capable of a translation into the tongue of the scholar when such a translation is asked for. In the present instance, I do not believe the thing can be done. I do not think Professor James could possibly say what he has said about mental phenomena and consistently use language which would treat consciousness as an indecomposable unit. As a philosopher he has accepted such a consciousness: as a psychologist he has fallen to the level of natural science and common-sense, and eschewed it completely. The interruptions from the philosopher introduce a disturbing element into the lucid expositions of the psychologist. One meets with difficulties which seem gratuitous. Much of the blame I should be inclined to lay on the shoulders of that unhappy intruder 'knowledge,' who seems to love darkness rather than light, and whose deeds may be more than suspected to be evil.*

* We may at least, with some confidence, accuse 'knowledge' of arbitrariness and unjust discrimination. We find that a consciousness cannot consist of a hundred feelings for the reason that each feeling is "shut in its own skin, windowless, ignorant of what other feelings are and mean" (I. p. 160). No feeling—being thus windowless and 'hide-bound'—can know another existent feeling. But a feeling (thought, consciousness) may know a material object which is not a feeling (p. 197); and it may also know another feeling, provided that other be non-existent—nay, in that case, it may even adopt it and hug it to itself (p. 340).

I am inclined to think that Professor James' treatment of consciousness is the result of a conspiracy between two quite distinct influences. We are almost tired of hearing from the apperceptionists that the contents of a consciousness must not be regarded as resembling a heap of wooden counters to which we may add, or from which we may subtract, a given number without causing any change in the rest. The notion of consciousness as an organic whole, and not an aggregate of changeless elements, is in the air, and deservedly receives emphasis. In the associational

The remedy for these ills appears to me to lie in a return to psychology as natural science. The excellence of Professor James' book—which I have chosen out of a large class for use as a text-book in the University of Pennsylvania—lies to a great degree in the fact that he does, in spite of himself, usually treat psychology as a natural science. It is easy to find fault with special points in almost any book, but it would be the extremest injustice to estimate a book after a consideration of such points alone. And I hope that what I have said will not be taken as an expression of opinion regarding Professor James' work as a whole. From many points of view I admire his volumes greatly.

VI.

I have developed my first point at such length that I cannot do more than mention a second. I must, however, mention it, for it seems to me of such importance that it should not be wholly passed over. Psychology as natural science must not merely assume a consciousness complex and capable of representing, in some intelligible sense of the word, an external world beyond it, but it must also recognize all conscious states to be mere phenomena, resolutely confine itself to phenomena, and, as science, eschew all metaphysical entities—'substrata,' 'unit-beings,' 'transcendental' selves, and what not. Whether one conceive conscious states as 'parallel' to brain states, or conceive of them as belonging with these latter to the one series

psychology which Professor James criticises ideas were much deader things than they are to us to-day, and I think his reaction against such a view of ideas very natural. But this influence alone would not have made him, I think, speak of consciousness as an indecomposable unity. The blame for this lies with his doctrine as to the nature of knowledge. If, when I perceive an object, I have, in addition to my percept, a 'knowledge' of the object, which gives me, somehow, the object known and the percept 'alongside' of each other, I may reduce my percept to a mathematical point if I choose, and yet go on talking about the parts of the object. The percept has become a useless thing. Its function has been usurped by 'knowledge.' I think that this is the real explanation of the fact that Professor James can speak of consciousness, even occasionally, as having no parts. He falls back on 'knowledge' for information which could not possibly be extorted from 'an uncompounded psychic thing.'

of causes, and determining physical movements, in either case one may study them from the natural-science point of view. They are in any case phenomena, which may be analyzed and described, and the relations of which to other phenomena may be determined by accepted scientific methods. The physiologist studies the phenomena presented by organized bodies without reference to any metaphysical 'substratum' or 'reality' underlying them, and so should the psychologist study the phenomena of mind. To him sensations, percepts, judgments, knowledge, the self, — everything he may find in a consciousness should be a phenomenon and nothing more, a conscious state or a component part of a conscious state. To divide mental phenomena into classes, and account for the one class by reference to that phenomenal something we call the brain, while referring the other to a 'self' not belonging to the world of phenomena at all, is surely unscientific. As well might the physiologist refer some of the phenomena of the human body to the 'substratum' granted it by the philosopher, holding explanation of them, in the ordinary sense of the word, inadmissible. The world of 'substrata' belongs to the metaphysician, if it belong to any one, and the psychologist will not gain by trespass. Let him stay on his own ground.

THE CASE OF JOHN BUNYAN. (II.)

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III.

"Now you must know," says Bunyan, "that before this I had taken much delight in Ringing,* but my *Conscience* beginning to be tender, I thought such practice was but vain, and therefore forced myself to leave it, yet my mind hankered. Wherefore I should go to the Steeple-house, and look on it, though I durst not ring. . . . But quickly after, I began to think, *How if one of the Bells should fall?* Then I chose to stand under a main Beam, that lay overthwart the Steeple, from side to side, thinking there I might stand sure. But then I should think again, Should the Bell fall with a swing, it might first hit the wall, and then rebounding upon me, might kill me for all this Beam. This made me stand in the Steeple-door; and now, thought I, I am safe enough; for, if a Bell should then fall I can slip out behind these thick Walls, and so be preserved notwithstanding. So after this I would yet go to see them ring, but would not go further than the Steeple-door. But then it came into my Head, How if the Steeple itself should fall? And this thought, it may fall for aught I know, when I stood and looked on, did continually so shake my mind that I durst not stand at the Steeple-door any longer, but was forced to flee for fear the Steeple should fall on my head."

The parallel between Bunyan's case and that of Dr. Cowles's patient, whose experience is so fully described in the

* I.e., of course, in ringing the chimes of the village church. Venables has skilfully pointed out, in various passages of Bunyan's writings, how deep a train of associations this practice later involved for the poet.

remarkable paper before cited, will from this point onwards become interesting to us. It is noteworthy that Dr. Cowles's patient, after some history of childhood fears, beginning at about ten years of age, became, for a time, 'well of these morbid experiences,'* but afterwards, in youth, experienced a fresh form of her previous disorder, and met this relapse at first in the form of 'feelings of hesitation in performing simple acts,' with a consequent necessity of repeating many such acts to be sure that they were right. 'From this point,' says Dr. Cowles of his patient, 'all the rest follows in its morbid train.' The fortunes of Bunyan were to be, up to a certain point, decidedly similar. The childhood period, with its warning terrors, had given place for a time to a healthy youth. But the elementary conscientious fears which now appeared, and which forced the lately reckless Bunyan to outward acts of unreasonable timidity, were soon to give place, as in Dr. Cowles's patient, to far more insistent and systematized impulses. In both of these cases the topics about which the insistent impulses finally systematized were matters of inner conscientious scruples. In both cases the general outward bearing and conduct long remained as far as possible normal, except where the inner sufferings of the patient must perforce break through and show themselves. In Bunyan's case it is interesting that these first signs of the coming storm were motor reflexes of a timid and partly of a morbidly inhibitory sort, produced irresistibly at the sound of those bells which he had so much loved to hear, and which, as Venables has shown by quotations from his later works, he never afterwards learned to forget.

The conversation of certain poor and godly people, about this time, revealed to Bunyan that, with all his legality, he had not yet learned what the true spiritual life is; and herewith began a second stage of his conversion. The consequence was much continuous meditation upon this higher religious life, and 'a softness and tenderness of Heart,' whereby his mind became 'fixed on Eternity,' and, for the time, refused 'to be taken from Heaven to Earth.' Theologi-

* Cowles, *loc. cit.* p. 238.

cal controversy with companions added itself to the foregoing to intensify Bunyan's interest in the secret of true faith. He now constantly read the Bible, which, however, to him, in his environment, seemed rather a collection of texts than of connected treatises. Henceforth his inner life was full of a not uncommon, but in his case especially significant, associative process, whereby he was largely at the mercy of any single text of his now well-thumbed Bible that at any moment might chance to occur to him, wholly separated, of course, from its context. He might be depressed. At such a time a threatening or discouraging text would come to mind; this or that Scripture would 'creep into his soul,' and wound him, or chill him all through. He could in but very small degree resist the effect of chance association by recalling the original relations or the meaning of this text as determined by its actual setting at the place where it occurs. No, this 'word' had come to him alone; alone he must interpret it and apply it to his case. Did its serious import overwhelm him? Then there was no way but to hunt at random, either in his Bible, or in the recesses of his chance associations, for some other 'word' to set over against the first. Then would follow very possibly long processes of this mere balancing of texts. One 'word' must be set against another, one set of texts must be neutralized by texts whose immediate emotional effects were more comforting. Bunyan also developed in connection with such tasks a peculiarly skilful sort of inner dialectic whereby he estimated the force of each text. He reasoned very subtly with these his own shadows. The decision of nearly every such crisis was determined in the end, however, less by the conscious dialectic itself than by the chances of association. At last, perhaps after days, in the later stages of his malady after months, of conflict, some decisive word would come to mind, would more or less irresistibly 'dart' into his soul, would even half seem to be spoken within him (a few times with the force of a pseudo-hallucination, and only once or twice with almost complete hallucinatory vigor). The 'word' that association thus made victorious might by its very clearness, or by the strength of its emotional setting, banish all the former 'words' from mind, and for the time doubts would leave him. Or again 'two Scriptures' would 'meet' in his heart, and one of them would triumph. This process is frequently exemplified in the *Grace Abounding*, and was of course largely determined, apart from the abnormal capriciousness of his associative processes, by Bunyan's religious opinions and companionships. But this method of thinking was of course an inconvenient complication in view of his now imminent disorder.

At the stage of his pilgrimage now reached, he began to read Paul's epistles with eagerness. They did not decrease his dialectical tendencies. One day, when alone on the road, he found himself wondering gloomily, as he had been doing for some time, whether he really had saving faith or no. Whereupon the 'Tempter,' who of course, in our author's account, has to bear the responsibility for many of Bunyan's insistent impulses, and for a large part of his associative processes, suggested, as he had several times done before, that there was no way for Bunyan to prove that he had faith save by trying to work some miracle; "which Miracle at that time was this, I must say to the Puddles that were in the horse-pads, *Be dry*, and to the dry places, *Be you the Puddles.* And truly, one time I was going to say so indeed; but just as I was about to speak, this thought came into my mind, *But go under yonder Hedge and pray first that God would make you able.* But when I had concluded to pray, this came hot upon me, That if I prayed, and came again and tried to do it, and yet did nothing notwithstanding, then be sure I had no Faith, but was a Castaway and lost. Nay, thought I, if it be so, I will never try yet, but will stay a little longer."

In this account it is of course the hesitancy and the brooding, questioning attitude that is symptomatic, and not the logic of the quaint reasoning process, which, in view of Bunyan's presuppositions, is normal enough in form. To such broodings the dreamer added about this time one very elaborate symbolic inner vision of his unhappy state as related to the state of the godly people whose faith he envied. The vision, which, as reported, is a fine instance of the automatic visualizing process already characterized, need detain us here no further. It is noteworthy that Bunyan reports it without any surprise, as an incident of a type very familiar in his inner life. The striving with chance Scripture passages continued, and now often drove him to his 'wit's end.' The comforting passages were occasionally hit upon, but only to give way soon to doubts. His questions as to what faith is, and whether he was of the elect, had already reached the limits of the normal. He was "greatly assaulted and perplexed, and was often," he says, "when I have been walking, ready to sink where I went with faintness in my mind." This is one of the few hints that we get of Bunyan's physical state at this time. The 'Tempter' was meanwhile quite capable of suggesting, as regards Bunyan's relation to his fellows in the faith, that these [viz., the known 'godly people' aforesaid] being converted already, "they were all that God would save in those parts; and that I came too late, for these had got the Blessing before I came." This thought was insistent enough to cause Bunyan great distress, and even anger at himself for having lost so much time in the past. After really desperate and lonely struggles with such wavering hopes, gloomy fears as to his salvation, and insistent questions and doubts on the whole subject, he at length forsook his solitude, and appealed for help to the 'godly people' themselves, who took him to their pastor, Mr. Gifford.

But herewith Gifford only made Bunyan's case for he time worse, by assuring him that he was a very grievous sinner, and by drawing his attention away from the universal problems about faith and election, back to the particular facts concerning the vanity of his wicked heart. The result was a new stage, wherein all the elements present in the two previous stages of his experience were morbidly combined, and the associative processes so inimical to his peace were rendered more automatic and systematic than ever. The first stage, it will be remembered, had been one of systematically insistent scrupulosity as to the details of his conduct, with elementary inhibitions and fears. The second stage had been one of large and more 'tender' emotional states, and of generalized broodings and doubts as to faith and election, accompanied with occasional feelings of general physical weakness and faintness. But now this elaborate process of morbid training came to combine both generalized and specialized elements. The first effect was that instead of the 'longing after God' which had characterized the immediately previous state of mind, Bunyan now found in himself a perfect chaos of 'Lusts and Corruptions,' 'wicked thoughts and desires which I did not regard before.' He must 'hanker after every foolish vanity.' His heart "began to be careless both of my Soul and Heaven; it would now continually hang back, both to and in every duty; and was as a Clog to the Leg of a Bird to hinder her from flying. Nay, thought I, now I grow worse and worse; now am I further from Conversion than ever I was before. Wherefore I began to sink greatly in my Soul, and began to entertain such discouragement in my Heart as laid me low as Hell. If now I should have burned at the stake, I could not believe that Christ had love for me: alas, I could neither hear him, nor see him, nor feel him, nor savour any of his things. I was driven as with a Tempest; my Heart would be unclean; the Canaanites would dwell in the land." To this fairly classic description of his general state, Bunyan now adds for the first time a mention of the presence of insistent 'unbelief,' whereof we shall soon hear more. Meanwhile, however, as he adds in a most characteristic fashion: "As to the act of sinning, I was never more tender than now. I durst not take a pin or a stick, though but so big as a straw, for my conscience now was sore, and would smart at every touch; I could not now tell how to speak my words, for fear I should misplace them. Oh, how gingerly did I then go in all I did or said! I found myself as on a miry Bog that shook if I did but stir; and was as there left both of God and Christ and the Spirit, and all good things."

When a man has once got so far into the 'Slough of Despond' as this, there is indeed no way but to go on. Such insistent trains of morbid association cannot be mended until they first have grown worse. The process of systematization continued in this case, much as in that of Dr. Cowles's patient.* There were for Bunyan, to be sure, the occasional

* Cowles, *loc. cit.* pp. 240–45.

remissions due to the temporary success of this or that Scripture passage. So in one instance the effective suggestion came from without, through a sermon on the text, *Behold, thou art fair, my Love; behold, thou art fair*—a sermon whose pedantically multiplied headings Bunyan years later remembered with perfect clearness. As he was going home after the sermon the two words, *My Love*, came into his thoughts, and "I said thus in my heart, *What shall I get by thinking on these two words?*" Whereupon "the words began thus to kindle in my spirit, *Thou art my Love, thou art my Love*, twenty times together, and still as they ran thus in my mind, they waxed stronger and warmer, and began to make me look up. But being as yet between hope and fear, I replied in my heart, *But is it true?* At which that Sentence fell in upon me, *He wist not that it was true which was done by the angel.* Then I began to give place to the Word, which with power did over and over make this joyful sound within my soul, *Thou art my Love, thou art my Love; and nothing shall separate me from my Love;* and with that Romans eight, thirty-nine, came into my mind. Now was my heart full of comfort and hope, . . . yea, I was now so taken with the love and mercy of God that I could not tell how to contain till I got Home." But this mood of course proved to be unstable, and Bunyan soon "lost much of the life and savour of it."

"About a Week or a Fortnight after this," continues Bunyan, "I was much followed by this Scripture, *Simon, Simon, Satan hath desired to have you.* And sometimes it would sound so loud within me, yea, and as it were call so strongly after me, that once above all the rest, I turned my head over my shoulder, thinking verily that some Man had, behind me, called to me; being at a great distance, methought he called so loud." This pseudo-hallucination of hearing, secondary, be it noted, to the now frequent and insistent automatic motor process of internal speech, whereby Bunyan obviously found such texts forced upon his attention, concluded this special episode, and this particular text, as he expressly tells us, came no more. Hallucinations of hearing form no part of this case in any but this secondary, transient, and 'borderland' form—a fact, of course, which has to be clearly borne in mind in estimating the phenomena. Later reflection, of a sort perfectly normal upon Bunyan's presuppositions, convinced him afterwards that this visitation was a heavenly warning that a 'cloud and a storm was coming down' upon him; but at the time he 'understood it not.' The minuteness of the account hereabouts is evidence both of the depth of the experiences, and of the remarkable intactness of Bunyan's memory amidst all this condition of irritable nervous instability of mood on the one hand, and of morbidly persistent brooding on the other.

IV.

But now for the culmination of the disorder,—a culmination which appeared in three successive and intensly interesting periods or stages, each one of which Bunyan narrates to us with extraordinary skill and vigor.

"About the space of a month after," he continues, "a very great storm came down upon me, which handled me twenty times worse than all I had met with before." Of this 'storm' the primary element, as we should now say, was a melancholic mood, of a depth and origin to him unaccountable. Former moods had been largely secondary, as would appear, to his doubts, although primary states of depression had also played their part. But this time the insistent impulses appeared as obviously quite secondary to the mood. The latter "came stealing upon me, now by one piece, then by another; first all my comfort was taken from me, then darkness seized upon me, after which" (the order is noteworthy) "whole floods of blasphemies, both against God, Christ, and the Scriptures, were poured upon my spirit, to my great confusion and astonishment. These blasphemous thoughts were such as also stirred up questions in me, against the very Being of God, and of his only beloved Son; as, whether there were, in truth, a God, or Christ, or no? And whether the holy Scriptures were not rather a fable, and cunning story, than the holy and pure Word of God? The tempter would also much assault me with this: *How can you tell but that the Turks had as good Scriptures to prove their Mahomet the Saviour as we have to prove* *our Jesus is? And could I think that so many ten thousands in so many Countries and Kingdoms, should be without the knowledge of the right way to Heaven (if indeed there were a heaven), and that we only who live in a corner of the Earth should alone be blessed therewith. Every one doth think his own religion rightest, both Jews and Moors and Pagans! And how if all our Faith, and Christ, and Scriptures should be but a Think-so too?*"

Bunyan of course sought to argue with these doubts, but this expert in the dialectics of the inner life now very naturally found all the weapons in the enemy's hands. He would try using the 'sentences of blessed Paul' against the 'tempter.' But alas! it was Paul who had taught both Bunyan and the 'tempter' how to argue with subtlety, and now the reply at once came, in interrogative form: How if Paul too were a cunning deceiver, who had taken 'pains and travail to undo and destroy his fellows'? Bunyan's only remaining comfort was at this point the usual one of the patients afflicted with such harassing enemies. He was aware, namely, that he hated his own doubts, and was so, in a way, better than they. But, as he expressively words it: "This consideration I then only had when God gave me leave to swallow my Spittle; otherwise the noise and strength and force of these temptations would drown and overflow and as it were bury all such thoughts." Meanwhile insistent motor impulses of a still more specific sort occurred. Bunyan frequently felt himself tempted 'to curse and swear, or speak some grievous thing against God.' He compares his state to that of a child whom a gipsy is stealing and carrying away, 'under her apron,' 'from friend and country.' "Kick sometimes I did, and also shriek and cry; but yet I was bound in the wings of the temptation, and the wind would carry me away." Nor were the fears of hopeless insanity, so common in such patients, absent from Bunyan's mind, so far as his knowledge permitted him to formulate them. "I thought also of Saul, and of the evil spirit that did possess him; and did greatly fear that my condition was the same with that of his." The sin against the Holy Ghost was of course suggested to Bunyan's mind amongst other possible crimes, and it seemed at once, of course, as if he 'could not, must not, neither should be quiet' until he had committed that. "Now, no sin would serve but that; if it were to be committed by speaking of such a word, then I have been as if my Mouth would have spoken that word, whether I would or no; and in so strong a measure was this temptation upon me, that often I have been ready to clasp my hand under my Chin, to hold my Mouth from opening; and to that end also I have had thoughts at other times, to leap downward into some muck-hill hole or other to keep my mouth from speaking."

But to follow further this chaos of motor processes is, for our purposes, hardly necessary. A system there indeed was amidst the chaos, but this system is now manifest enough. Suffice it that the whole race had now to be run. At prayer Bunyan was tempted to blaspheme, or the 'tempter' moved him with the thought, *Fall down and worship me.* At the sacraments of the church, which, although not yet a member of the church, he attended as spectator, in hope of comfort, he was also 'distressed with blasphemies.' There were still no true hallucinations, but "sometimes I have thoughts I should see the devil, nay, thought I have felt him, behind me, pluck my Clothes." As to mood, Bunyan was now usually 'hard of heart.' "If I would have given thousands of pounds for a Tear, I could not shed one; no, nor sometimes scarce desire to shed one." Others 'could mourn and lament their sin.' But he was, as he saw, alone among men, in this hardness of heart, as in the rest of his troubles. The unclean thoughts and blasphemies aforesaid were likely, as is obvious, to appear as reflexes, of an inhibitory type and meaning interestingly analogous to his earlier conscientious scruples themselves. For these blasphemies were excited by and opposed to any pious activity, precisely as the old conscientious fears had been excited by and inhibitory of any activity which his natural heart had most loved. Hearing or reading the Word would be sure, for instance, to bring to pass the blasphemous temptations. The 'tempter' was a sort of inverted conscience, busily insisting upon whatever was opposed to the pious intention. Meanwhile Bunyan of course complains of that general confusion of head of which all such sufferers are likely to speak. When he was reading, "sometimes my mind would be so strangely snatched away and possessed with other things, that I have neither known, nor regarded, nor remembered so much as that sentence that but now I have read." This 'distraction' was often at prayer-time associated with insistent inner visual images, as of a 'Bull, a Besom, or the like,' to which Bunyan was tempted to pray.

Bunyan attributes to this condition an endurance of about a year. Detailed and obviously trustworthy as his psychological memory is, his chronology seems to suffer, very naturally, with a tendency to lengthen in memory the successive stages of his affliction. One can hardly find room, in the known period occupied by the entire experience, for such lengthy separate stages as the writer assumes. The present, or first culminating period of the malady, finally passed off by a gradual decline of the insistent symptoms,—a decline assisted, as would appear, by a controversial interest which Bunyan was just then led to take in the 'errors of the Quakers,' to whose condemnation he devotes a paragragh of his text, hereabouts, in his Autobiography. The objective turn which such controversial thoughts gave his mind was used, as he himself feels, by the Lord, to 'confirm' him.

One would suppose that the foregoing story, written with the most moving pathos by Bunyan, ought of itself to be a sufficiently obvious confession, even to readers of comparatively little psychological knowledge. The long-trained habits of verbal and emotional association which are exemplified in these repeated experiences with the remembered passages of Scripture, the systematized attitudes of conscientious fear and inhibition which date back to the beginning of our author's conversion, the obvious essential identity between all these mental habits, and those which Bunyan's 'tempter,' his inverted conscience,—equally fear-compelling, equally inhibitory of his present ardent desires,—represented, whenever this 'tempter' disturbed him at prayer, even as his conscience had in former days learned to disturb him at bell-ringing,—all these phenomena give us a most instructive object-lesson concerning the familiar processes by which the human brain, whether in health or in disorder, gets moulded. The emotional instability that lies at the basis of this particular morbid process,—an instability without which, of course, just these habits could never have become such formidable enemies, is perfectly clear before us. Of the precise physical basis of this instability we can indeed only form conjectures; but we know that this was an extremely sensitive brain, and that the childhood dreams and terrors had been of a type such as to furnish obvious warnings that this mind needed especial care. We know too that such care was in so far lacking, as this still very young man had now to suffer the anxieties of providing for his family at a moment when his troubles about his soul were intense, and when his poverty was great. Meanwhile, one aspect of the symptoms, which we have already noticed, is as obvious as it has been, in the past, neglected by Bunyan's readers. This man, a born genius as to his whole range of language-functions, had been from the start a ready speaker, had developed in boyhood an abounding wealth of skilfully bad language, and had then, in terror-stricken repentance, suddenly devoted himself for many months to a merciless inhibition of every doubtful word. We observe now that insistent motor speech-functions were the most marked and distressing of his mental enemies, and that both the tempter, and that comforter whose strangely suggested Scripture passages occasionally consoled Bunyan's heart, tended to speak, 'as it were,' within the suffering soul. When one considers, still further, the careful way in which, by his own description, Bunyan excludes from his case all hallucinatory elements except the few pseudo-hallucinations, how can one doubt the type of patient with whom one has to deal? Memory, as one sees, is remarkably intact. Any tendency to pathological delusion is obviously lacking; for that Bunyan is beset by the 'tempter' is for him a mere statement of the obvious facts in the light of his accepted faith, and is, from his point of view, a strictly normal and inevitable hypothesis, which he never in any morbid fashion misuses. For the rest, he retains throughout as clearly critical an attitude towards his case as the situation in any wise permits; otherwise we should never have come to get this beautiful confession.

And yet, as said, the biographers have repeatedly missed nearly all these psychological aspects of the case, and that, too, whatever their theory of the poet's experiences. Some, as pointed out, have endeavored to conceive all this as merely the deep religious experience of an untutored genius. Religious experience it indeed was; nor does its deep human interest suffer from our recognition of its pathological character. Genius there also, indeed, is in every word of the written story. But the specific sequence of the symptoms thus recorded, and the striking parallel with such modern cases as that of Dr. Cowles's patient (who was surely no genius, and whose morbid conscience busied itself with far more earthly matters than the religious issues central in Bunyan's mind)—these things forbid us to doubt that the phenomena are characteristic of a pretty typical morbid process, which has certainly gone on in very many less exalted brains than was that of Bunyan. Other biographers have spoken, as Macaulay did, of 'fearful disorder,' but have had no sense of the clear difference between an hallucinatory delirium, which could only develop either in a very deeply intoxicated or exhausted, or else in a hopelessly wrecked brain, and a disorder such as this of Bunyan's, which could get thus dramatically systematized only in a sensitive but nevertheless extremely tough and highly organized brain, whose general functions were still largely intact. So sympathetic an observer as Froude, on the other hand, almost wholly ignoring the pathological aspect of the case, can actually suppose that Bunyan's 'doubts and misgivings' were 'suggested by a desire for truth'; because, forsooth, from the point of view of a nineteenth-century thinker: "No honest soul can look out upon the world, and see it as it really is, without the question rising in him whether there be any God that governs it all." Froude imagines, therefore, that Bunyan later went no further in doubt largely because 'critical investigation had not yet analyzed the historical construction of the sacred books.' But surely thus to argue is wholly to miss what it is that makes a given sort of questioning, or of other impulse, normal or morbid, for a given man, and under given circumstances. And here is perhaps the place to define more precisely this very matter in our own way.

Morbidly insistent impulses, of whatever sort, are, oddly enough, never morbid merely because they insist. For all our most normal impulses are, or may become, insistent. One has a constantly insistent impulse to breathe, a frequently insistent impulse to eat; and one's life depends upon just such insistences. Insistent desires keep us in love with our work, take us daily about our duties, guide our steps back to our homes, seat us in our chairs to rest, are with us, in their due order, from morning to night, whether we bathe, dress, walk, speak, write, or go to bed. To run counter to such normally insistent impulses pains, and may in extreme cases very greatly distress, or even in the end quite demoralize us. Insistence of will-functions is, then, so far, a sign of health, and means only the kindly might of sound habit. An 'imperative impulse' of the morbid sort is therefore, in the first place, one that, under the circumstances, opposes instead of helping our normal process of 'adjustment to our environment.' But herewith we have still only defined, so far, that element of the morbid impulse which the latter shares in common with all defective mental processes. The peculiar *differentia*, however, of all the various forms of morbidly insistent thoughts, fears, temptations, etc., is that their tendency to bring one out of 'harmony with his environment' is subjectively expressed, for the sufferer himself, in the form of a sense that the fear, thought, or other impulse in question is opposed to his fitting relation to his environment *as he himself conceives that relation.* The hallucination or the delusion gives one a pathologically falsified environment, and then one's adjustment objectively fails, because one knows not rightly the truth to which one ought to be adjusted. Confusedness, or mere incoherence of ideas and impulses, or other such general alteration of consciousness, equally means failure, but here also without any completer subjective sense of what one's failure objectively involves. But the sufferer from morbidly insistent impulses, whether or no he conceives his environment rightly, still knows how he conceives it, and has his general plans of thought and will; but he himself, meanwhile, finds, within himself, 'in his members,' 'another law warring against the law' which he has accepted as his own. Without pretty definite plans, then, there can be no morbidly insistent impulses. Failure, or strong tendency to failure, in the adjustment, as conceived and planned by the sufferer himself,—such failure being due to this inner conflict,—this it is alone that makes us speak of morbidly insistent impulses.

But not even thus do we define all that it is necessary to bear in mind in judging such cases. Impulses, feelings, thoughts, more or less inimical to our deliberate plans, are constantly, if but faintly, suggested to us, by our normal overwealth of perceptions and of associations. Without such overwealth of offered perceptions and associations, we should not have sufficient material for mental selection; yet such overwealth is necessarily full of solicitations, tempting us, with greater or less clearness, to abandon or to interrupt our chosen plans of action. Nor is there any fixed limit to the range of those 'imaginations as one would,' that, as Hobbes already pointed out, may at any moment be initiated in a man's inner life by chance experience and association. Therefore, mere opposition between our chance impulses and our plans is a perfectly normal experience.

Normal impulses then are insistent. And normal trains of impulse, or plans of conduct, are constantly besieged by the faint but more or less inimical distractions of normal experience. When, then, is any single impulse, as such, abnormal? When it insists? No, for breathing is an insistent impulse. When it opposes the current trains of coherent thought or volition? No, for every momentary inner or outer distraction tends to do that; and there is hardly any known impulse or thought or feeling of which a normal man may not at almost any moment be reminded, through the chances of perception and of association. What then is the subjective test of the abnormal in impulse? One can only find it in this: Association chances to suggest any impulse inimical to one's actually chosen plans for 'adjustment to the environment.' So far there is no essential defect. This happens to anybody. But normally the coherence of one's series of healthily insistent or of voluntary impulses is so great, or the strength of the intruder soon becomes, under the influence of the opposed ruling interests, so faint, that this intruder is erelong sent below the level of consciousness, or harmlessly 'segmented,' and that with an ease and a speed proportioned to the incongruity and to the felt inconvenience of this enemy itself. But, in the abnormal cases, things go otherwise. Perhaps the intruding impulse is not a chance one, but is itself part of a previously established system of inhibitory habits. Or perhaps it is supported by numerous now partly or wholly unconscious motives, say by masses of internal bodily sensations (as in case of pathological fears or of certain physical temptations of abnormal vigor). In all such cases it may prove too strong to be controlled. Or again, the general condition of the sufferer is one of irritable weakness. The sustained coherence of normal functions is then already impaired by nervous exhaustion; the main trains of association hang weakly together; their general power of resistance, so to speak, is lowered. The intruding impulse, on the contrary, is then the mental aspect of a suggested nervous excitement that, beginning at one point, quickly spreads to others, and for the time takes possession of the functions of this unstable brain. And now, in any of these cases, we have a failure to resist the intruder, a failure which the sufferer himself bitterly feels. Objectively the failing adjustment appears as hesitation, or as useless repetition of acts, or as unaccountable impulsive 'queerness' of conduct, or even as helpless inactivity, with various quasi-melancholic symptoms,—silence, hiding, self-reproach, lamentation. Within, the sufferer, who, to suffer decidedly from this sort of malady must be a person of highly organized plans and of self-observant intelligence, feels a prodigious struggle going on. All seems to him activity, warfare, self-division, tumult.

In judging of such a case, one must therefore carefully avoid being deceived either by the imperativeness or by the quaintness of the particular impulses involved. All depends upon their relations in a man's mental life. The intense interests of the inventor, of the man of science, of the rapt public speaker, are not necessarily at all analogous to the 'obsessions' of the sufferer from insistent impulses, although the former are, like breathing, imperative. Nor are the merrily absurd impulses of a gay party of young people at a picnic abnormal, merely because they are for the time incoherent, and are thus opposed to serious thought and conduct. No, it is the *union* of a tendency towards incoherence in feeling and conduct, with an imperative resistance to the actual and conscious plans, whereby the sufferer deliberately intends to be in some chosen fashion coherent,—it is this union of incongruity with insistence that constitutes the subjective note of the morbidly insistent impulse.

These are commonplace considerations. I should not introduce them here, were not the literature of this whole topic so often affected by confusions of conception. In the light of such obvious considerations, Froude's refusal to see the abnormity of Bunyan's insistent questions or 'blasphemies' as to the being of God, and the like, becomes sufficiently insignificant as affecting our present judgment. Any man may by chance, in his mind, come momentarily to question anything. That is so far a matter of passing association, and involves nothing suspicious. A modern or, for that matter, an ancient thinker may moreover persistently question God's existence. If the thinker is a philosopher, or other theoretical inquirer, such doubts may form part of his general plans, and may so be as healthy in character as any other forms of intellectual considerateness. But if a man's whole inner life, in so far as it is coherent, is built upon a system of plans and of faiths which involve as part of themselves the steadfast principle that to doubt God's existence is horrible blasphemy, and if, nevertheless, after a fearful fit of darkness, such a man finds, amidst 'whole floods' of other 'blasphemies,' doubts about God not only suddenly forced upon him, but persistent despite his horror and his struggles, then it is vain for a trained sceptic of another age to pretend an enlightened sympathy, and to say to this agonized nervous patient: 'Doubt? Why, I have doubted God's existence too.' The ducklings can safely swim, but that does not make their conduct more congruous with the plans and the feelings of the hen. The professional doubters may normally doubt. But that does not make doubt less a malady in those who suffer from it, and strive, and cry out, but cannot get free.

This observation, that the symptomatic value of these insistent impulses lies solely in the *relation* between the impulses themselves and the organized mental life, the plans, insight, and chosen habits of the patient, reminds us also in this case that Bunyan's experiences clearly indicate the essential psychological equivalence of several of the various sorts of *manias* and *phobias* which some authors, imagining that the content rather than the relations of the impulses concerned is important, have so needlessly chosen to distinguish. Bunyan was tempted to doubt, fear, question, blaspheme, curse, swear, pray to the devil, or to do whatever else conscientious inhibition and irritably weak speech functions had prepared him to find peculiarly fascinating and horrible. There was no importance in the mere variety of the wicked ideas that the one 'tempter' suggested. The evil lay in the systematized character of the morbid habits involved, and in the exhausting multitude of the tempter's assaults.

(*To be concluded.*)

COMMUNITY AND ASSOCIATION OF IDEAS: A STATISTICAL STUDY.

BY PROFESSOR JOSEPH JASTROW,
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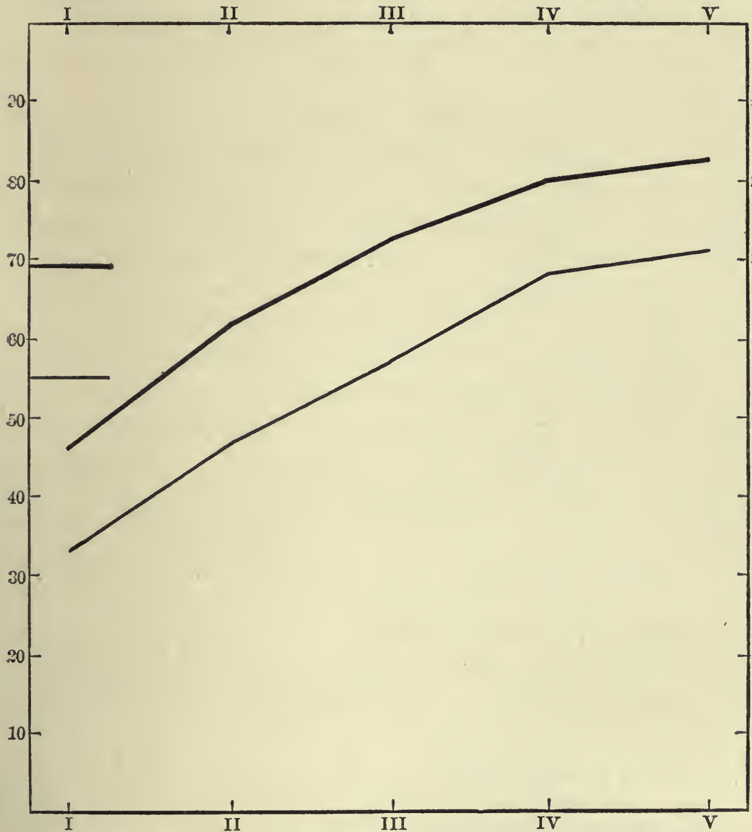
The application of statistics to the study of mental phenomena promises to supply the data for new and suggestive generalizations, as well as to corroborate, often in an unexpected manner, the laws of mind derived from off-hand observation. The census and newspaper statistics on matters large and small have familiarized us with the notion that facts which separately may have but little importance, when considered in groups give rise to significant truths. In the hope of contributing to our knowledge of the nature and regularity of such mental processes, I have upon various occasions requested a class of students to serve as the subjects of experiment.* In the test here to be described a word was written upon the blackboard and, by the withdrawal of a screen, was shown to the whole class at the same moment; each student thereupon wrote as rapidly as possible *the five words first suggested to him* by the word upon the board. In this way five associations were obtained from each student to each of the following ten words: *book, man, tree, cat, hand, hat, bread, pen, write, blue.* By counting separately for each of the five associations how often different students have written the same word we may determine the degree of similarity of their associations, and further how this community of ideas varies as the associations recede from their common starting-point. The result of this enumeration appears in the following table.

This table is based upon 69† lists of words, theoretically

* One of these tests is described in *A Study in Mental Statistics* (New Review, December 1891) and another in *A Statistical Study of Memory and Association* (Educational Review, December 1891).

† This number is too small to establish beyond the influence of chance detailed conclusions; the conclusions most firmly established and those simply suggested or made probable are indicated as they occur.

	First Word.	Second Word.	Third Word.	Fourth Word.	Fifth Word.	Total.
Number of different words ...	307	414	480	522	515	2278
Percentage of " " ...	46.2	62.1	72.7	80.1	82.8	69.8
Number of 'unique' words...	222	313	377	447	442	1801
Percentage of " " ...	33.4	47.2	57.1	68.5	71.1	55.2
Total number of words.......	665	663	660	652	622	3262



Curves showing the relation of the community of ideas to the 'distance' in associated words from the original suggesting word. I, II, III, IV, V, represent the first, second, third, fourth, and fifth association to a given word; the vertical distances to the intersections of the heavy curve with these lines when measured from below represent the percentages of *different* words, when maesured from above, the percentages of *same* words in the entire list of first, second, third, fourth, and fifth associated words respectively. The lighter curve represents the same relations for the percentages of words occurring but once in the same position on the entire lists. The short marks to the left represent the mean values of each curve.

of 50 words each, but actually containing in all 3262 words. In the first line of the table appear the number of different words written as the first, second, third, fourth, and fifth associations respectively, with their sum total; in the second line these numbers are expressed in percentages of the total number of words written, which in turn are given in the lowest line of the table. A second indication of the community of ideas may be obtained from the proportion of associations written by but one person of the entire 69; these once-occurring or 'unique' words are tabulated in precisely the same way as the number of different words and appear in the third and fourth lines of the table.

The significance of these numbers appears most clearly in a graphic presentation; we see at a glance how regularly *the proportion of different words, as also of unique or once-used words, increases as the associations proceed.* There is most community of ideas amongst the first associations to a given word, distinctly less community amongst the second associations, and progressively less, on to the fifth. The greatest difference is between the first and second associations, the differences decreasing with successive associations. The complete parallelism between the proportions of different and of unique words is also striking. The divergence of mental paths from a common centre, the appearance of individuality and disappearance of community of ideas, are thus clearly exhibited; and the result is important as well for its psychological bearings as for its testimony to the value of the statistical method in psychological investigations.*

It is always interesting to compare the mental processes of men and women. Former investigations have indicated that

* These results invite comparison with those of the former study; the percentage of different words for the first associated words is here 46.2; in the former study 34.4; of 'unique' words 33.4 and in the former study 20.0, the number of lists being the same in the two cases. This would indicate a greater amount of individuality in the contributors to this than in those to the former study. As above recorded, the same association occurring in two different positions is counted as two; if counted as one only, that is, irrespective of position, the percentage of different words becomes 45.7, and of 'unique' words 31.3; a comparison of these with the former averages, 69.8% and 55.2%, indicates that 24.1% of all the words recur in different positions and that 23.9% of words occur but once in one position, but again in other positions.

in unrestricted and extended series of associations such as the writing of the first one hundred words thought of, women repeat one another's ideas more frequently (in one result about 25% more frequently) than men. When the suggesting word was supplied no noteworthy difference between the sexes appeared, regarding the community of ideas amongst the first suggested words. In the present study two entirely different groups of nineteen men were selected by chance for comparison with the nineteen women who took part in the test. The result of the comparison appears in the accompanying table.

		1st Word.	2d Word.	3d Word.	4th Word.	5th Word.	Total.
Percentage of different words	Women......	70.4	85.8	87.4	92.1	96.5	86.2
	Men: Group A	65.0	81.5	87.4	90.7	93.5	84.5
	Men: Group B	59.7	81.8	87.6	90.8	92.9	82.5
Percentage of 'unique' words	Women......	40.3	53.0	56.3	72.5	77.8	59.6
	Men: Group A	32.8	45.7	60.1	67.0	68.0	54.5
	Men: Group B	31.2	47.6	56.4	67.0	68.5	54.1

It will be seen that in all but the third word and in both the proportion of different and of 'unique' words there is less community of associations amongst the women. This result, however, is based upon too limited data to be accepted as final, and there are indications that the associations of these nineteen women have unusually little in common.

With regard to the difference in tendency amongst the ten words to suggest the same associations, it appears, as a result of various modes of measuring them, that *blue*, *cat*, and *pen* are most apt to suggest the same words to different persons, *hand*, *book*, and *man* least apt to do so, while *tree*, *hat*, *write*, and *bread* present an average tendency in this respect.

The most frequent associations with the number of their occurrences without regard to place are: *pen—ink*, 43; *hand—finger*, 31; *blue—sky*, 30; *tree—leaf*, 29; *blue—green*, 28; *cat—dog*, 28; *pen—paper*, 27; *bread—butter*, 27; *blue—red*, 27; *man—woman*, 26; *cat—mouse*, 25; *pen—write*, 24; *write—pen*, 24.

It remains to investigate the *nature* of the associations.

For this purpose a classification followed in a former study and suggested by an analysis of the associations themselves may be adopted. (I) Whole to Part, or General to Special, such as *tree—leaf* or *tree—oak ;* (II) Part to Whole, or Special to General, as *hand—arm, blue—color ;* (III) Object to Activity, as *pen—write ;* (IV) Activity to Object, as *write—pen ;* (V) Object to Quality, as *tree—green ;* (VI) Quality to Object, as *blue—sky ;* (VII) by Natural Kind or one object suggesting another of the same class, as *cat—dog,* both being names of animals, as *bread* suggests other articles of food, *blue* other colors, and the like ; (VIII) by Similarity of Sound, as *man—can, write—height ;* (IX) Miscellaneous, including all that are ambiguous or not readily classified.

The distribution of the associations amongst the nine types appears in the table ; the percentages of each kind of association both in general and for each position separately are likewise given.

	I	II	III	IV	V	VI	VII	VIII	IX
Total......	15.6	2.8	6.2	5.2	14.9	3.3	24.4	10.9	16.7
1st assoc...	17.1	6.0	4.8	6.0	11.3	2.5	34.3	9.5	8.5
2d assoc....	18.3	2.9	6.6	5.5	12.9	3.0	28.3	9.7	12.8
3d assoc....	14.2	2.1	6.2	6.1	16.4	3.9	22.9	10.7	17.5
4th assoc...	15.2	1.2	8.1	4.4	17.8	3.6	18.8	11.9	19.0
5th assoc...	13.2	1.8	5.3	4.2	16.4	3.2	16.7	12.9	26.3

The types of associations found to be prominent in the former study are also prominent in this ; the most frequent associations are those by Natural Kind, while those from Whole to Part and from Object to Quality are also prominent. Associations in one direction may be more frequent than in the reverse direction—Whole to Part more frequent than Part to Whole, and the like.

In the change in distribution of the associations in the several positions, it is possible to distinguish certain significant tendencies; more extended data would be necessary to establish completely the relations involved. The two most regular and prominent changes are the decrease of associations by Natural Kind, and the increase of Miscellaneous associations as the associations proceed; the one decreases by regular steps from 34.3% to 16.7%, and the other increases from 8.5% to 26.3% of all the associations. Associations by Natural Kind are simple, while those termed Miscellaneous are thus indicated as variable and unusual; it appears then that the simple associations are exhausted before the more remote ones are thought of. This conclusion reinforces from a new point of view the result formulated in the general curve; the percentage of common words decreases at the same time that the nature of association varies; *it is a variation of types of association as well as of words.* Furthermore, by calculating the proportion of different as also of 'unique' words among the associations by Natural Kind without regard to position, we find only 25.6% and 10.5%, as against 45.7% and 31.3% for all associations in general—certainly a marked contrast.

The next table furnishes the data for comparing the distribution of the various kinds of association among men and women; it is formed just as was the former table, but the smallness of the numbers emphasizes the necessity of great caution in drawing deductions. Masculine preferences appear to be for associations by Sound, from Whole to Part, from Object to Activity from Activity to Object, and also for those by Natural Kind. Feminine preferences are for associations from Part to Whole, from Object to Quality, Quality to Object, and Miscellaneous. These differences are in general in accord with those formerly established and may be brought into relation with recognized differences in the mental processes of men and women.

A word should be added regarding the method of classifying and counting these associations. Inasmuch as no restrictions were imposed upon the words to be written it was left open whether the five words should all be associated with the original word, or the second word be suggested by the first, the third by the second, and so on, with little or no thought of the original word. After due consideration the former plan

		I	II	III	IV	V	VI	VII	VIII	IX
Total	M	16.7	2.6	6.6	5.4	13.6	3.0	26.0	12.4	13.7
	F	12.7	3.5	5.2	4.8	18.2	4.0	20.2	7.0	24.4
1st assoc.	M	16.4	5.8	4.9	6.0	9.3	2.5	36.3	11.4	7.4
	F	18.8	6.5	4.3	5.9	16.2	3.2	29.0	4.8	11.3
2d assoc.	M	19.1	2.2	6.6	5.5	11.1	3.0	30.4	11.7	10.4
	F	16.2	4.9	6.4	5.4	17.7	3.2	23.1	4.9	18.2
3d assoc.	M	16.4	2.3	6.7	5.7	14.2	4.4	23.5	12.8	14.0
	F	8.7	1.6	4.9	7.1	21.8	2.7	21.2	5.4	26.6
4th assoc.	M	17.1	0.9	9.3	5.2	17.3	3.0	20.1	12.6	14.5
	F	10.1	2.2	5.1	2.2	19.1	5.1	15.2	10.1	30.9
5th assoc.	M	14.7	1.6	5.6	4.4	16.4	2.3	18.7	13.6	22.7
	F	9.3	2.3	4.0	3.5	16.3	5.8	11.7	10.5	36.0

was adopted as following more closely than the other the natural order of thought. A large majority of associations are either clearly associated with the original word, or are capable of either interpretation; the thought rarely wandering entirely or far away from the original word. It is estimated that in not more than five per cent of the words is the association clearly with the preceding and *not* with the original word. Typical instances of such associations are: *Bread, cow, milk, pitcher, crockery; Write, letter, home, brother, vacation, school; Cat, mouse, trap, cheese, poison, death.* With more than five associations to each word it would undoubtedly be necessary to recognize more completely this difference in the nature of the associations, whether serial like the links of a chain, or radiating like the spokes of a wheel.

ON REACTION-TIMES AND THE VELOCITY OF THE NERVOUS IMPULSE.*

BY PROFESSOR CHARLES S. DOLLEY, M. D.,
Philadelphia,
AND
PROFESSOR J. McKEEN CATTELL, PH. D.,
Columbia College, New York.

Our object was to determine the conditions which affect the length of reaction-times on dermal stimuli, and to study the application of the reaction-time to the measurement of the velocity of the nervous impulse in motor and sensory nerves, and in the motor and sensory tracts of the spinal cord.

Sec. 1. *Apparatus and Methods.*—In order to measure a reaction-time at least three instruments are required—one to give the stimulus and record the instant at which it is given, one to record the instant at which a movement is made, and one to measure the intervening time. We used the Hipp electric chronoscope for measuring the time, having made several improvements in its construction and regulation. With our apparatus we could measure the time of a reaction with a variable error of about 1σ (i.e. one thousandth of a sec.), and a constant error of about the same size. The variable error is practically eliminated in the average of 100 measurements, and the constant error is practically eliminated when a difference is taken. We used various methods to apply an electric shock or a blow to the skin. In the case of a blow we were able to measure exactly its force. The greater part of the apparatus was secured through an appropriation from the Bache Fund, and is the property of the National Academy of Sciences.

* Abstract of a paper presented before the meeting of the National Academy of Sciences, Albany, 1893. The paper with description of apparatus, tabulated results, and historical references will be printed in the *Memoirs of the Academy*.

We made 10 reactions in a series and 10 series in a set, each time given in the Tables being the average of 100 reactions. We give the mean variation of the separate reactions and of the separate series and probable errors when it seems needful. The mean variation of the separate reactions is on the average about 10σ, and the probable error of the figures given in the Tables would be about 1σ. Although we omitted no measurements in calculating the averages, the probable errors are much smaller than in previous investigations on reaction-times. In some cases we made 10 reactions in succession at intervals of 2 sec., and only measured the resultant time. By this method more reactions can be made in a given time, but the probable error of the single reactions is not known.

The experiments were begun in 1889 in the psychological laboratory of the University of Pennsylvania and completed in 1893 at Columbia College. The experiments were made by D and C (the writers) and J, all of whom were used to scientific work.

Sec. 2. *Reactions on Electrical Stimuli.*—An electric shock can be applied conveniently to different parts of the body. The shock may be made as strong as desired and the moment of its occurrence registered. We used electrodes of various sorts. The method we found best was to apply one electrode (usually a platinum surface 10 mm in diameter) to the points on the skin which we wished to stimulate, while the other electrode was conducted to a pail of saturated salt water in which the left foot was placed. The stimulus was given 10 times in succession at the same point, and then immediately switched to another point without shifting the electrodes The shock was usually given on the left-hand side of the body, the reaction being made with the right hand or foot.

The sensory effects of electrical stimulation of the skin have not been properly investigated, and this is the more curious as the effects on the organs of sight, hearing, taste, and smell have been thoroughly studied. With a galvanic current from 28 cells in pairs we found a decided difference in sensation according to the pole with which the electrode was connected. Thus when the positive pole was applied to the outside of the upper lip (the other pole being conducted to the foot) there was a prickling sensation, a strong taste and a flash of light, the prickling and taste continued while the current was closed and there was a flash of light on breaking. When the negative pole was applied to the same point there was a slight shock and flash of light and no taste, but sensations of piercing and boring followed which were unendurably painful. The current caused tetanus of the muscle and left blisters. This experiment shows that the current from the negative pole was more intense and from the positive pole more diffused (extending with much energy to the organs of taste and sight), which indicates that the current passes through the body from negative to positive pole.

We used in our experiments the momentary shock following breaking of the primary circuit. The same objective current does not give a shock of the same subjective quality and intensity when applied to different parts of the body. The sensation is more piercing when the electrode is applied close to the nerve, and more massive (as from a blow) when there is muscle intervening. The sensation of a shock from 8 cells on the upper arm might be as intense as that from 28 cells on the wrist. The shock from the same current also varied with the pressure of the electrode and especially with the moisture of the skin. Further as the experiments proceeded the part of the skin to which the shock was applied became continually more sensitive. The shock is more piercing from a small electrode and more massive from a large electrode.

We have made so many experiments (more than twenty-four thousand reactions) and with such numerous variations in the place of applying the stimulus, and in the nature of the stimulus and of the movement, that we fear an abstract of our results will be somewhat confusing. The details could only be properly understood from the Tables, and a study of these will only be undertaken by those especially interested in research in this direction.

In our first experiments we chose four points on the skin for the application of the stimulus. These were permanently fixed by pricking the skin and introducing nitrate of silver.

Two of the points were on the arm over the median nerve, and two on the leg over the posterior tibial nerve. The points on the arm were 30 cm apart and those on the leg 50 cm apart, and the length of intervening nerve would be nearly the same. To stimulation of these points 5360 reactions were made, the movement being made in two thirds of the cases with the hand and in one third with the foot.

The reaction-time was the shortest when the stimulus was applied to the upper arm and the movement made with the hand. The time was 149.6σ for D and 113.1σ for C, about $\frac{1}{7}$ and $\frac{1}{9}$ sec. respectively. Such a personal difference must be due either to differences in the nature of the process or to differences in the sensitiveness of the parts of the nervous system concerned. The variation from time to time is much greater than can be due to chance variations, and the differences must measure secular changes in the nervous system.

When the stimulus was applied to the lower arm or on the leg the reaction-time was longer, the excess of time for the lower arm being 14.2σ for D and 6.9σ for C. The lower point was 30 cm further from the brain than the upper point, and if we could assume the difference in time to be due to the difference in length of the nerve travelled we should have a velocity of the impulse in the sensory nerve of 21.1 m per sec. for D and 49.5 m per sec. for C. The velocity in the sensory fibres of the posterior tibial nerve would be 31.1 m per sec. for D and 64.9 m per sec. for C. We are not, however, prepared to accept these velocities as valid. The differences in the time of reaction are undoubtedly correct, the variable error being practically eliminated. But the variation of the two observers and of the same observer at different times are so considerable, that we think these must be attributed to differences in the cerebral processes rather than to differences in the velocity of the impulse in the sensory nerve. The shorter reactions on the upper point might be due to other causes than nearness to the brain, such as to greater intensity.

When the shock was applied to the leg in one case, and to the arm in the other, the impulse in the former case had in addition to travel through the spinal cord from the lumbar to the brachial plexus, and the times are considerably longer. The differences when the movement was made with the hand were 26σ for D and 27.1σ for C. When, however, the movement was made with the foot the differences were 18.4σ for D and 18.7σ for C. The excess of distance in the spinal cord was the same as before, but the times were about 8σ shorter. We thus show that when the stimulus is applied to the left leg the cerebral reflex is relatively shorter to the right foot, and when applied to the left arm is relatively shorter to the right hand. The sensory fibres from one part of the body are most closely connected with the motor fibres to the same part. If we may assume that the cerebral reflex occupied the same time in the cases compared we should have a velocity in the motor tracts of the spinal cord of about 15 m per sec., and we may at least assume that the velocity is not less than this.

When the movement was made with the foot in one case and with the hand in the other, the stimulus being applied to the arm, the excess of time with the foot was 37.7σ for D and 54.4σ for C. When the stimulus was applied to the leg the differences were less (8.5σ for D and 9.4σ for C). We have thus again measured the difference in time of the cerebral reflex when the motor impulse proceeds to the part of the body from which the sensory impulse arrives and when it proceeds to a different part.

The difference in the time when the reaction is made with the hand and foot, respectively, is partly due to the time required to traverse the motor tracts of the spinal cord, but it may also be due to differences in the cerebral processes. The cerebral reflex is undoubtedly less perfect to the foot than to the hand. The difference in the case of the two observers is almost certainly a difference in the cerebral process. C's reaction with the hand is very automatic, with the foot it is more nearly like D's. If the whole excess of time in the case of D were consumed in traversing the motor tracts of the cord between the brachial and lumbar plexus we should have a velocity of about 10 m per sec. In so far as we can accept these results, the velocity in the sensory tracts of the cord would be greater than in the motor tracts, and this could be explained by a partial co-ordination of the movement in the cord.

As the differences found might be due to varying intensity of the shock or differences in the cerebral reflex, we made a large number of reactions (10,400) with a view to studying these factors. After some diverse experiments which will be found in our paper, we chose four points on the arm, and in some of the experiments used three intensities of shock. Two of the points were the same as before, but we chose an additional point on the lower arm near the wrist the stimulation of which was followed by muscular contractions and a massive sensation, and a point on the middle arm the stimulation of which was followed by a prickling or piercing sensation. We had thus two points on the lower arm close together, the stimulation of one giving a strong and massive sensation, the stimulation of the other giving a weaker and more prickling sensation. On the mid-arm the stimulation was followed by a prickling sensation, on the upper arm by a massive sensation. As a result of our experiments we found that the time of reaction was shorter when the intensity of sensation was greater, whether the increase in intensity were due to a stronger objective shock or to greater physiological effect on different points from the same shock. Thus as the final result of 1200 reactions on each point (made by J and C), we found the time on the lower point giving the massive sensation to be 126.6σ, and on the upper point giving a similar sensation to be 126.8σ, almost exactly the same. We must therefore conclude that the time of transmission in the nerve was exactly counterbalanced by a shorter cerebral reflex in the case of the point further from the brain. In the case of C the subjective intensity of the shock was greatest on the upper point and the final time was 2.2σ shorter on this point. In the case of J the subjective sensation was stronger on the lower point and the time was 2.4σ shorter. These differences are so small that the velocity of the impulse was not counterbalanced by any difference in intensity in favor of the lower point, but we conclude that the cerebral reflex is shorter when the stimulus is applied to a point near the hand, the movement being made with the hand on the opposite side of the body. The sensory fibres from one part of the body are most closely connected with the motor fibres to a corresponding part.

When the same stimulus was applied to two points on the arm nearly equidistant from the brain but accompanied by different sensations, the reaction-time was 10.2σ shorter on the point for which the sensation was the stronger. When it was applied to the point on the lower arm and to the point on the mid-arm giving nearly the same sensation, the times differed by only 0.6σ.

When three intensities of shock were used, the time of reaction was on the average 19.6σ shorter for a very strong shock than for a medium shock, and 9.8σ shorter for the medium shock than for the weak shock. But the physiological intensities of the shocks could not be measured. The reaction times were nearly the same for the two observers (J, 127.4σ and C, 128.3σ) when the shock was strong, but were longer for C when the shocks were very weak.

When the size of the electrode was altered the difference in the length of the reaction-time seemed due to the intensity only. The time was also nearly the same when the shock was applied from electrodes 5 cm apart, when the current was sent through the limb, and when it was sent through the entire body.

When the attention was directed alternately to the stimulus and to the movement no difference in reaction-time was found for J and C, but D's reaction-time was longer when the attention was directed to the movement, he being in the habit of attending to the stimulus. When the reaction-time is short and regular the amount and direction of the attention does not materially affect the time of reaction; but when the process is less reflex, it may be lengthened by an unusual direction of attention.

The reaction-time was shorter (J, 18.9σ, C, 14.2σ) when the shock is applied to the hand with which the movement is made than when it is applied to the other hand. This might be expected, as the movement is a natural reflex—a person will without reflection withdraw the hand when it touches a hot object. The fact is of interest in connection with the results already noticed, which show that the cerebral reflex is in general quicker when the sensory fibres stimulated are from the same part of the body as that with which the movement is made.

The reaction-time is longer when the movement is made from the shoulder, and nearly the same when it is made from the fore-arm, wrist, or finger. This shows that when the movement is made with the foot the delay may be partly due to a more difficult co-ordination in the higher centres. The reaction-time is longer with the organs of speech than with the hand, but it is the same for the right and left hands.

Sec. 3. *Reactions on Touch.*—In the case of reaction experiments with dermal stimuli the electric shock has mostly been used, as it is easy to apply the shock to different parts of the body. We have, however, seen that the physiological effects of the shock vary greatly on different parts of the body, and even at the same point they cannot be kept constant. We have found that the same objective force of blow is followed by the same subjective sensation more nearly than in the case of electrical stimulation. On different parts of the body the same blow, indeed, calls forth different sensations, the sensations being more intense when the part is hard, as over a bone. But the difference is not so great as in the case of the electric shock, and at the same point the same sensation can be given time after time and day after day. The probable error is consequently smaller than in the case of the electric shock; indeed, the variable error in our experiments on touch is much smaller than in any reaction-time experiments hitherto published.

We used various methods for applying a touch or blow, but in this abstract we need only describe the method which we find best. We allowed a hammer to fall from a fixed height, the current controlling the chronoscope being closed when the blow was given. The weight of the hammer and the height from which it fell could be altered, as also the area with which the blow was given. A blow of a given force could consequently be given time after time, and the force and area of the blow could be varied when desired. We made reactions in answer to blows on different parts of the body, the hammer weighing 30 g and falling 20 cm. The area which gave the blow was circular, 1 cm across.

The reaction-time with a blow was about 10σ shorter than with an electric shock. It was nearly the same for J and C, and about 30σ longer for D. Thus when the blow was given on the arm the reaction-time was (as the result of 600 experiments with each observer) 113.9σ for C and 114.7σ for J. When the blow was given on the thigh the time was 148.5σ for D and 121.5σ for C.

In some of the experiments we used three intensities of stimulus, the hammer weighing 15, 30, or 60 g and falling 20 cm. The blow from the 60 g weight was nearly painful on a hard part of the body, and the blow from 15 g was quite strong. There was not much difference in intensity of sensation and the decrease in reaction-time was not great. As a result of all the experiments (2400) the time was decreased 1.3σ when the weight was increased from 15 to 30 g, and was decreased 1.7σ when the weight was increased from 30 to 60 g. If, as Fechner's law assumes, the intensity of a sensation increase as the logarithm of the stimulus, the reaction-time would tend to vary inversely as the intensity of sensation.

The shortest reaction-time followed stimulation of the finger or cheek in the case of C (105.8σ for the finger and 103.1σ for the cheek). The reaction-time on stimulation of the finger was thus about 8σ shorter than on stimulation of the arm. As in the case of electrical stimulation we find that the cerebral reflex is shortened when the stimulus is applied to the opposite side of the body to a point corresponding to that with which the movement is made. The time was about 1σ shorter for the toe than for the thigh. As the average of two observers, the times for the neck and cheek were nearly the same.

The time was 7.8σ shorter when the stimulus was applied to the arm than when it was applied to the thigh. If this difference be due to the time of transmission from the lumbar to the brachial plexus, we should have a velocity of the nervous impulse in the sensory tracts of the spinal cord of about 40 m per sec. The times on the upper and lower arm and on the upper and lower thigh, respectively, were the same. In the case of the arm the time for the upper point was 0.2σ shorter for J and 0.6σ longer for C. In the case of the thigh the time for the upper point was 1.1σ longer for D, and 0.1σ shorter for C. The difference in the length of nerve traversed is not counterbalanced by a difference in intensity. Doubling the stimulus shortens the reaction by only 1.5σ, and the differences in sensation were not so great on the different points as on the same point when the stimulus was doubled. When the stimulus is applied to a point further from the brain the time of transmission in the nerve seems to be exactly counterbalanced by a shorter cerebral time, the sensory fibres from a point nearer the extremities discharging more quickly into motor fibres to the extremities.

We do not think that the velocity in the plain nerve can at the present time be determined by differences in the reaction-time. But equal difficulties are present when the motor nerve is stimulated electrically. It would seem that the velocity of the impulse in the nerve can only be measured when we are able to record its progress, perhaps by electrical or chemical changes. But we hope our work has thrown some light on the subject, especially in the case of the motor and sensory tracts of the spinal cord. We think, further, that a general survey of our experiments indicates a rate in the plain nerve much greater than that commonly accepted of 30 m per sec.

We think that the study of the reaction-time is itself important both for physiology and psychology. These sciences cannot rank co-ordinate with the physical sciences until they consist of exact measurements. Experiments on the reaction-time have been of use in the analysis of physiological and mental processes and in studying the relations of these. The reaction-time has also certain practical applications in pedagogy and medicine. Thus experiments such as these indicate important personal differences, or they may be used to locate exactly the place of disease in the nervous system.

DISCUSSION.

COLOR-SENSATION THEORY.

I find myself much indebted to Dr. Sanford for his admirably lucid exposition of my color theory, but truthfulness compels me to confess that he attributes to me a degree of modesty to which I can lay no claim. The way in which I regard my own theory is perhaps rather difficult to seize: Dr. Sanford has quite misconceived the meaning of what I say concerning my attitude towards it. He says: "The author's theory, which is regarded, indeed, rather as an indication of the direction that such a theory should take than as a developed scheme, is," etc. Now I neither regard my theory as an indication of the *direction* that such a theory should take, nor as an undeveloped scheme. I do, on the contrary, regard it as (1) *a perfectly adequate* theory, that is, as a theory which actually does that which a theory of the phenomena in question is required to do; but, on account of the purely hypothetical character of the conception by which it accomplishes this (partial decomposition is one of the most familiar things in chemistry, but we do not *know* that it takes place in the retina), there is no reason to suppose that there may not be other conceptions possible which would fulfil the requirements equally well. In other words, I look upon it as a member of a *class* of theories, all satisfactory, and all, with our present knowledge, unprovable, but of a class which at this moment is composed of a single member only. The one great requirement of a color-sensation theory—and it is an immensely difficult one—is that it account for the absolutely unique fact in sensation that certain sensation-pairs (viz., any two complementary colors) lose themselves in a totally different sensation, and that other sensation-pairs, undistinguishable from these objectively, do nothing of the sort. Helmholtz explains this characteristic of color-vision by ignoring it. Hering gives a perfectly adequate explanation of it, but by means of a conception which has no parallel among physiological doctrines. My conception, I find myself obliged to think, is both physiologically unobjectionable, and capable of furnishing an easy explanation of the critical facts of color-vision.* Of course, I am not saying that a good explanation of the most important fact of color-vision is *sufficient* for a theory, but only that it is *necessary*. The smallest fact discoverable, if it were incompatible with a theory, would suffice to overthrow it. Now any number of other members of this class of 'adequate' theories may spring up at any moment, but there is no reason to suppose that they would be at all similar to my theory, or at all in the same direction with it. It is much more likely that they would make use of totally different conceptions—derived from the phenomena of photo-electricity or of polarization of light, or even from something that would not be a *vera causa* at all, and not simply not a *vera causa in situ*.

Neither do I regard my theory (2) as an undeveloped scheme. It is true that the fictitious process in the retina to which I attribute the transformation of light into some sort of nervous excitation is an excessively simple one, but on the other hand it is quite sufficiently complicated for doing the work which is required of it; and any degree of complexity or development in a purely hypothetical scheme, beyond what is absolutely required in order that it may fulfil the function demanded of it, has no other effect than to render the theory needlessly cumbrous. The carrying out of the work of translating all the facts of color-vision into the terms of the theory will, I hope, follow in course of time,—unless, indeed, it should receive such hard blows as to lay it low before that time shall come.

But the sentence of my reviewer's to which I particularly take exception is this. He says: "As the author herself observes, if Hillebrand's demonstration of Hering's theories of complementary colors stands, it is fatal to her theory, at least in its present form." It would, indeed, be fatal to my theory, in any possible form, if Hillebrand's supposed proof that white-producing colors are antagonistic, and not complementary, were valid. (This is a very different thing, by the way, from a proof of Hering's theories; there may be many other antagonistic color theories, and there is one—that of Ebbinghaus.) Now it is open to my reviewer to say that it is matter of doubt whether Hillebrand's proof is valid or not, but to say that I say so is again to attribute to me a degree of modesty which I must hasten to disclaim. What I did say was, "*Were* Hillebrand's proof valid," etc., and I go on to say that I believe it to be thoroughly fallacious. My reviewer may consider that my objections to Hillebrand's argument do not hold, but to attribute to me the opinion that it is matter of even

* I am naturally very much gratified at finding that so acute a physiologist as Prof. Burdon-Sanderson has also expressed himself as of this opinion. *Nature*, vol. 48, p. 469.

doubt which way the truth may lie, on account of the slightly rhetorical way in which I have expressed myself, is hardly fair. To inflict upon the world a theory which was in my own opinion so near as that to being upset beforehand would be a highly reprehensible proceeding and one which I should not like to be guilty of.

I mention briefly (1) that I consider Dr. Sanford's objection to my explanation of simultaneous contrast to be well taken, and that I shall hereafter attribute that phenomenon to a purposeful reflex action, which is also Hering's explanation of it, at bottom ; and (2) that the sensation of black is accounted for, upon my hypothesis, as the effect on the nerve-ends of the resting condition of the photo-chemical substance ; it is therefore the antithesis to every color as well as to white, and it is the constant background against which all colors and white are seen. This is in harmony with the fact that, while Hering's *Lichthof* must play an important rôle among the phenomena of vision *there is no such thing as a Dunkelhof.* But this subject needs to be set forth at greater length. One other point I touch upon,—experiments upon the color wheel are inconclusive when their results are of a negative character, but only then. If a red-green gray and a blue-yellow gray, when made out of colored papers, act alike, it cannot be inferred that there is no difference between them, merely because the things sought to be compared not have been obtained with much purity ; but if they act differently, it is another matter. A difference in the properties of two things can only be due to a difference in their constitution, and the fact that they are not as purely different as they might be does nothing to invalidate this conclusion. Moreover, the fact that two differently constituted grays vary unequally with changing illumination is, as I have mentioned, a deduction from König's exceedingly exact measurements of the Purkinje phenomenon. Hering attempts to account for it by a very rapid variation in the amount of coloring matter in the yellow spot under different illuminations. This would seem to be as inadequate to explain the large amount of difference which is observable as is his scheme for explaining the difference between the red-blind and the green-blind by differences in the amount of yellow substance in the retina of different individuals. That the phenomenon should cease to be detected at some little distance from the fovea is not strange in view of the very rapid falling off of the color sense on leaving the fovea, as exhibited in the curves laid down by Eugen Fick (*Pflüger's Archiv*, Bd. XLIV).

CHRISTINE LADD FRANKLIN.

BALTIMORE.

HERR LASSWITZ ON ENERGY AND EPISTEMOLOGY.

Herr Lasswitz draws two conclusions in his epistemological study of the modern theory of energy (see notice of articles under Psychological Literature) which have a psychological value. The first is that the substitution of energy for mass in formulating the equations between the so-called 'forces of nature' obviates the fallacy which follows a too common interpretation of physics, that its fundamental unit is expressed in terms of the sensation of touch. A molecule or atom is of course as far beyond the possibility of a sensation of touch as it is beyond one of sight. Nor are we justified in carrying beyond the limits of perception, by magnifying in imagination, those sensations which have followed the exact determinations of the phenomena of nature down to this threshold; for these very determinations demonstrate that below this point the balancing of energies between spatial objects, among which is the human organism, does not allow of a reading into terms of any sensation.

We are forced by this into the conclusion that physics in so far as it involves determinations which run out to the indefinitely small must always abstract from the quality of the sensation, and leave simply the *rational* statement, in terms of magnitude, of the dependence of the processes of nature upon each other.

We wish to draw one important corollary from this proposition. The impossibility of dividing the sensation up into equal parts—of dividing one sensation up into the several sensations which should compose it—has been already maintained with sufficient emphasis in criticism of Fechner's Psycho-physical Law, but the essential difference which lies between the methods of the natural sciences and those of psychology expressed in the threshold values of the various sensations (that is, the absolute beginnings of sensations contrasted with the possibility of reducing the stimulus to an indefinitely small magnitude) has not been sufficiently insisted upon.

The difference is perhaps most succinctly brought out by the contrast between the quality in sensuous experience and the same in physics. The first is static in its value—that out of which a naïve dualism constructs matter. The second is but the law in the midst of change. It is only by the infinitesimal concept that the physicist is able to express color, sound, resistance (touch), etc. The problem of the physicist is to make a magnitude out of change—by means of the mathematical formula. He has, in expressing color, only a periodicity in a process of radiation upon which to base the static element that makes up the content of most objects of perception.

The problem of the experimental psychologist is just the opposite. It is to find a change that may be expressed in law, in a magnitude which cannot be broken up. Sensation, emotion, feeling are magnitudes which within themselves suffer no disintegration, can be broken up into no parts. The psycho-physicist has hit therefore upon the ingenious expedient of paralleling the infinitesimal state in the physical phenomena by the just observable differences in the psychical. The results of this method of research have, however, but poorly rewarded the remarkable ingenuity expended upon it. And a moment's thought will reveal the inefficiency of this tool borrowed from the physical sciences.

The validity of the infinitesimal statement lies in its definition in terms of the law. The process is but arrested to assert the relations which lie between its different moments. How different the just observable difference in psycho-physics! Here there is no law in which to define the state, but we are confronted by a series of equations in which $a = b$, and $b = c$, but in which $a \lesseqgtr c$.* Instead of defining the arrested process in terms of its formula, psycho-physics has striven to formulate a law out of a series of states that can be defined no more nearly than in the assertion that they are different. How futile would be the attempt to build up the law of the circle out of successive relations of the co-ordinates, these not being defined in terms of the angles made with a vanishing secant, but only in the assertion that the successive co-ordinates are just perceptibly different from each other! Measure these co-ordinates as exactly as one may, and he has still no content in the successive states and can reach such a content only when he can define the relations of the co-ordinates absolutely by means of the law of the curve.

In a word, the physicist has abstracted the entire mathematically statable content of the sensation—and only this; and for the psycho-physicist to strive to use that which is left for the same purpose is to make it evident that he does not comprehend the relations of the two fields. We trust with Herr Lasswitz that the substitution of energy for mass in the physicist's statement will carry home the nature of the scientist's abstraction.

The unity which underlies the physical object or system (*Gebilde*) is thus reduced to a rational categorization which is the framework of consciousness—a framework which is given and of which the unity of the individual consciousness is but an expression. This is the sec-

* Natorp's *Einleitung in die Psychologie*, p. 84.

ond of Herr Lasswitz's propositions, and he draws from it the solution of the problem of the relation of the unity of the object to the unity of the individual consciousness. It is really only a deduction from the first proposition. Granted that the unity of the full sensuous object is that of the individual consciousness, the categorization in terms of space, time, and energy abstracted from this must fall under the same unity.

An important corollary to this, which also fills out the criticism passed upon the method of psycho-physics, is to be found in the continuity of the methods of exact science even if carried up to the full psychical phenomenon.

Professor Baldwin has identified Kant's transcendental unity of consciousness with attention. If we now define attention as the domination of any one act over all tendencies to action within the organism at any one time, and define the object as a group of activities co-ordinated with reference to some particular act,* we have a psycho-physical fact which can be stated in terms of the food-process by the biologist and of the compensation of the intensities of energy by the physicist. For an object which is a co-ordinated group of activities must have developed in the process of evolution under the same law which governed the development of the whole organism. This can be stated in terms of a food-process—involving the assimilating of food, its expenditure in motion which brings the organism in contact with new food, a negative reaction upon a non-nutritious or dangerous environment, and an overflow in reproduction. The same process must serve as a formula for the development of the psychological object, constructed as it is out of the activities which the search for food in an increasingly complicated environment has called out.

But the more abstract laws of physics are no less applicable than those of evolution to objects so constructed. A single instance may be found in the parallel set of relations of the various energies of light, sound, etc., to that of mass ; of the sense-organs of color, tone, etc., to that of touch ; of the sensations of color, sound, etc., to those of contact.

These may be read into a relation between an energy acting continuously along a single line and periodical energies of radiation acting along innumerable lines. Whether these latter may be expressed in terms of vibrations of particles of a ponderable medium and so be reduced to mass or not, the ideal of physics will hardly be reached

* Professor James has shown that our object changes completely with the activity which it represents, that a paper which presents a surface for the pen is quite a different object from paper used for kindling a fire.

before they are reduced to some single form of energy. We shall have something strictly analogous to the relation between direct action and action through media. The assumption that all senses may be reduced to that of touch or contact has long been practically unquestioned. And here we have the relation between sense-organs that receive direct continuous contact and those that respond to radiating periodical stimuli, serving as before to express immediate relation and that through media; or, in other words, the relation between physiological reactions upon direct contacts and those upon medial contacts which serve to make the direct contacts possible. Lastly we have the sensations of color and sound which have a character symbolical with reference to touch, and the activities which they call forth mediating the more important activities which are called forth by the stimuli of contact.

If, now, the psychological object be formed out of full sensuous activities responding to stimuli which affect the different sense-organs; if these reactions take place in a system built up by action upon its environment, and if the relations of its environment to it are expressed in the fundamental laws of energy, evidently those reactions and those objects must be functions of the laws of physics. A statement of the object in terms of our activities enables us to apply the laws of the exact sciences to their development and relative values.

George H. Mead.

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PSYCHOLOGICAL LITERATURE.

GENERAL.

A Theory of Development and Heredity. HENRY B. ORR, Professor at the Tulane University of Louisiana. Macmillan & Co., New York and London, 1893. Pp. ix + 255.

This very interesting and suggestive little volume by Professor Orr follows Professor Brooks' 'The Law of Heredity' of 1883, as the second work upon this subject which has appeared in this country. Professor Brooks' theory was an expansion and modification of Darwin's provisional hypothesis of Pangenesis, and was truly a theory both of Development and of Heredity. Professor Orr's work is an original modification of the speculations of Lamarck, Spencer, Butler, Haeckel, and others, and is a theory of Development based upon the psychic properties of living matter. The reviewer is unable to see wherein the author has advanced a theory of Heredity proper.

Lamarck ignored embryology in his work of 1802, but it is clear that a modern theory of Heredity must take into account the facts of embryology, and recognize that new individuals arise, not from all parts of the body, but from extremely circumscribed germinal cells. These cells are set apart at the outset of the life of the individual, and become, so far as we can see, among the most *isolated* in the body; yet they contain the whole store of Heredity. It is further clear that a Lamarckian theory of Heredity must connect the activities of the body in some way with the activities of these cells. Otherwise these activities can in no way be impressed upon the second generation. Professor Orr passes over this supremely difficult point in the transmission of somatogenic or acquired characters as if it were of no consequence, but it is one which Lamarckians cannot evade. From the fundamental principle of elementary nervousness running through this book, the reviewer expected that the author would seek to connect his psychic development of the individual with the hereditary germinal substance through the nervous system, as this system is essentially the material vehicle of the psychic side of organic matter, but he does not do so. The investigations of Golgi, Cajal, and Retzius have revealed a nerve-supply of all parts of the body vastly greater in extent than we ever supposed. Every tissue is permeated with an infinite network of the finest nerve-fibrils. Retzius has especially investigated the nerve-endings in the region of the germinal cells, but thus far his preparations show that these cells have rather an under than an over supply of nerve-fibrillæ. The sympathetic connections of various parts of the body through the nervous system are so remarkable that we can conceive that this system may be the medium of connecting the changes in the body with the potential activities of the germinal cells. Yet, at present, we must admit that in all the activities of the body there is nothing analogous to that which would be requisite for a nervous theory of Heredity, namely, that certain specific changes in certain parts of the body should affect the hereditary germinal substance in such a manner as to cause similar changes to appear in the offspring.

The strength of this work lies, therefore, upon the side of individual development; and, as it is a psychic theory, it appears to be especially timely in connection with the present monistic development of biological thought as expressed by such writers as Lloyd Morgan, Romanes, and Haeckel. The gist of the argument is that, starting with elementary nervousness as one of the fundamental properties, the effects of the reactions of living matter to environment are analogous to those seen under the laws of repetition and association in mental phenomena; that stability of function and structure bears a direct ratio to the frequency of reaction, and that all functions are built up in association with others. The volume opens with a general statement of the problems of evolution raised by Weismann, and throughout there are scattered some very forcible arguments against the theory of evolution exclusively by the survival of fortuitous favorable variations. The standpoint of the author is with Lamarck and Spencer, that the reactions of the individual to environment is by transmission, the main factor of evolution. Nervous conductivity in animals, and some similar power in plants, connects the organism with its environment. We follow the author step by step in his long and very ingenious argument based upon the analogy between mental processes and the slow steps by which living matter gradually acquires its more and more complex characters. As the mind gains ease, frequency and fixation of certain processes by frequent repetition, so in development characters become stable according to the period of time in which they have been performed. Thus the strong 'hereditary impulse' is built up. The application of this analogy to the simple life-processes of the single-celled organisms, of the two-layered organisms such as Hydra, and of the highly compound organisms, is carried out with a great deal of force in the sixth and seventh chapters. On the mechanical side of development and the origin of variations Professor Orr gives a rather naïve treatment of the kinetogenesis theory as applied to the skeleton (page 158), not referring in any way to the researches of Hyatt, Ryder, Cope, Arbuthnot, Lane, and others, which have covered this subject in such detail, the evolution of the complex surfaces of joints and relations of these surfaces to certain muscles and tendons being one of the favorite subjects with the Neo-Lamarckians. He refers degeneration wholly to the inherited effects of non-reaction, and points out in the overfed parasitic animals, where the economy of growth principle cannot operate, that degeneration is quite as rapid as in the struggling cave-forms of life. The phenomena of correlation of growth, of dimorphism and polymorphism in species, of alternation of generations, of the origin and significance of sex, are all treated in a thoroughly interesting manner.

The question remains, in conclusion, whether or not this contribution of Professor Orr's advances the great biological question of the day; whether the application of the laws of psychology to all the phenomena of life is 'a real advance in the right direction,' as the author modestly expresses it in his preface. Is this application merely based upon analogies or upon a real similarity? This is the critical point. Real similarity necessitates one of two conclusions: either that all the activities of the organism—contractile, secretory, metabolic, reproductive, and mechanical—have two sides, the material side and the psychic side, and it is only in the irritable and automatic activities that the material side is subordinate and the psychic side predominates; or, second, that the elementary nervousness, expressed in the nervous systems of higher forms, is the dominating factor in evolution. This second conclusion lands us in the difficulty which we have seen at the outset—that if the author's theory of evolution is the correct one, the relation between the nervous system and the hereditary germinal substance should be very conspicuous, whereas we find at present that it appears to be rather obscure. The prior conclusion forces us to assume that besides the nervous system the psychic side of the activities of life are carried on by some mechanism or unifying principle in organisms at present unknown to us. If there should in future prove to be such an unknown unifying principle, Professor Orr will have made a substantial advance in the right direction. HENRY F. OSBORN.

COLUMBIA COLLEGE.

Elements of Psychology. By JAMES MARK BALDWIN. New York, Henry Holt & Co., 1893. 12mo. pp. xvi + 372.

The aim of the author of this manual is to present the newest essentials of the science in a single compact volume. The book is a condensation of his well-known *Handbook of Psychology* (2 vols., 8vo, same publishers). He has aimed to simplify the exposition throughout by omissions, by recasting whole chapters, and by adding illustrative facts and explanations.

The Introduction (pp. 1–55) consists of four chapters. The first requisite of a science is the recognition of a distinct group of facts or phenomena to be explained. The writer very wisely, therefore, begins (ch. 1) by drawing attention to the differences between psychological and physiological phenomena. The second requisite of a science is the application of the methods of science to the facts to be explained. The method of psychology is next (ch. 2) presented. Then follows (ch. 3) an account of the structure and functions of the nervous system. The treatment of this subject has been put here at the beginning as a pedagogical concession to the critics of the *Handbook*, in which it appears as the introduction to part third. It is rather hazardous to attempt to present the salient facts of nervous physiology, and that too to beginners, in thirty pages; and the author is to be congratulated on the successful way in which he has done it. He closes the Introduction by (ch. 4) dividing his subject into four parts: the General Characteristics of Mind; Intellect; Feeling; Will.

Part First (pp. 56–80) discusses Consciousness and Attention. He distinguishes between Consciousness and Self-Consciousness. The former is the common and necessary form of all mental states,—their one condition and abiding characteristic. The nervous conditions of consciousness and the development of consciousness are briefly discussed. Then follows (ch. 5) a presentation of the nature, forms, and bearings on the mental life, of Attention.

Part Second, on Intellect (pp. 81–221), consists of nine chapters: Division of the Intellectual Functions; Sensation; Perception; Retention and Reproduction; Recognition and Localization; Association; Imagination; Illusions; Thought. The intellectual function is divided into the *Apperceptive* (embracing presentation, representation, and elaboration) and the *Rational*. Strange to say, 140 pages are devoted to the former, while nothing but a bare allusion (p. 221) is found in the book about the latter. The treatment of the Rational Function in the *Handbook* was meagre and unsatisfactory, but to pass it over in almost absolute silence in the *Elements* is a serious defect in the book. The presentative function is next divided into *Sense-Perception* and *Self-Consciousness* and then less than a single page (p. 126) is given to the latter all-important subject. In these particulars the simplification by condensation is decidedly no improvement. The next chapters on Sensation and Perception are among the clearest and best in the book. Sensations are "both affective and presentative," although "the intensive subjective state constitutes the sensation proper." They have as general characteristics which may be investigated, *quality*, *quantity*, *duration*, and *tone*. Sensations differ qualitatively independently of their hedonic tone. He finds it "reasonably safe to conclude that there are well-specialized nervous functions which correspond to the great differences of quality in sensations." The various classes of sensations are presented, and muscular sensations and the phenomena of contrast are discussed at considerable length. Under quantity of sensation, Weber's law is stated and the results of the psycho-physical inquiries to which it has led are indicated. Incidentally *extensity* is distinguished from quantity viewed as intensive and the remark is made: "The fact that it is found in connection with some of the non-spatial senses, e.g. sound, seems to be sufficient proof that it is not an immediate datum of space-knowledge, as some would have it." The time-relations of sensation (Psychometry) are then summarized, and the subject of their *tone* postponed for later discussion. The chapter on Perception is brief for so intricate a subject, but it is marked by great simplicity of treatment and clearness of statement, whatever may be thought of the positions taken. The problem is to explain our adult consciousness of a world of clearly discriminated, tri-dimensionally extended, extra-mental objects located in the midst of surrounding extents of which the world consists. He finds three logically (though not chronologically) distinct stages or steps in the development of this consciousness. The first is *Differentiation*. "It is probable that the earliest consciousness is a mass of touch and muscular sensations experienced in part before birth, and that it is only as the special senses become adapted to their living environment and sensitive to their peculiar forms of excitation that the general organic condition is broken up and the kind of sensations differentiated" (p. 60). The second stage is *Localization*—the mental reference of sensations to a locality in space: the passing from a subjective consciousness to an objective. This is effected in connection with our muscular and touch sensations. The third and final stage is *Sense-Intuition*, in which the mind by attention and association identifies things seen with things touched, etc., that is, gathers together the sensations into the permanent units or wholes which we call 'things. Throughout the discussion the activity of mind, and the fact that perception is an achievement of the mind, are emphasized.

The next two chapters treat of Reproduction—memory as retentive, reproductive, and recognitive, together with consciousness of time. A satisfactory discussion of the representative 'image' and its functions in the mental life is lacking here; otherwise the subjects are suggestively handled, especially the subject of time. Under the head of 'Combination' follow two chapters, one on Association and one on Memory. The position is taken that *contiguity* is the one ultimate law of association. The control of association by the rational will and its relation to the higher thought-activities is scarcely referred to, and no reference is made to the important principle which Professor Ladd so happily describes as 'condensation' in association. On the whole the discussion of association leaves much to be desired even in so elementary a treatise. A helpful chapter on Illusions follows the discussion of memory.

The treatment of the Intellectual Function closes with sixteen pages devoted to Elaboration or Thought—conception, judgment, and reasoning. The treatment of these important subjects is so brief as to be almost worthless. The author would have done better if he had not attempted to treat these topics in detail. As it is, his treatment is not so very serious a defect, as at this point the book would probably be supplemented, by those using it as a text-book, by some elementary work on logic.

Part Third, on Feeling (pp. 222–307), consists of seven chapters: Nature and Divisions of Sensibility; Pleasure and Pain; Nature and Divisions of Ideal Feeling; Common Ideal Feelings—Interest, Reality, Belief; Special Ideal Feelings—Presentative Emotions; Special Ideal Feelings—Emotions of Relation; Quantity and Duration of Emotion. For this part we have almost only words of praise. The second volume of the author's *Handbook*, which treated of the Feelings and the Will, was a distinct improvement on the first volume, which treated of the Intellect. And this condensation is a decided improvement on the *Handbook* in two particulars: sensuous pleasure and pain and ideal pleasure and pain are treated together in one chapter and not separated as in the larger work; and much of the indefiniteness which characterized the larger work disappears in the smaller. After discussing (ch. 16) the nature and divisions of sensibility, he divides all feeling into *Sensations* and *Emotions*. All Feeling in addition to its qualitative and quantitative characteristics has *tone* (pleasure-pain). Having in the first part discussed Sensations, he confines himself in part third to Pleasure-Pain and the Emotions. The discussion (ch. 17) of the physical conditions and the nature of pleasure and pain is excellent. A suggestive chapter (19) follows on (Common Ideal Feeling) interest, reality, and belief. The next two chapters (20, 21) treat ably, following in the main the Herbartian classification, the various kinds of Emotion. The treatment of 'Conscience,' however, is unsatisfactory; and all that is said of the religious emotion is: "The great class of *religious* feelings are also most closely connected with ethical emotion and rest upon it." This part closes with a chapter on quantity and duration of emotion, in which a variety of topics such as emotional expression, the association and conflict of emotion, and the like are handled.

Part Fourth (pp. 308–372) consists of chapters on the Motor Consciousness, Stimuli to Involuntary Movement, Stimuli to Voluntary Movement, Voluntary Movement, Volition. Exception will be taken to much that is said or implied here, according as one agrees or disagrees with the author's general philosophical position. The facts, however, are carefully and impartially presented ; and the discussion as regards both matter and form is superior to what is usually found in elementary works. This third part does much to give completeness to the work.

We have reviewed this book at such length for the reason that in spite of all the defects to which we have alluded, and others which we have passed over, we regard it as, on the whole, the best elementary text-book on psychology for use in academies, high-schools, and our smaller colleges now before the public. It is written from the scientific standpoint and in a thoroughly scientific spirit by one versed in the literature and acquainted with the latest advances of the science. The only other book which can compete with it is Professor James' *Briefer Psychology.* It lacks the brilliant qualities of James' book, while, on the other hand, it is more systematic and complete and hence better adapted for a text-book. The two books admirably supplement each other. If the one could be used as a required text-book and the other as supplementary reading, we believe that elementary instruction in psychology would be vastly improved. The mechanical make-up of the book is excellent ; but the publishers should see to it that an index is added.

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CHILD-PSYCHOLOGY.

L'Evolution intellectuelle et morale de l'enfant. G. COMPAYRÉ. Paris, Hachette, 1893. Pp. xxiv + 371.

The Psychology of Childhood. FRED. TRACY. Boston, Heath & Co., 1893. Pp. 94.

In the present condition of the study of the psychology of childhood, books of two classes are necessary aside from the detailed records of exact observations and experiments. We need first of all summaries which shall offer us from time to time exact and minutely-detailed topical statements of all observations already made by everybody and everywhere. Such statements are necessary, if reliable and exhaustive, to all who are themselves making observations or using them for purposes of interpretation. The second class of works which are needed, and which alone truly represent the object of the study, are works of interpretation and theory whereby the net results of the observations of all workers are made available for general psychological theory, especially on its genetic side.

The work of Compayré is not adapted to either of these purposes, nor does the author himself make new observations of importance on children. His book—magnificently printed on very heavy paper with wide margins—follows the French traditions on this topic. He covers the whole field in a pleasant, too-talkative style, stating old hypotheses clearly and well, without too much criticism, and failing to bring—as Perez fails to bring—the observations cited into range of the more important genetic questions ot later discussion by psychologists. Apart from interesting *aperçus* on minor points—such as the dependence of the child's memory on present objects (127 and 139), the recognition of 'automatic' imitation (181), etc., I find no important gain either to theory or fact. The book, however, will do good service as an 'Introduction' to the subject for readers who want to know the general state of the questions at issue and the nature of earlier observations and literature. It is probably the best book of this character; and possibly this is what the author had mainly in mind.

Tracy's work falls distinctly under the first of the two useful categories I have mentioned—a remark which I have a right to make, since it was undertaken at my suggestion and much of it written in consultation with myself with the purpose explained above in view.* As far as the carrying out of this purpose—to produce a condensed objective statement, by topics, of all work done to date everywhere—as far as this is concerned, the work is to be heartily commended. Tracy covers the literature much better than Compayré (who, for example, makes no reference, I think, to American work). He is also less subjective, although in this respect some of his sections might be further improved. The chapter on 'Language,' moreover, makes contribution to the theory of infant speech—a point which was spoken of by Prof. Dewey in the last issue of this REVIEW (I. p. 63 f.). I have, therefore, no hesitation in endorsing the words used by President Hall in his preface: "This work was greatly needed, and has been done with a thoroughness which all interested in the subject must gratefully recognize." The real value of the book, however, for its purpose, is almost entirely negatived by its external blemishes and deficiencies. It has neither table of contents

* See the author's note, B, p. 90.

nor index: a most remarkable double oversight in a book whose *raison d'être* is ready reference. Suppose a reader desirous of knowing all the observations hitherto made on the infant's 'walking reflex': he has not only no way to find this topic, but, further, no way to find the chapter in which it occurs—except as he may turn pages until he find the chapter on 'Volition,' and then look through that until he find a section which would 'seem likely' (such a section as 'reflex movement') —and explore that until, not finding what he wants (as he would not find this particular topic; it is probably an omission, though it may be where I have not looked), he would have spent and lost his time. It would seem necessary, if the book is to do what its excellences really fit it to do, that a detailed index should be prepared and sold with the work. It could then be bound in by the purchaser. Further blemishes, also, increase one's sense of the extreme carelessness of some responsible party. The print of the text is ruinous to the eyes in any light but daylight, and the foot-notes would arouse the *laboris amor* of histologists. Further, there are three typographical errors on the first three-quarter page of Chapter I, and the proportion seems to be pretty well kept up throughout. Nothing but a new edition would correct these blemishes: and it is to be hoped, in view both of the late results which should be frequently incorporated, and of the merit of the author's labor, that a new edition may soon appear. In that case it is to be hoped that Dr. Tracy will not again allow his work to be so mangled.

J. M. B.

THE NERVOUS SYSTEM.

Die trophischen Eigenschaften der Nerven. J. GAULE. Berliner Klin. Wochensch. Nos. 44 and 45. 1893.

Die trophischen Eigenschaften der Nerven. J. GAULE. Auszug aus dem in Nürnberg auf der Naturforscherversammlung gehaltenen Vortrage. Centralb. f. Nervenh. und Psych., Nov. 1893.

The background of organic processes of which our special sensations form the visible surface is of recognized psychological importance. From the physiological side this background may be considered as the total result of those activities of the nervous system which are called trophic, and hence an interest attaches for us to any advances in this field.

The titles given above refer to a running account of the general conclusion at which Gaule has arrived after experimenting for several years. He establishes his theoretical standpoint by dividing the forces acting upon the living organism into two groups: the group which we commonly recognize, which directly affects the sense-organs and thus calls forth those responses in the muscles and glands whereby adaptation is accomplished. But besides this group of which we are conscious there are such forces as gravity, humidity, atmospheric pressure, electrical tension, etc., which are constantly present, which change slowly, but of which we have as a rule no direct consciousness. Nevertheless we must also adapt ourselves to this latter group, and the processes by which this is accomplished may be designated as the trophic functions of the nervous system.

Experimenting, he repeated in the first place Majendie's operation on the fifth nerve, in which that nerve is cut intracranially. We know that at whatever point in its course this nerve is cut the cornea becomes anæsthetic. If the section be made in such a way as to damage the nerve-cells of the great Gasserian ganglion or the smaller number of cells found in the course of the nerve where it joins the medulla, then besides the anæsthesia, there appear trophic disturbances.

In this latter instance trophic changes are said to occur in the cornea in spite of all possible protection offered to it, whereas in the former, in which the cornea is merely anæsthetic, mechanical protection from outside influences is sufficient to prevent any trophic change.

Gaule insists that we are, therefore, to consider the trophic disturbances as due to the group of the general forces which he has enumerated and against which it is not possible to offer any protection. Since, however, the forces which were involved act in the same manner on both the eyes and only one of these shows change under the influence of them, we should look for the cause of these changes in the injury to the organism itself. From these experiments, then, we conclude that the connection of the cornea with injured ganglion-cells belonging to the fifth nerve disturbs its normal nutrition, and that this disturbance must depend upon some influence emanating from the cells themselves. The disturbance, then, has its origin in the nerve-cells, and whether we consider it as due to excessive or reduced activity there makes comparatively little difference. Furthermore, it is to be noted that this influence is transferred from the ganglion along the nerve to the periphery, a direction which is the reverse of that in which we ordinarily picture this nerve as acting.

Gaule next investigated the spinal ganglia in frogs and rabbits, and found as a result of excitation trophic changes in the muscles, glands, and skin. Taking the rabbits, which give the clearest reaction, the following results were obtained: these ganglia can be excited so as to bring about trophic reactions by the interruption of rather a strong, constant current, by cutting in various ways, and by cauterizing. Induction currents were found ineffective, and the stimuli mentioned produced results only when acting upon the ganglion still inclosed in its sack. If the sack had been previously opened no results followed. The distribution of the disturbances was not only in the area supplied by the sensory nerve, the ganglion of which was irritated, but it extended outside that area. So far as the muscles were concerned the trophic changes not only affected those of the same side and those most closely associated with the sensory nerve concerned, but also others which were remote. At the same time they affected muscles in the opposite half of the body. It will be seen from this that the trophic influence of the spinal ganglion is not only exerted in the same manner as in the case of the Gasserian ganglion, but that also it must act through the spinal cord in order to produce the disturbances in the muscles. That such is really the pathway for the impulses is indicated by the fact that section of the dorsal root between the ganglion and the cord abolishes the effect upon the muscles. Both the conditions of effective stimulation and the distribution of the effects, as thus described, are well calculated to excite remark, not to mention the fact that the disturbance can be passed on from the sensory to the motor fibres.

When Gaule turned next to the study of the sympathetic ganglion, and confined his attention to the changes taking place in the muscles, he was able to control the results with more accuracy. He used the inferior cervical ganglion which was connected by the ramus communicans with the spinal ganglion. Upon injury or stimulation of this ganglion there appeared constant changes always occurring in the same muscles (biceps brachii and psoas) both upon the same side as that of the ganglion which was stimulated and also upon the opposite side. The fact that this influence thus crossed the middle line showed that the action must take place by way of the spinal cord, and Gaule conceives that the pathway is along the ramus communicans to the spinal ganglion and thence to the cord. There is one curious condition, however, which is said to control these results. One of the branches of the inferior cervical ganglion acts to inhibit its trophic function in such a way that if, when the ganglion is stimulated, this branch is also stimulated, the results recorded do not follow. The trophic change which occurs in the muscles as the result of stimulating the sympathetic ganglion brings about a solution of the muscle substance somewhere about the middle of the muscle, the fibres rupture, blood-vessels are broken, and an ulcer is formed, which on the cessation of the stimulus slowly disappears with the formation of scar tissue.

The most striking fact which Gaule has to communicate is that this process can be followed from its very beginning in the living muscle of the animal undergoing stimulation, and the various changes leading up to the formation of the ulcer can be directly observed. Such being the case, we have through these investigations some very important and wide-reaching phenomena forced upon our attention, and the fact that by stimulation of the sympathetic ganglion we can regularly develop ulcers in a given muscle must command attention, however peculiar and inexplicable some of the minor points may appear. H. H. D.

ABNORMAL.

Les altérations de la personalité. ALFRED BINET. Internat. Scientific Ser. Paris, Alcan, 1892. Pp. viii + 323.

The use and development of the experimental method in psychology are giving an entirely different aspect to most of the questions to which this science gives rise, and are beginning to profoundly modify our views as to some subjects which had remained, almost to our own day, the objects merely of abstract analysis. In the mind of the metaphysician the idea of personality calls up the conceptions of individuality and self-identity. Every one who experiments is continually seeing how often the facts contradict theories based solely on logical deductions.

In this work M. Binet makes a study of the problem of personality. He publishes a very considerable number of observations, some of them made by himself, and some taken from the works of leading writers on this important subject. But, as he states in the introduction, he makes it an invariable rule to rely only on such observations as can be easily verified. His inductions have been drawn only from facts over which those making the experiments have had control, which have been reached by different methods, and in the majority of cases from different points of view. Double alterations of the personality of the same individual, the alternation or the coexistence, the separation or the conjunction of distinct consciousnesses, are the phenomena which he analyzes successively, in both natural and artificial sleep, in hysterical anæsthesia, in the conditions of diverted attention and of suggestion. Even normal subjects may present a plurality of consciousnesses. In the light of these phenomena the theories of the association of ideas and of memory are enriched. Sensation, judgment, and reasoning may manifest their presence subconsciously apart from the normal personality. The book, although limited to the question of personality, throws light on the whole field of psychology.

Binet first studies the spontaneous phenomena. "They present," he says, "this great advantage, that the preconceived idea of the narrator does not involuntarily or unconsciously distort them." The observations, some of which have become classic, of Azam on Felida, of Bouru and Burot on Louis V——, of Mesnet on F—— the sergeant of Bazeilles, of Guinon on B—— the journalist, etc., show that the same individual may have successively two or more mental conditions in which it is possible to discern two important psychological modifications, a change of character and a modification of memory. When the experiments come to be more methodically conducted, moreover, a third element comes to light, viz., the distribution of sensibility on the surface of the body.

Two classes can be named : sometimes the person affected (like Felida and Louis V——) continues while experiencing the second condition to live his ordinary life, keeping his mind open to all the usual perceptions and ideas : sometimes (like F—— and B——) he is delirious—he is possessed by thoughts which give to his activity a systematic trend, and he avoids everything not germane to his immediate prepossession. Nor does it infrequently happen in either the one case or the other that the individual affected manifests more than two, sometimes even five or six, different mental conditions.

The changes in character are very marked. Subjects pass from melancholy to gayety, from sober and honest persons to gourmands and thieves. Their sensibility is altered. F—— preserved in the second condition his muscular sensibility ; but he lost his general sensibility, hearing, taste, and smell. His sight was not exercised except on the occasion of touch, and was restricted to those objects with which he was in actual contact.

The modifications of memory are also very interesting. In the normal state, the subject does not remember what happened during the second state. In the latter, when he is not delirious, he sometimes forgets the events of the normal state ; but more often he remembers them. He remembers, equally well, everything he did and said during his former experience of the second state. His memory may also extend very far into the past and embrace periods which had been entirely forgotten in the normal state, but which he will now recall in most minute detail. When he is delirious in his second personality—what is his condition ? In the absence of direct testimony (since we cannot speak with the subject) the repetition or continuation of actions begun in the former second state sufficiently demonstrates the unity of the somnambulistic personality. B——, the journalist, continues while in successive second states to write a novel and takes up the story each time at the right point, at the very word where he had been interrupted.

Certain observers (and among them Menet, who has studied the case of F——) believe that in the abnormal state there is no trace of conscious thought, of judgment, or of imagination. Huxley, indeed, has used this as evidence in support of the epiphenomenon-theory of consciousness. They believe that actions of subjects are the effect of a purely reflex and mechanical activity. Binet believes this opinion to be erroneous. For in the course of the experiments, patients give evident signs of surprise or astonishment at sounds, which would not happen if consciousness were absent.

During the normal state, what becomes of that extraordinary existence which possesses its own memory and its own character? Direct observation can give no answer. The experiments of Gurney show that traces of somnambulistic experience may continue in the waking state without the normal patient having the least suspicion of it. Numbers and names have been repeated to the somnambulistic subject. On awakening he remembers nothing. A rolling planchette with a pencil attachment is placed in his hand. In a few minutes his hand moves and writes the exact words and numbers which had been pronounced. There are thus coexistences of two distinct consciousnesses, "an *ensemble* of distinct psychological phenomena, thoroughly co-ordinated the one with the other, kept apart and continued without reference to normal personality." The somnambulistic personality is also a unity, since when the subject is again put to sleep, he declares that he has been writing with the planchette.

The second part of Binet's book is devoted to these phenomena of the coexistence of more than one consciousness. They are most easily observed in hysterical insensibility and in the state of diverted attention. Hysteric anæsthesia is an insensibility by reason of the partial unconsciousness which proceeds from the fact that the personality of the subject is cut in two or doubled. From an anæsthetic arm we may obtain the phenomena of the repetition of actions and of adaptation. When a pencil is placed in the anæsthetic hand of the subject and is concealed from him by a hand-screen, the hand itself will repeat a great many times a sign or letter which the subject is told to write. The subject, however, has consciousness neither of the impulse given to his fingers nor of their natural movements. Shall we say, then, that this is the effect of a mere physiological mechanism? This is doubtful, for the hand does not give evidence of memory alone. If, with a blunt point, we trace on the hand letters or figures (impressions which the normal subject does not feel at all) this will be enough to make the hand write them. There has been, then, a transformation of cutaneous sensations into their graphic equivalents. It is better still if one voluntarily alters the spelling of a word. In this case, the hand hesitates and restores the correct orthography. It cannot, then, be said to be a passive condition. Furthermore, when the hand is guided, it is often observed to resist or anticipate the movements of the operator. When the subject is writing with his anæsthetic hand, it often happens that the hand repeats the same letter over and over, a sort of hand-stammering in fact.

Have motor images been partially awakened subconsciously so that as long as they meet with no resistance they expend their force in repetition of the act? It may with equal reason be supposed that there is something in these actions in the nature of a suggestion comprehended by a subconscious subject.

We now pass to the phenomena of adaptation. Suppose that we excite the anæsthetic arm of a subject, concealing it by a screen. If the arm is supported for a moment it remains stretched out. If we wish it to fall we raise it and suddenly release it. The arm seems to understand the wish of the operator. If a weight is suddenly attached to the subject's arm, an effort is made proportioned to the new burden. If, on the contrary, we support the arm, it will be gradually lowered. If a well-known object is placed in the insensible hand, touch will suggest its use. It will open and shut a pair of scissors or press a dynamometer. Some subjects cannot be influenced except by their regular hypnotizers. Neither can an anæsthetic arm be placed in a state of catalepsy except by one operator. If any other hand touch the subject the phenomenon does not occur. The unconscious subject can therefore exhibit choice. Now there are some phenomena more complex than these. If the anæsthetic hand of a subject be pricked from behind a screen, he exclaims, 'You have hurt me!' Should we therefore say that sensibility was restored? We question him, and he replies that he has felt nothing and has said nothing. It was the unconscious that for a moment appeared upon the scene.

"To explain how unconscious actions are produced," says Binet (p. 117), "we must not be content with the hypothesis of unconscious sensations. Isolated, unconscious sensations could produce nothing. Now, in analyzing the principal observations which we have collected, we have noticed the intervention of phenomena of memory and of reasoning, so that the unconscious movements reveal within us the existence of an intelligence other than that of the self of the subject, and which acts without his aid and even without his knowledge.

Binet next studies the phenomena obtained by aid of diverted attention. Attention is an effort of the mind and of the organism which has the general effect of increasing the intensity of certain states of consciousness. This does not happen without attention. All that is not the object of attention remains in a condition of less sensibility, and the same is true of anæsthesia. The attention contracts the field of consciousness. At the same time, among hysterics chiefly, at the side of the normal personality, a subconscious personality, with which we can communicate and obtain responses, develops itself. For this person the anæsthesia does not exist: it itself remembers what it has done, and can receive suggestions. In writing, it uses the word 'I' to designate itself, and speaks of the normal self in the third person.

A second self, formed by the aid of diverted attention, does not make one with the self of the anæsthetic and the self of the somnambulist. The proof for this is easy. The self of the subject in somnambulism can repeat the names spoken to the second self in the state of diverted attention. Suppose the subconscious personality has been created by an involuntary suggestion of the operator, it nevertheless remains true that the disaggregated psychic phenomena have grouped themselves around a new centre.

The subconscious self is frequently a colaborer with the normal self. The voluntary movements of an anæsthetic member survive the loss of the consciousness of passive movements. The question, from the point of view of psychology, is very obscure, for the reason that anæsthesia of any sense, with rare exceptions, induces the loss of memory of the corresponding images. Therefore, the individual affected cannot represent to himself beforehand the movement to be performed. On the other hand, no afferent sensation can inform him as to the position of his limb. Subjects frequently say they represent to themselves, visually, the movements of the hand concealed behind a screen—deceiving themselves again. They are often deceived, while it writes, as to what letters it is tracing at any given moment. Therefore, no definite image remained in the normal personality, as a visual memory which guided the voluntary movements of the anæsthetic limb either well or ill. One is therefore obliged to admit the existence of sensations and kinæsthetic images in subconsciousness, to explain the co-ordination of these movements. The normal self commands actions which the subconscious self carries out.

Moreover, a conscious mental representation can excite subconscious movements without the knowledge of the subject. This fact is shown by the phenomena of automatic writing. The subject is asked to tell his age. He does so, and the pencil which has been slipped into the anæsthetic hand writes the same response. The movements of writing cannot thus produce themselves. The sensations and ideas of the principal consciousness determine the different effects within the second consciousness. An hysterical patient hears the strokes of the metronome. Between her fingers is placed a tube of India rubber wound round a cylinder, so arranged that the rubber yields at each beat. The patient is told to think of a number. The anæsthetic hand makes the corresponding number of strokes.

We have considered the action of normal consciousness upon sub-consciousness. Binet next considers the inverse phenomena. The sensations coming from the anæsthetic regions remain unconscious, but may penetrate the normal consciousness under the form of images, ideas, and false perceptions or hallucinations.

For example, we may prick an anæsthetic hand nine times. The subject has no consciousness of the pricks, but he thinks the number nine. He is also able to see points, bars, and columns, corresponding in number to that of the stimuli. He also exhibits the transposition of sensations. Binet hung around the anæsthetic head of a patient a medal with the design in relief. The subject said immediately that she was dazed, that she saw bright spots in the form of a circle. When asked to state what she saw, she related exactly (much more exactly than a normal subject could afterwards do) the details of the design, although the subject was an entirely strange one to her. This experiment indicates on the part of unconsciousness a quite remarkable activity of perception.

"The dividing of consciousness, therefore, is not by a sharp line of demarcation, suspending all relations between the consciousnesses," writes Binet. "On the contrary, the psychological phenomena of each group exercise a constant influence upon those of the neighboring group. The division leaves unimpaired the automatism of mental images, sensations, and movements. It consists solely in a limitation of consciousness. Each ego knows only what transpires in its own domain."

Binet next shows that there may be a plurality of consciousnesses in normal subjects. The phenomena above referred to have long been observed with nearly the same results, but less marked, in the cases of normal subjects. Their unconscious movements should be considered, it seems, as the effects of a very slightly marked mental duplication. When an individual is told to divide his attention so as to do two things at once—for example, to talk and to draw—it often happens that he directs his thought to one of the actions and performs the second automatically. Indeed we may sometimes, in the case of a normal subject, obtain a complete diversion of the attention and establish by means of automatic writing a division of consciousness analogous to that described above.

Suggestion is ordinarily followed by a division in consciousness. Here suggestions tending to create a new personality may be distinguished from those whose end can only be accomplished by a division of consciousness. In the first case the observer bids the subject assume this or that personality. Richet has thus made his patients, A and B, take the characters, successively, of peasant, actress, general, priest, nun, sailor, young woman, and little girl. The subjects retain their hallucinations along with their adopted characters. Their faculties of perception and ideation are perverted to the same standard, and whatever might contradict the suggestion of the operator is cast aside and banished from consciousness. In a similar manner the subject may be carried back to a previous stage of existence. In such a case many things return to the memory which in the normal state had fallen into utter oblivion. From this we may infer that the limits of our personal and conscious memory are not absolute. "What we know of ourselves," says Binet, "is but a part and perhaps an extremely small part of what we are." The laws of association fail to explain how it is that the things which have been kept in memory do not awaken at the stimulus of new impressions to which they should respond. Causes the most profound, because unconscious, are operating to distribute our ideas, perceptions, and mental images in syntheses at once automatic and independent. In the case of post-hypnotic suggestions the subject fails to perceive the hallucinations of his normal personality. For cases of long-deferred suggestion some explanation must be offered as to how the subject is able to calculate the time. It is still the subconscious personality that here intervenes, that keeps mental images in the given order and reckons the days and hours. In fact he can make these judgments as well as one who has actually passed through the experience. We say to the subject of double personality, "When the sum of the numbers which I am going to pronounce amounts to ten you will raise your hands." We murmur 2, 3, 1, 4, and the movement is made. Such phenomena, therefore, ordinarily take place unconsciously.

The subject of Systematic Anæsthesia yet remains. It is suggested to a subject that on his awakening he will not see a certain person, who, however, remains near him. His vision remains unimpaired, but the subject insists that the person in question is not there. The phenomenon is exceedingly complex. "The inhibited perception continues to manifest itself, but it remains unconscious. The subject seems to possess an intelligence different from the normal self, which decides whether he shall choose this or that." The facts of spiritualism and of the varying personalities which mediums claim to exhibit are explained by a division of consciousness accompanied by auto-suggestion.

Binet's conclusions are as follows : 1. The elements entering into the normal constitution of self may exist in a state of disaggregation. 2. A consciousness continues to accompany these elements, although the self loses knowledge of it. 3. Sometimes under pathological or experimental conditions these elements organize themselves into a second personality.

Binet's work is an extremely interesting study of the phenomena of subconsciousness. Psychology extends to the limits of definite consciousness and normal personality, the unity of which—Binet and Ribot being here in accord—should be sought for only "in the co-ordination of a certain number of psychological phenomena during a certain time." The hypothesis of subconsciousness, Huxley's adherents will say, is unnecessary. The physiological mechanism will account for everything. Is it true that the explanation is sufficient when the subject writes with an anæsthetic member an entire page in which the ideas follow one another in the proper order? Language, whether written or spoken, of course fails to explain either consciousness or personality. But what is the difficulty in admitting the existence of a second personality, when such personalities are produced spontaneously in some subjects? For cases of adaptation, and especially for those of repetition, we may appeal to muscular habit. But if the fact of subconsciousness has been proved, is it not natural to believe that, beneath the threshold of personal consciousness, lesser stimuli, down to a certain minimum, give rise to the phenomena of subconsciousness? It is rather the mental *nothing* which it is difficult to conceive. To say that beneath a certain stimulus all consciousness vanishes is to dig around consciousness an arbitrary ditch.

Are the subconscious phenomena necessary to explain the cases of repetition and adaptation connected with a secondary self? This question is very obscure. The second part of Binet's conclusion seems correct in the terms in which he has expressed it, and we agree with the author. But it seems to us that he here recedes a little from some of his more decided positions in the body of the work. Isolated subconscious sensations are productive of nothing. But are not the elementary syntheses of psychic phenomena enough to explain the simplest facts, the repetition in writing a letter, the resistance of the anæsthetic arm to strong pressure, its obedience to a moderate one, and even the act of pressing the dynamometer? May not the rectification of a fault in spelling be explained by the facts of motor habit? We hesitate to attribute to subconsciousness an established organization and definiteness analogous to that of normal consciousness when a secondary personality has not been clearly proved.

On the other hand, we hold with Binet that the phenomena pertaining to our normal personality remain in a state of disaggregation. As we go through life certain of our natural propensities fall into desuetude, certain mental images of past experience sink into oblivion. Our self is being made over anew. But nothing perishes that has once existed, and the past can be restored to life. Our consciousness reveals but a tiny part of what we really are, and "consciousness slips away and is lost by imperceptible transitions." * T. COURTIER.

SORBONNE, PARIS.

État mental des hystériques : les stigmates mentaux ; État mental des hystériques : les accidents mentaux. PIERRE JANET. Rueff, Paris, 1892, 1894. 2 vols., 12mo. Pp. 233, 304.

L'amnésie continue. PIERRE JANET. Revue générale des sciences, 30 mars 1893.

M. Janet is a *vielseitiger Mensch*, being now an M.D. and visiting physician at the Salpêtrière, without having ceased to be a Professor of Philosophy at the Collége Rollin. The present volumes continue the line of observation and reflection so brilliantly begun in his earlier work, *L'automatisme psychologique*, and may be said to set the seal on the revolution which during the last decade has been going on in our conceptions of hysterical disease. Amongst all the many victims of medical ignorance clad in authority the poor hysteric has hitherto perhaps fared worst; and her gradual rehabilitation and rescue will count amongst the philanthropic conquests of our generation. At first branded as one inflamed with uterine furor, she was next burned as a witch, and finally treated as so radically perverse and mendacious a jade as to be theatrical even in the hour of death. Now, thanks first to Charcot, Janet, Pitres, Gilles de la Tourette, and in a less degree to many others, she or he (for hysteria is now allowed to be a male complaint) can be regarded as estimable morally, and only pitied as one subject to a curious form of weakness of the intellect.

The weakness in question is described by M. Janet substantially as follows. In the constitution called hysterical the threshold of the principal consciousness is not fixed, but movable. It can be shifted by physical and moral shocks and strains so that sensations and ideas of which the patient ought to be fully aware become 'subliminal,' or buried and forgotten, and in this parasitic state persist more or less monotonously. The nucleus of these subconscious fixed ideas usually consists of reminiscences of the shock by which the mind was originally shattered ; but in process of time other painful reminiscences

* Translated by C. A. Tawney, Fellow in Philosophy in Princeton College.

may be added, and accidental associations may complicate the system which, from its hiding-place below the principal consciousness, may produce effects of the most baleful sort upon the latter, effects irruptive (hallucinations and motor impulses) as well as subtractive (anæsthesias, amnesias, aboulias, confusions, etc.), and moreover may influence the bodily functions in manifold and formidable ways.

M. Janet proves the existence of these fixed ideas by many methods, by hypnosis, by automatic writing, by the hallucinations that come out in 'crystal-gazing,' by the patient's talk in sleep, by utterances during the 'attack,' and finally by what he calls the 'method of distraction,' which practically is only a variety of automatic writing. In these circumstances hysterics will reveal obsessive memories and ideas of which their principal consciousness is wholly unaware, and will explain in detail the images by which their various symptoms are determined. The most general morbid result, or stigma, from which they suffer is a *narrowing* of their principal consciousness, to which narrowing M. Janet more or less successfully ascribes the various defects by which the 'hysterical' character is popularly known: vacillation, inconsistency, revery, lack of will and of power of attention, enfeebled memory, and *ennui*. He shows us one woman acquiring accidentally these symptoms in consequence of certain ideas which she had received during hypnosis, and which remained subliminal after she was awake; and another reacquiring lost memories and will-power *pari passu* with the destruction, by suggestion, of her subliminal delusions. The anæsthesias, so characteristic in hysteria, he explains as acquired habits of ignoring certain sensations, which thus get handed over to the subliminal self. That they are false anæsthesias, and that the sensations of eye, skin, or what not, which appear non-existent, are really there, though hidden from view, is, thanks to Binet, Bernheim, Janet, and others, one of the most securely established facts of recent psychology. The most immediate result of this disintegrated condition of the mind is *suggestibility* on the subject's part, concerning which phenomenon M. Janet's second volume contains some very acute pages of psychological reflection. An idea implanted in such a mind develops its own eccentric consequences in a way impossible where the mental elements are more firmly knit together.

Grafted on this general background are the more fluctuating 'accidents' of the malady. The subterranean ideas and memories have periodical eruptions which constitute the well-known hysterical 'attacks.' Of what possesses the consciousness during an attack the patient can generally give no account when it is over; but the emotional attitudes and ejaculations of which it consists, and the causes which may provoke it, show, when combined with the somnambulic and other revelations, that its nucleus is an hallucinatory re-enactment of the shock from which the whole morbid history dates. Thus George has an attack if you show him a lighted match, or if he looks at the fire in the stove. In this attack he shouts 'fire' and calls on the *pompiers*. He became ill after a fright caused by a fire ; Alz . . . had his shoulder wounded in an elevator-accident, and now, if you touch the place, you provoke an attack characterized by terror of being crushed ; Renée has a classical complex attack of 'grande hystérie,' of which most of the elements can be traced to reminiscences ; she mews like a certain cat that startled her, and barks like a dog that she detested when a child ; she imitates a little idiot at whose sight she was once horrified ; she strikes an attitude that copies the posture of 'Truth' in a picture in her room ; Marcella has hallucinations which reproduce painful experiences—in short, the attack is everywhere essentially a reminiscent dream. Charcot's 'complete' attack is no special natural entity, but only a dream due to the association into a system of a number of different morbid memories. The ideas may be stratified, as it were : "Is . . . , as a result of having been violated and having clandestinely had a child, first manifests refusal to eat (subconscious fixed idea of suicide), later is irascible and violent (subconscious idea of homicidal revenge), and finally, whilst apparently well, has a *bizarrerie* which remains, and consists in her inability to bear the sight of little children. She is impelled to beat them ; and if they remain long in her presence she goes into an attack (subconscious aversion to a child as the cause of her disgrace). The case of Marcella, published three or four years ago by Janet in the *Revue philosophique*, is a beautiful example of stratification in fixed ideas. As each one was removed by suggestion, a deeper and older one came to the surface and worked itself off, until with a final outbreak of suicidal frenzy, the girl got entirely well. The fixed ideas may slumber until some weakening of the nervous system favors their morbid activity. E.g., Col. is victim of a railroad accident, and passes six months in the hospital with a grave abdominal injury. During the next six years he seems well, save that he can no longer get drunk as he formerly did, for, if he does, he raves of the accident and suffers cruelly in his abdominal wound. At the end of six years he undergoes domestic calamities, witnesses the death of wife, child, etc., whereupon depression, revery, incapacity for work, set in, and he comes again to the hospital with a meteorized and hyperæsthetic abdomen, which M. Janet does not hesitate to ascribe to subconscious reminiscence, for if the old scar be touched an hysterical attack is provoked, consisting in hallucinations of the railroad tragedy.

Spasmodic disorders, *tics*, as the French call them, are among the commonest hysteric symptoms. These also, according to M. Janet can be traced to subliminal ideas. The girl Mel . . . has a choreic movement night and day, which imitates movements that she daily has to perform in her factory. Doing it in her sleep she murmurs, '*il faut travailler, il faut travailler*,' and the whole thing is finally explained as the result of her having overheard, whilst lying half-awake, her parents lament their poverty and inability to pay their rent. "A simple procedure cured this chorea, due to filial piety." Vel . . . for eight years has had a particularly odious tic, consisting in an expulsion of air through the nose, and a contortion of one side of the face. This resists every conceivable treatment, and for the patient's consciousness is irresistible and motiveless. The moment he is hypnotized, however, he says, '*j'ai une croûte dans le nez; elle me gêne*'; and a corrective suggestion then made abolishes the symptom. It would appear that the subconscious delusion here dated from certain nose-bleeds in a typhoid fever eight years before.—And so we are led by our author through the whole train of hysteric symptoms. The paralyses are interpreted by him as amnesias of certain kinæsthetic images, when they are not results of subconscious delusion; the 'contractures' come from fixed ideas; so do the dumbnesses and refusals to eat. Isabella cannot eat. She knows not why, but it appears that during each of her 'attacks' her dead mother appears to her, upbraids her for a past misdemeanor, calls her unworthy to live, and forbids her to take food. Similarly Marcella's anorexia comes from a voice which she hears during her attacks and which orders her to starve.

Our space permits no more details, but the reader can already see how rich a mine both of new facts and of new ideas Dr. Janet's little volumes are. Every psychologist should make their acquaintance. Their author's intellect is, if anything, too inductive; he is never quite at ease when away from one of his concrete examples, and he cares perhaps too little for things unlike what he has himself seen. But these are the only faults I can find with his work. He has certainly established his main point, and the class of cases which he describes will hereafter rank as real. But they are all grave cases, where the patients were *non compos* and had to be taken care of at an institution. How far their type can be generalized, and how far the milder cases met in private practice will also be found to suffer from split-off fixed ideas, remains for the future to inquire. According to all past analogies, what will probably happen is that the morbid type conceived by M. Janet will undergo both restriction and extension. In certain kinds of so-called hysteria no subliminal ideas will be ascertained; whilst such ideas probably will be ascertained in cases not easily recognizable through their other features as similar to those which M. Janet cites. At any rate this observer has set a great ball rolling, and it is to be hoped that he will be able to continue playing an active part in the superintendence of its career. W. J.

Ueber den psychischen Mechanismus hysterischer Phänomene. J. BREUER und S. FREUD. [Mendel's] Neurol. Centralbl., 1893, pp. 4, 43.

"Hysteria is a disease of the hypnotic stratum," wrote Mr. F. W. H. Myers many years ago, and this important paper is a comment on his dictum and an independent corroboration of Janet's views reported above. The distinguished Viennese neurologists who sign it stumbled accidentally on cures which enable them not only to give a general formula for the disease, but a general method for its treatment. Hysteria for them starts always from a shock, and is a 'disease of the memory.' Certain reminiscences of the shock fall into the subliminal consciousness, where they can only be discovered in 'hypnoid' states. If left there, they act as permanent 'psychic *traumata*,' thorns in the spirit, so to speak. The cure is to draw them out in hypnotism, let them produce all their emotional effects, however violent, and *work themselves off.* They make then (apparently) a new connection with the principal consciousness, whose breach is thus restored, and the sufferer gets well. Janet's Marcella, mentioned above, would be a case in point. W. J.

The Philosophy of Mental Healing: a practical exposition of natural restorative power. LEANDER EDMUND WHIPPLE. Metaphysical Publishing Co., New York, 1893. 8vo. Pp. 234.

It is but just to our American mind-curers of the various sects to say that for years past the notion that all sorts of morbid symptoms may spring from subconscious fixed ideas, such as old fears, griefs, and remorses, has been the basis of their treatment. Mr. Whipple's book sets forth this notion in an earnest way, in the good English of an educated man. Although the theoretic exposition seems to the more carnal and school-bred mind of the present reporter to lack technical sharpness, it is much more assimilable than any previous statement which he has read. The most striking feature of the book to him is a list of cases which the author gives at length. Neuralgia, rheumatism, bronchial catarrh, debility, nervous agitation, chronic diarrhœa, insomnia, dyspepsia, and chills are shown, in as many patients, to have arisen from subconscious or conscious fixed ideas, which being scattered, recovery ensued. The author well says that, in patients of this sort, to treat the mere external symptom would be as bad practice as for an engine-driver to slow his engine by scotching the wheels and pistons rather than by regulating the steam-box or the boiler. Yet in cases of refusal to eat, like those quoted from Janet in the last review but one, the only practice known at an ordinary asylum is forcible feeding by the stomach-pump. It is to be regretted that Mr. Whipple gives no detailed account of the practical method by which the fixed ideas are to be pulverized away. In most 'metaphysical healing' it seems to involve something like the telepathic action of one subliminal self upon another; but with this we navigate in full wonderland, where without safe guidance we had better not proceed. W. J.

THE PERCEPTION OF LIGHT AND COLOR.

Zur Farbenperimetrie. EMIL HEGG. Arch. f. Ophth., XXXVIII. (3) 145. 1891.

Eine Methode der objectiven Prüfung des Farbensinns. M. SACHS. Arch. f. Ophth. XXXIX. (3) 108–125. 1893.

The experiments of Hess (Arch. f. Ophth., XXXV. 4, 1889) have made it plain that there are certain colors which do not change their tone as they are viewed by portions of the retina more and more remote from the fovea. These colors are (with slight variation due to individual differences in the retina and to changes in the objective illumination): yellow ($575\mu\mu$), green ($495–497\mu\mu$), blue ($471\mu\mu$), and a red somewhat more blue than the spectral red; and these colors are called by him, in correspondence with this fact, the invariable colors. Yellow and blue are visible farther out upon the retinal field than red and green; from this fact it follows that a mixture of red and yellow will begin to lose its red constituent first and will grow yellower before it grows colorless, and that, in the same way, all colors except the invariable ones are subject to changes of tone as they approach the periphery. These facts have been confirmed by several other observers (the writer has obtained the same results, as regards the spectral colors, in Prof. König's laboratory). It will therefore be necessary, hereafter, to replace the vague statements in regard to change of color which are now to be found in the text-books by this more definite knowledge. Emil Hegg, in pursuance of the subject, has been able, after overcoming many difficulties, to first prepare upon the color-wheel, and then to reproduce in paint upon tin plates, colors which have these properties: 1. They are invariable in tone. 2. Their saturation is such that *equal* sectors, mixed upon the color-wheel, give gray. 3. When observed in the extreme periphery of the eye, when they are colorless they are of equal brightness. Colors thus prepared are found to be, as might be expected, extremely well adapted to detecting localized defect in color-sense in diseased retinas, and hence are very useful for the practical oculist. They may be obtained from Herr Maler Lauterburg, in Berne.

The writer of this paper shows a confusion in the use of the term brightness which is not uncommon among the followers of Hering. Colored papers which look equally bright when colorless he sometimes refers to as having equal white valence, which is correct for one who admits the existence of a separate process for white; but again he speaks of them as being simply equally bright, and he says (p. 149) that colors can be compared with each other as regards their brightness-value by means of the Hering screen used in Hess' experiments already referred to. This is not correct. It is now some time since Hering has remodelled his theory in such fashion as no longer to say that the brightness of a color-sensation is simply its accompanying whiteness, but rather that the color-process adds a specific amount (positive or negative in quality) to the total brightness-effect. This is, of course, a provision which he should have incorporated into his theory in the beginning, had he not remained, apparently, for many years oblivious of the well-known Purkinje phenomenon. It is therefore not possible, theoretically, to say that colors are equally bright because they look equally bright when the illumination is very faint, or when they are seen in the extreme periphery of the eye; nor is it true as matter of fact. On the Chevreul brightness-scale, which gives twenty-one steps between black and white, Hillebrand found that the colors had to be moved (up or down as the case might be) from three to six steps according as the comparison took place in a bright or in a faint illumination. (*Specifische Helligkeit der Farben*, Ber. d. Akad. d. Wissensch. in Wien, XCVIII. 3, 1889.) And from this it follows also that color-value (*Farbenwerth*) is a term whose meaning may easily become ambiguous; it might as readily mean the specific brightness due to the color-process as the power of quenching its antagonistic color and producing white. It is in the latter sense that Hegg uses it. It might perhaps be better to adopt this phraseology: two different colors (in the general sense) have equal white-value if they look equally bright in the dark; they have equal color-value if they *also* look equally bright in a good light (but from the fact alone that they look equally bright in a good light it does not follow, of course, that their white-values and their color-values are severally equal); they have equal *color-quenching value* if equal portions of them, mixed on the color-wheel, make white (gray).

In continuation of his former work (*Pflüger's Archiv*, Bd. 52, pp. 79-86) in which he showed that the width of the pupil varies very exactly with the subjective brightness of the light which falls upon the eye, Dr. Sachs has tested the method as a means of detecting color-blindness, both partial and total, and has found it to be very effective. The person to be examined looked at an opening in the window-shutter of a dark room, which was covered in succession with different gray and colored glasses, and the varying width of his pupil was observed with the aid of a telescope. Two glasses were selected which were of equal brightness for the normal eye in an illumination so faint that their color was not perceptible,—which had, that is to say, equal white valence. At an ordinary illumination these proved to be of very different brightness for the normal eye, as indicated by width of pupil as well as by sensation, but for the totally color-blind person no illumination could be found at which they affected the pupil differently. A red-green blind person was also examined, with the result that to blue and yellow glasses he reacted like a person with normal eyes, while red and green left him with width of pupil unaltered. An instructive experiment was the following: a red or yellow glass was found which, while it had less white valence, was brighter for the normal eye than a given green or blue; the change from one glass to the other caused the pupil of the totally color-blind person to contract and that of the person with normal vision to widen. The method would be of special value for testing persons of defective intelligence, deceitful or hysterical persons, and infants. It ought to be used at once to determine the question whether the color-sense is developed in infants later than the sensitiveness to changes of brightness of white light.

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BINOCULAR VISION.

Die Stabilität der Raumwerte auf der Netzhaut. FRANZ HILLEBRAND. Zeitschr. f. Psych., v. 1–60. 1893.

The problem which this paper attempts to solve is thus stated: Are, for all cases of binocular single-vision, the space-values of the two retinas stable or otherwise? Historically this question has been answered in both ways. The older 'projection-theory,' that an object is seen at the intersection of the lines of regard, involves variability of space-values; for if for one distance of the fixation-point these lines fall upon retinal points *a* and *a'*, for any other distance, if one line falls upon *a*, the other cannot fall on *a'*. Hering, on the other hand, maintains that the localization of a point depends, not on the point of intersection of the lines of regard, but on the space-values of the two retinal points affected.

Experiments to decide this question must consider only *primitive* sensations (those given by retina and muscles), unmodified by empirical elements (perspective, size of retinal image, etc.) which aid in localizing with reference to the horopter. Such empirical aids as affect the apparent distance of this surface itself are irrelevant, since we have to consider only whether particular objects appear in front of, in, or behind this surface. The experiments must separately decide as to whether the variability, if it exists, is horizontally disparate, or vertically disparate, or both.

To determine the first of these questions, three vertical cocoon-threads were used, their supports being made invisible by screens. The two outermost were fixed at like distances from the frontal plane; the middle one was to be so placed that it appeared in the same plane, the eyes being fixed and symmetrically converged. Under these conditions the middle thread must actually lie behind the plane of the other two, if they are near the eye of the observer, in front of it if they are farther away. That is, the vertical horopter is only at one particular distance a plane; nearer, it is a surface concave toward the observer; farther away, convex.

This fact, as the author shows at length, *can* be explained under the assumption that retinal space-values are stable, provided we assume that, of the angles formed in each eye by the two lines of direction with the line of regard, the nasal angle is constantly greater than the temporal. Helmholtz, however, believes that it admits of explanation only under two assumptions: (1) a falling away of a vertical disparity, which when present corrects the illusion; (2) a false estimation of distance. This explanation involves the assumption of *variable* space-values. Its two parts are separately examined.

1. If very minute scraps of paper ($\frac{1}{2}$ sq. mm) were fastened in irregular order on the vertical threads, thus furnishing the otherwise lacking vertical disparity, this new factor made no difference in the results; the placing of the middle thread remained as before. So, too, if the threads were made invisible, a single bright scrap being fastened on each at different heights. The same was true if horizontal threads were placed directly in front of the three vertical threads (again visible, and without the paper scraps). But if the apparatus was so arranged that the pushing back of the middle vertical thread carried

back with it the middle parts of the horizontal threads, the result was changed: they no longer appeared in a plane when in the same position as before, and it was impossible to find a position in which they did so appear with any definiteness.

The other experiments prove that the vertical disparity given by the horizontal threads cannot account for the results obtained in this last experiment. Their explanation lies rather in the fact that the pushing back of the horizontal threads gives an *empirical* factor, namely, perspective, which influences the estimation of distance. But, as was pointed out before, not empirical factors, but only the primitive sensations, have value in deciding the main question. Now in the experiment on which Helmholtz relies to prove the influence of vertical disparity, gold pearls were so fastened upon vertical threads that the resulting estimations were based, not upon this disparity, but upon perspective, and perhaps to some extent on size of retinal images: both of them empirical factors. The same is true of Helmholtz' experiment with stereoscopic figures. It is clear, then, that vertical disparity is without influence on localization in distance.

2. The second factor in Helmholtz' explanation is that the false estimation of the distance of the middle thread causes us to make a corresponding false judgment as to the relative position of the side threads. In case there are no vertical disparities present, the judgment must be determined by this factor alone. There are two types of illusion with respect to distance of objects: (*a*) when the fixed point is localized in accordance with the convergence of the eyes, but this convergence is not adjusted to the real distance of the object, e.g., by the use of prisms, etc.; (*b*) when the localization does not accord with the convergence, even when this is adapted to the actual distance of the object: when two stereoscopic pictures are joined, the object does not appear at an infinite distance. These two cases must be separately experimented with to see if they support Helmholtz' conclusion. And in these experiments we must take care that the retinal images suffer no change, in spite of any other change in the conditions of the experiment.

(*a*) If two systems, each of three vertical cocoon-threads, are looked at with parallel lines of regard, and thus fuse into apparently one system, its apparent distance will coincide with that of a screen which may be placed at varying distance behind them. Thus the convergence remains constant, while the apparent distance varies. If then the middle thread be so placed that it appears in the plane of the other two, it still appears in that plane if the apparent distance of the plane is changed. When the convergence remains constant, the form of the vertical horopter is independent of the apparent distance of objects lying within it.

(*b*) By the use of a haploscope specially designed for the purpose, it was proved also that when the apparent distance remained constant or very nearly so, the greatest possible variation in convergence was attended by no change in localization with reference to the horopter. All the illusions appealed to by Helmholtz can be reduced to one of these two types, or to a combination of both. In one experiment by Helmholtz, however, namely, that with prisms (*Phys. Op.*, p. 657), the localization with respect to the horopter does vary when the convergence varies; but this is due, not to the variation of convergence, but, as is shown by an extended examination of the influence of the reflections and refractions of the prisms, to variation in the retinal image of one eye,—a factor which causes such results even without change in convergence.

These facts, then, prove that the localization of a point relatively to the horopter must be regarded as a physiological function of a particular pair of retinal points. To such a pair we can ascribe a space-value and maintain that this space-value is stable. As Hering had already assumed, in order that a point seen by such a retinal pair may appear in the horopter, the two lines of direction of the external point must form with the corresponding lines of regard angles of which the nasal must be greater than the temporal; i.e., breadth-values increase faster on the outer retina than on the inner. This agrees with the fact that, in monocularly bisecting a line, the portion whose image falls on the inner retina is made longer than the other. Finally, binocularly singly-seen objects are not seen at the point of intersection of their lines of direction; the position of the visual object does not coincide with that of the corresponding real object. But this illusion is not an illusion of judgment in regard to the content of sensation, as Helmholtz thought, but one in regard to form and situation of the actual object.

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ÆSTHETICS OF FORM.

Zur experimentellen Aesthetik einfacher räumlicher Formverhältnisse. LIGHTNER WITMER. Philos. Studien, IX. 1. 96–144, 2. 209–263. Also separately published by Wilhelm Engelmann, Leipzig.

The author of this investigation made use of figures cut from cardboard—rectangles, triangles, etc.—and of simple linear figures drawn on paper—crosses, rectangles, vertical lines divided by dots, and others.

All the figures of a single group or series were placed before the subject in a serial order such that the mathematical proportions of the parts of the figures varied by a constant amount between the ratio 1 : 1 and 1 : x (x being any large number). For example, in a series of crosses comprising perhaps 20 to 30 crosses, the length of crossbar and vertical remaining constant, the crossbar moved 1 mm in each figure from the centre point of the vertical to a point 1 mm from the top of the vertical. As far as practicable or necessary for the purposes of experiment, every possible mathematical ratio is represented by a figure, whose place in the series is determined by the mathematical proportion of its parts. This method permits of the easy observation of the relative increase or decrease in the æsthetic feeling attaching to the regularly-increasing proportions, and is not open to certain objections that may be made to the method of choice as applied by Fechner to arrive at the æsthetic value of the 'golden section' and the simple mathematical proportions of the musical harmonies. Each series comprised from 15 to 30 figures, and results are given in detail from 65 series. Other series were constructed and used, but no report is given of results, as it was found that those series in which the linear magnitudes under consideration did not stand at right angles carried the investigation too far afield, through the multiplication of variable conditions.

The 65 series are classified under the following groups: 1. Proportional division of a straight line (13 series). 2. Two lines meeting to form a right angle (2 series). 3. A line perpendicular to a second line at some point other than the ends of the latter (8 series). 4. Cross-figures (11 series). 5. Various inclosed figures—ellipses, rectangles, triangles (12 series). 6. Multiple proportion in simple figures (7 series). 7. The same in more complicated figures (7 series). 8. Figures to investigate the dependence of the pleasing quality upon absolute size. Of these groups 1, 2, 4, and 5 form the experimental basis for the main conclusions of the investigation, but all groups are reported upon, and the results tabulated, with no omission of individual variant results. Sixteen tables present the results of the judgments of the 14 subjects taking part in the investigation. An additional table gives the average variation of the individual judgments of each of seven subjects from the average judgment of each, and the average departure of the judgment of each subject from the average judgment of all subjects. A section of the paper is devoted to some hitherto unpublished experiments of Fechner's, presented with three tables of results from manuscripts kindly loaned the author by Frau Fechner and Fechner's literary executor, Prof. Dr. Kuntze. Another section concerns itself with a theoretical consideration of methods, containing also six tables of control experiments. Still another is devoted to a consideration of Fechner's methods; another to Zeising's speculations and investigations; an introductory section to a short historical review, and a final section to a consideration of possible interpretations of the results.

These results go to show that the æsthetic value of simple visual forms may be represented by a curve—the abscissas representing in regular succession all proportions from 1 : 1 to 1 : x (x being an indefinitely large quantity), and the ordinates denoting the degree of subjective pleasure or displeasure (agreeableness or disagreeableness) attaching to a given proportion; or, looked at from another point of view, the objective æsthetic value of the proportion. For all groups of figures and for all positions of the figure there are but two even relatively most pleasing proportions—the ratio 1 : 1, or *symmetry*, and a ratio that lies between 2 : 3 and 1 : 2, the '*most pleasing proportion*,' to designate which the author suggests a symbol ○, to be distinguished from Fechner's symbol ⊙ for the mathematical ratio of the harmonic section. The curve begins as a straight line at 1 : 1 (which represents that a range of ratios is perceived as 1 : 1); it is interrupted, to begin with a negative or diminished ordinate value at a ratio that appears just 'off' from 1 : 1 (about 1 : 1.18 in rectangles); from this point the curve rises to a maximum at 1 : 1.63, whence it falls gradually but continuously to 1 : x. Symmetry in all figures stands so apart from all other proportions that a comparison between it and any other proportion on the same terms as between the other proportions among themselves is not justifiable. Cutting off the straight line of the subjective ratio 1 : 1, the remainder it is proposed to call the 'curve of æsthetic proportion.' The maximum of this curve in nearly all series falls between 1 : 2 and 2 : 3, and the average of all results places it so near the 'golden section' (○ = 1 : 1.63 and ⊙ = 1 : 618) that this formula might justifiably be employed to express in mathematical terms the 'most pleasing proportion.' But its use would be in so far objectionable, as it would carry with it the implication that the æsthetic value of the 'most pleasing proportion' is dependent upon the mathematical properties of the golden section. If the 'most pleasing proportion' is to be expressed in mathematical terms, the ratio 3 : 5 is suggested as being an approximate expression of the results of this investigation, free from unnecessary and false implications.

Summarized, the results of this investigation are as follows: 1. The most pleasing proportion is a ratio 1 : 1.63, or expressed in whole numbers, approximately 3 : 5. In reality it is more correct to speak of a range of most pleasing proportions, as the curve falls but slowly to both sides of the maximum. 2. Symmetry and Proportion are æsthetically quite unlike, and their pleasing character is due to wholly different causes. 3. The pleasing character of proportion is not to be explained by any association either in the individual or in the race. 4. An explanation of the 'most pleasing proportion' is not to be sought in the mathematical qualities of the 'golden section,' i.e., in an equality of ratios. 5. Still less is the assumption justifiable of a mathematical formula as the mystical constructive principle of the universe, the more perfect expression of which constitutes the basis of the beauty of visual forms. 6. To consider æsthetic proportion as a '*most pleasing difference of parts*' is justified by the results of the present investigation. 7. No explanation is offered to show why just this amount of difference is most pleasing; future analysis and experiment will probably unravel a multiplicity of factors, physiological and psychological.

AUTHOR'S ABSTRACT.

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MUSIC, SPEECH AND SONG.

Psychologie du musicien. L. DAURIAC. Rev. philos., XVIII. 449–470 and 595–617. 1893.

I. The Evolution of Musical Ability.—Music is not an imitative art. The so-called 'music' of birds consists of noises rather than tones; the discrete scale is a human invention. Music in its higher development is essentially modern. Historically, the composer and virtuoso precede the 'amateur auditor.' At present the 'amateur auditor' generally precedes the virtuoso, and the composer is the latest stage of all, though there is no strict law. The composer is so far influenced by the productions he has heard that they mould his style, without necessarily impairing his originality; this causes the historical progress in music and often produces local coloring.

II. The 'Ear for Music.'—The appreciation of differences in *pitch* is the test of musical appreciation. This seems to grow more delicate in the race and in many individuals. A false note is more readily detected (1) the longer the tones last; (2) the larger the number of concomitant tones; (3) the greater their intensity. Distinction of *timbre* is not essential to an ear for music; it is largely due to voluntary training. The difference between classic and contemporary orchestration is perhaps owing to our present greater knowledge of the physiological effects of various timbres. The ability to distinguish the relative *intensity* of tones seems to be always present. Some persons distinguish rhythm who confuse all airs having the same rhythm.

The writer distinguishes between *music-deafness* and *tone-deafness.* The former is inability to appreciate music (æsthetically), while distinguishing between tones ; the latter is inability to tell tones apart.

H. C. WARREN.

PRINCETON.

Internal Speech and Song. J. M. BALDWIN. Philos. Review, II. 385–407. July, 1893.

The author begins with the question : Can the speech-centre be innervated directly by the auditory or other sensory centres, or must the kinæsthetic word-centre always stand between? Surveying the evidence with some detail, he concludes that there may be a direct flow from the auditory or visual centre to the motor speech-centre, the kinæsthetic speech-centre not necessarily being excited on the way. Speakers are probably of two types, sensory and motor, the latter, judging by the analogy of reaction-time (which is usually more rapid when the attention is bent upon the movement), being presumably the more rapid in their utterance.

When a man is habitually motor or habitually sensory, is this because of a mere habit of his attention ?—or are there native motors and persons who are natively sensory? Prof. Baldwin says that we cannot be *exclusively* either motor or sensory in our reactions because of the necessary neural circle by which all sensory attention overflows into motor adjustments that come back in turn in the shape of an augmentation of sensation. He formulates a 'law of sensori-motor association,' as follows : *Every sensational state is a complex of sensor and motor elements, and any influence which strengthens the one tends to strengthen the other also.* But our various attentions and memories do not develop simultaneously. Motor speech-ideas are preceded by visual ideas of objects, and these by auditory ideas of words understood. Thus the auditory and visual memories get a good 'start' on the motor ones. Unliterary people may sometimes remain 'audiles' all their life. Others, and these apparently the larger number, grow into motors. The reaction-time of a sensor will probably be shorter by the sensory method, that of a motor will be shorter by the 'muscular' method—thus Prof. Baldwin would explain the results of some recent observations which disagree with those at first obtained. Passing from internal speech to internal song, the author seeks a theory of tune-recall which shall account for our ideas of pitch, rhythm, and timbre. There is a motor type of musical memory which requires to think the words or hum the rhythm inwardly before the tune is realized in fulness. In some cases the tune must even be associated with a particular instrument and with the movements made in playing it thereupon. But that this is only one type among many is shown by the fact that musical recognition and expression may precede verbal recognition and expression in childhood, and may be retained when the subject has fallen a victim to verbal aphasia, either sensory or motor. That musical recall can be auditory is also shown by other facts, especially those which relate to the recall of pitch. v. Kries considers that the 'absolute' recognition of a note's pitch is due to association with the note's name. Obviously it ends in this where the note is named; but Prof. Baldwin suggests that the deeper basis of mere recognition and mere recall of a note and its pitch as something familiar consists in the revival of those motor associates of the note which are involved in adjusting the attention to it. "When a presentation comes a second time into consciousness, it is adjusted to more easily because its apperception in attention proceeds upon a basis of ready-formed association. This relative ease of adjustment is felt as the subjective aspect of recognition."

This article (like much of its author's writing) is in places deficient in perspicuity. But it is important, apart from its richness in details, because it offers a basis of mediation between the two theories of Recognition over which Höffding and Lehmann have recently waged war. One theory, stated in its radical form, says that a thing looks familiar to us when it recalls to us its past *self*. The other theory says it looks or sounds familiar when it recalls its past *surroundings*. The difficulty with the latter view is that the supposed surroundings fail to become explicitly conscious where the recognition is confined to the bare 'sense of familiarity.' How do we know, then, that they are at all tending to revive? But Prof. Baldwin, in making them sink to the level of the mere motor associates of former acts of attention, gives a good reason why our consciousness of them should be so indistinct and why at the same time we should so unmistakably greet the sensory experience which they accompany as one already 'ours.' W. J.

EPISTEMOLOGICAL.

Die moderne Energetik in ihrer Bedeutung für die Erkenntnisskritik. KURD LASSWITZ. Philos. Mon., XXIX. Hefte 1–4.

The author adds in these articles another chapter to his history of modern atomism.* He had reached in his history the conclusion that physics find in the kinetic atomism an ideal carrier for its details.

* *Geschichte der modernen Atomistik*, K. Lasswitz, Hamburg, 1891.

The development of the theory of energy, especially that which it has received at the hands of Professor Ostwald of Leipzig, obliges him to at least restate this proposition if he is to justify it in the face of this most modern theory.

Herr Lasswitz prefaces his discussion by a deduction of the concepts of quantity and quality based upon a chapter of Dr. Natorp's *Logik.* Starting from the manifoldness and unity which are the fundamental aspects of all phenomena of nature, the author defines quantity as that property of things by which they may exhibit a difference without a change of the unit—the difference of magnitude. From the standpoint of quantity the possibility of comparison of things consists only in their manifoldness, in so far as multiplicity may be without difference. Quality, on the other hand, is that property which involves a difference of units—every quality is a law of the formation of a magnitude. In virtue of the property of quantity things differ only so far as they can be brought under a single unit; in virtue of quality things differ in so far as they must be brought under different units. The three categories of quantity are unity, plurality, and totality, in which the third category is result of combination of the other two.

Corresponding to these we have three categories of quality—identity, difference, and variability. These depend upon the fact that every qualitative phenomenon is a continuous one in nature. To obtain therefore a condition which shall be identical with itself we must have recourse to the infinitesimal concept. But even when asserting this as identical and therefore different from other states or conditions, we must define it by the law of the change from one state to another.

Color as a qualitative condition of things must be studied in the spectrum where it is a continuous phenomenon, continuous by changing from the most saturated red to violet. If by the infinitesimal concept we fix one portion as identical, we can define it only in terms of the law of change, as ordinarily expressed in the number of vibrations. This gives us variability as the concept of quality, by which it can be treated as a magnitude and so become an object of study for physics. States made up of like units and units constructed upon the concept of variability form the subject-matter of physics. This gives the phenomena of nature as categorically determined, but does not give it as actually existing. This element which involves the objective existence of a state or condition is energy. This is first defined as that magnitude in virtue of which equivalence exists among the 'forces of nature.' The three categories of relation which are postulated as involving existence are those of substance, causality, and system.

The author finds the reality of the category of substance in the conservation of energy, i.e., in the fundamental unity which must be posited as underlying all the manifoldness of the natural phenomena. The reality of the law of causality is identified with the law of the compensation of intensities which the theory of energy postulates as the law of all change. Two factors determine the amount of energy (*Energiemenge*) in any spatial configuration—its so-called capacity and its intensity. Given a number of energies—that of mass, of warmth, of electricity—any one spatial configuration may have relatively varying capacities for each or they may (as in many cases) be equal for a number. The intensity of this energy, however, may be in a state of constant change within the spatial configuration or between different spatial configurations or bodies. The heat may equalize itself throughout a single body or it may be received from another. On the other hand, the differences of intensities in different portions of a body may be compensated by different forms of energy. The condition, then, of any change will be that there be present uncompensated differences of intensity. The relative spatial positions of these differences and their varying intensities will determine the temporal order in which change shall take place. This law of the theory of energy, therefore, expresses the reality in the relation of cause and effect.

The general interrelations of the energies, the laws in accordance with which they are exchanged and compensate each other, and the fundamental principles of their action, give us in the third place an interrelation between the different objects whose substantial and temporal reality are given by the laws of energy, which the author expresses by the relation of system. The unified groups of objects which exist in the world—its organisms and systems—express this third relation; and by them the whole concept of reality is exhausted.

Instead, then, of the reduction of all the phenomena of nature to terms of mechanical physics, this theory of energy places the different energies upon an equality with each other—expresses each as a qualitative state or condition by the mathematical law of its process. The roundabout methods by which light and heat and electricity are reduced to facts of molecular motion or energy can be dropped, and the mathematically stated facts of each can be brought into correlation with each other by means of the principles of conservation and compensation. In the place of the mechanical unit, the gram, comes the erg, i.e., double the energy which a gram of weight possesses when it moves with the velocity of a centimeter in a second.

There are two difficulties which beset physics, in the light of which Lasswitz discusses this theory. The first is the imputation against modern molecular physics that it arbitrarily reads all other sensations into those of touch, or contact. This imputation the author has strenuously denied in his history of atomism, and welcomes the theory of energy because it shows so clearly that no sensation is laid at the basis of the statements of physics, but that this science is a rationalization of the phenomena of nature in abstract terms—terms that abstract from all qualitative expressions except in so far as these can be found in the law of their changes or processes. On the other hand, the author is confident that the reduction of all energies to the terms of a single one will be a necessary development in the theory of energy, and that this will be the energy of mass in a molecular formation. The aim of physics to seek a unified statement of the world will drive the theory of energy into this. But the means for this expression which this theory offers, i.e., the terms of energies expressed in the formulæ of their laws of change, with the unity of the conservation of energy substituted for that of an underlying substance, he finds superior to those of the mechanical physics which hold the field to-day. The second difficulty is that of the expression of sensation in terms of motion : and here Lasswitz finds also that the theory of energy has removed inconsistencies that could not be avoided by mechanical physics. In the first place physics at once assumes the function of stating the phenomena of nature in abstract terms—those of the rational judgment—not in terms of an extra-existent substance which is read in terms of sensations of touch.

In the second place the unity of its determinations are those of the objectified subject. They are the laws of the content of consciousness abstracted from the ego at its centre. The fundamental inadequacy of the expression of sensation in motion is due to the abstract terms of the science. The question whether, wherever in nature unity in a system is found, we must suppose an ego also, the author leaves undecided.

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NEW BOOKS.

Psychology, Descriptive and Explanatory : A Treatise of the Phenomena, Laws, and Development of Human Mental Life. GEORGE TRUMBULL LADD. Charles Scribner's Sons, New York, 1894. Pp. xiii, 676. $4.50.

Inductive Psychology : An Introduction to the Study of Mental Phenomena. E. A. KIRKPATRICK. Winona, Minn., 1893. Pp. 104. 50 cents.

Le conscience du moi. PAUL CARUS. Alcan, Paris, 1893. Pp. 144.

Philosophy of Reality: Should it be favored by America? JAMES McCOSH. Charles Scribner's Sons, New York, 1894. Pp. vii, 78.

Mental Development of the Child. W. PREYER. Translated from the German by H. W. BROWN. International Education Series. D. Appleton & Co., New York, 1893. Pp. xxvi, 170.

Notes on the Development of a Child. MILICENT WASHBURN SHINN. University of California Studies. Berkeley, published by the University, 1893. Pp. iv, 88.

L'éducation de la volonté. J. PAYOT. F. Alcan, Paris, 1894. Pp. 274.

Les centres cérébraux de la vision et l'appareil nerveux visuel intra-cérébral. VIALET. With Preface by DEJÉRINE and 90 figures. F. Alcan, Paris, 1893. Pp. 355.

The Psychic Factors of Civilization. LESTER F. WARD. Ginn & Co., Boston, 1893. Pp. vi, 232.

Philosophy of History—France. ROBERT FLINT. Charles Scribner's Sons, New York, 1894. Pp. xxvii, 706. $4.

Civilization during the Middle Ages. GEORGE B. ADAMS. Charles Scribner's Sons, New York, 1894. $2.50.

Mes nouvelles conclusions sociologiques. COMTE DE CHAMBRUN. Lévy, Paris, 1893. Pp. 126.

System der Ethik, mit einem Umriss der Staats- u. Gesellschaftslehre. FRIEDR. PAULSEN. Hertz, Berlin, 1894. Verb. u. verm. Aufl. Vol. I, pp. xv, 429; Vol. II, pp. v, 576.

Abnormal Man: being Essays on Education and Crime and related Subjects, with Digests of Literature and a Bibliography. ARTHUR MACDONALD. Circular of Information No. 4. Bureau of Education, Washington, 1893. Pp. 445.

Ninth Annual Report of the Bureau of Ethnology, Smithsonian Institution (1887–1888). J. W. POWELL, Director. With 448 Illustations and 8 colored Plates. Pp. xlvi, 617. Washington, Gov. Print. Office, 1892.

NOTES.

THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

The second annual meeting of the American Psychological Association was held at Columbia College, New York on Dec. 27th and 28th, 1893. In the absence of President Low the meeting was called to order by Professor Butler, Dean of the Faculty of Philosophy of Columbia College, who introduced the President of the Association, Professor Ladd of Yale University. There were in all five sessions, extending from eleven o'clock on the 27th to half-past four o'clock on the 28th.

The following papers were presented: (1) 'The Psychological Standpoint'; Professor Fullerton, University of Pennsylvania. (2) 'The Case of John Bunyan'; Professor Royce, Harvard University. (3) 'Experiments on Visual Memory'; Mr. Warren, Princeton University. (4) 'The Confusion of Content and Function in the Analysis of Ideas'; Dr. Miller, Bryn Mawr College. (5) 'Do we ever Dream of Tasting?'; Professor Murray, McGill College. (6) 'An early Anticipation of Mr. Fiske's Doctrine as to the Meaning of Infancy'; Professor Butler, Columbia College. (7) 'Address of the President'; Professor Ladd, Yale University. (8) 'Accurate Work in Psychology'; Dr. Scripture, Yale University. (9) 'The Problem of Psychological Measurement'; Mr. Mead, University of Michigan. (10) 'The Perception of Magnitude and Distance'; Dr. Hyslop, Columbia College. (11) 'Pain and Pleasure'; Mr. Marshall, New York. (12) 'Pain Contrasts'; Professor Pace, Catholic University, Washington. In addition to these papers, which made up the official programme, informal papers were presented by Professor Münsterberg, Harvard University, Professor Cattell, Columbia College, and Dr. Scripture, Yale University, giving in each case an account of recent research.

Nearly as much time was taken up in the discussion of papers as in the reading of them. The members who took part in the discussion, in addition to those who presented papers, were Professor James, Harvard University, Professor Baldwin, Princeton College, Professor Starr, College of Physicians and Surgeons, Professor Dewey, University of Michigan, Professor Armstrong, Wesleyan University, Professor Strong, University of Chicago, Professor Krohn, University of Illinois, Dr. Witmer, University of Pennsylvania, and Brother Chrysostom, Manhattan College. There were in all thirty-three members present, representing sixteen of the leading colleges and universities of the United States and Canada.

At the business meeting of the Association it was decided to print proceedings, which should include short abstracts of the papers. Fourteen new members were elected, and Professor Dewey of the University of Michigan was elected a member of the council. Professor James of Harvard University was elected President for the coming year and Professor Cattell of Columbia College, Secretary. Accepting the invitation of President Patton of Princeton College and Professor Baldwin, the Association adjourned to meet at Princeton on Dec. 28th and 29th, 1894.

J. McK. C.

Miscellaneous.

The account of the Psychological Laboratory of Harvard University prepared by Professor Münsterberg and issued by the University as part of its exhibit at the Chicago Exposition is an extremely useful pamphlet for those having charge of psychological laboratories. In addition to an account of the laboratory (with illustrations), a complete list of its apparatus, and the subjects of twenty-three researches in progress, it contains the names and addresses of seventy makers of, or dealers in, psychological apparatus, and an extended bibliography of the literature of experimental psychology.

In addition to the comprehensive treatise on Psychology by Prof. Ladd, just published by Messrs. Charles Scribner's Sons, systematic works are announced by Mr. Stout, St. John's College, Cambridge, Professor Müller, Göttingen, and Professor Ebbinghaus, Berlin.

Messrs. J. B. Lippincott & Co. have in press *System of Diseases of the Eye*, edited by Professor Norris and Dr. Oliver of Philadelphia. The work is written by about sixty American and foreign authors, and includes the fullest treatment in English of the development, anatomy, and physiology of the eye and of the psychology of vision.

After Oct. 1894 the *Philosophische Monatshefte* is to be edited by Prof. Benno Erdmann of Halle and to be published in conjunction with the *Archiv für die Geschichte der Philosophie* by Reimer in Berlin. The two journals will be conducted on a common plan and will supplement each other, the former being devoted to systematic philosophy and the latter to history of philosophy.

Dr. L. E. Hill, Associate Professor of Physiology in University College, London, is offering during the Easter term of the current year a course of lectures on Physiological Psychology.

M. Alfred Fouillée, author of *Psychologie des idées-forces*, etc., has been elected a member of the French Academy of the Moral Sciences.

MSS. intended for publication in The Psychological Review during 1894 and books, etc., intended for review, should be sent to Prof. J. McKeen Cattell, Garrison-on-Hudson, N.Y. From Oct. 1894 to Oct. 1895 they should be sent to Prof. J. Mark Baldwin, Princeton, N. J.

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THE PSYCHOLOGICAL REVIEW.

FREEDOM AND PSYCHO-GENESIS.

BY PROFESSOR ALEXANDER T. ORMOND,

Princeton University.

There is a tendency in the thinking of the time to evade the question of the freedom of the will. Some excuse themselves for this neglect on the plea that the issue has become antiquated or exploded. But so long as the sense of responsibility for his actions survives in man, the question of freedom will remain central for him and his interest in its solution will be vital. We may assume then that neither the psychologist nor the metaphysician can waive the responsibility of its consideration.

Much of the perplexity that surrounds the question arises from the absence of any definite concept of the nature of the subject under debate. Usually there is in the minds of both the asserters and deniers of freedom a kind of vague apprehension that it is somehow inconsistent with the idea of law, and that a world of freedom would be virtually the same thing as a world of chance. To a mental state like this the alternatives are chance and fate, and the only escape from the iron clutches of an all-devouring necessity seems to be through a repeal of the law of causation and a plunge into 'primal eldest chaos.'

The dilemma which thus arises supplies a problem to the psychologist, although the source of the difficulty is partly extra-psychological and consists in the assumption that mechanical law or determination by other is the only conceivable type of orderly activity, and that it must be extended over human volition, unless we are prepared to regard the will as lawless. The resources of psychology in dealing with the question are both direct and indirect. The direct method of approach is through the analysis of the activity of choice as it manifests itself in consciousness. If we separate this analytic business from all questions of the remote antecedents of choice and set ourselves to obtain as adequate an intuition as possible of the actual factors which enter into a present act of volition, we shall, I think, reach something like the following conclusion. In the first place the idea of motiveless choice, for us men, must be dismissed to the limbo of exploded philosophical myths. Motivation may be assumed as a universal law of choice, and the initial question will be to determine the mode of the operation of this law. Here we take the first step that lifts the issue above the plane of both fate and chance. Psychological analysis proves the immanent character of all normal motivation. Whatever relation the remote grounds of our actions may bear to us, the immediate determinants of choice and action must, in order to influence the will, become internal as parts of the energy that wills and chooses. Determination here is not external but internal. This conclusion taken in connection with two additional considerations will suffice to give a fairly adequate notion of the nature of the voluntary function.

One of these is the selective character of choice. True choice is always a case where one is taken and another left. There are, it is true, influential psychologists like James who regard ideomotor action, that is, immediate reaction upon presentation, as the type of all volition. Against this position the objection holds, I think, that it reduces all choice to immediacy and leaves no place for deliberation. But the choice that we mortals know most about is a mediate function which operates through selection of alternatives. And selection of alternatives involves a two-sided process, conscious annulment of ends as well as conscious self-commitment to the end that is chosen. The remaining feature of choice that is vital to it is the power of arrest which the mind is able, through its command of attention, to exercise over the forces that are impelling it to volition. Through this power of arrest the mind is able to effect a stay of the voluntary proceedings until it has collected its scattered forces and is in a position to act as a unit. Thus, in what we may call normal choice the determining motive is the whole self that chooses,* while abnormal forms of choice would arise as departures and aberrations in various ways from this normal standard.

This is perhaps as far as the direct analysis of consciousness can take us in determining the nature of choice, but it is far enough to justify several important conclusions. The first of these is that the activity of will cannot be subsumed under the category of mechanical causation whose form is determination by other, but that in will we come upon a form of activity that is self-determining. We have seen that the immediate antecedent of choice, when it is normal, is the whole present self. In choice then the mind simply determines itself from one state to another. If we represent the two states by a and b and the activity of choice by x, every case of normal choice will involve the self-moving of the mind from a to b through function x. The causal antecedent of x is, therefore, the mind in state a, while the consequent is the mind in state b, and x is the activity or movement in which the transition is made. Normal choice is, therefore, self-movement and not movement by other. Another conclusion that follows from the above analysis is that fatalism rests on a false idea of the relation of a man to his own choice. The fatalist is one who denies his own agency in volition. The only type of determination, in his view, is determination by other. He, therefore, makes a false diremption between himself and the determining causes of his action and conceives himself to be a mere puppet in the hands of God, Nature, Fate, or whatever his Absolute may chance to be. But if the immediate antecedent of choice is the chooser himself, and if choice is self-determination, the presupposition of fatalism falls to the ground; for, however a man's choice may be determined, it cannot be that he is a mere spectator of the drama, or that he is run by alien forces that act without his own assent.

Self-determination is freedom: or, if we regard it as a type

* Two interesting discussions of the relation of motive to choice are Baldwin's—*Hand-book of Psychology*, Vol. II.: *Feeling and Will*, pp. 352–376; and Hodgson's—*Mind*, April 1891: *Free Will: an Analysis*.

of causation, it is free causation. That freedom is realized, therefore, in the form of volition is a psychologically verifiable fact. But in arguing the question we have distinguished the present act of will from its indirect antecedents and conditions. They are, however, never separate in fact, but the present choice is, in some sense, what it is, because of its antecedents. This changes the issue into a question of predeterminism. It may be demonstrated that the present choice is self-determined, and at the same time the self that chooses may be predetermined by its antecedents. We may thus escape fatalism and still find ourselves in the clutches of necessity.

It is clear that the issues involved in this phase of the question cannot be settled by an appeal to the individual consciousness. The problem of predeterminism is one that involves the factors of heredity and environment, and the point to be debated here is the relation of the present self that chooses to these predetermining agencies. At the basis of the inquiry rests the fact of a developing series the parts of which are bound together by the law of causation and all of which are, therefore, dependent on the chain in which they constitute individual links.

Now, the series with which the psychic nature of man is most completely identified is the biological. Man is a living being and his psychic activity is a species of life. This does not, however, reduce psychology to a branch of biology, but rather comprehends the biologic activity in that of the soul, just as the intelligence of the animal is comprehended in that of man. The term that is central in the biological series is the germ-cell out of which the organism develops and through which it propagates its species, and it is in connection with it that the bearings of heredity and environment need to be primarily estimated. Of the two factors, that of heredity is clearly the more fundamental, since it is through its agency that each successive environment is supplied with the special material upon which its modifying forces are to play.

How then are we to conceive heredity? It is clear that the germ-cell is the medium through which persistent effects must be produced. But at the very threshold of the inquiry into the nature of heredity, biologists have split into two contending camps known as Neo-Lamarckians and Neo-Darwinians, the leader of the latter school being Professor Weismann, whose whole doctrine of heredity rests on the assumption, which is beyond proof, that the germ-cell out of which the organism develops, after it has separated from the parent organism and become fertilized, breaks into two parts, one of these developing into the new organism which is open to the modifying influences of the environment, while the other part remains unchanged as the germ of a future organism. Weismann, therefore, denies the modifiability of that part of the germ-cell through which the continuity of the species is maintained, and on this ground denies the transmissibility of acquired characters or modifications. Having virtually eliminated the environment as a factor in development, the Neo-Darwinians have three agents left: (1) new combinations of original characters which are effected through the modes of transmission, sexual or asexual; (2) accidental variations, or the appearance of characters which cannot be accounted for by the first cause; (3) natural selection which tends to eliminate all variations arising through the first two agencies, that are useless or injurious, and causes only those that are positively useful to survive.

Now, a careful analysis of these factors gives us the somewhat startling result that a whole class of variations, those that have no ancestral copies and on which development most directly depends, are left virtually unaccounted for. Darwin himself regarded variations in general as accidental; at least he brought forward no theory of explanation, while the Neo-Darwinians are able to account for some variations by new combinations of ancestral copies, but they have no adequate explanation for that large class of changes which the opposing school of biologists are in the habit of ascribing to the modifying influences of the environment.

It is because the Neo-Lamarckian school have command of all the Weismannian resources and are able in addition to fall back on the modifying activity of the environment as a cause of original variations, that their doctrine seems to possess a decided advantage over that of their rivals both as a theory of development and of heredity. They reject Weismann's absolute isolation of the germ-cell of future organisms and hold that it is to some degree open to the modifying influences that affect the present organism in which it dwells. They are thus able to reach an idea of the development of organisms that is more flexible than the Weismannian, since the germ-cell is represented as fluent and open to all sorts of modifying influences; as well as more completely mechanical, inasmuch as the results are represented as arising out of a long series of almost infinitesimal changes produced by the varying play of environing forces.

The functions of heredity and environment will be most adequately conceived when considered in their relation to the germ-cells out of which the successive organisms develop. We have in the germ-cell a biological unit which contains the stored-up potence of a developed life, there being included in this unit as part of its potential, the accumulated modifications of a series of antecedent environments. And this unit containing the results of past modifications is to be conceived as continuing susceptible to all the modifying influences that affect the parent organism in which it is latent as well as to the more effective agencies which play upon it after it has become the active germ of a new organism.

The history of the living organism may be taken as including that of the mind; for whether we regard the mental as involved in the original potence of the germ-cell, or as superinduced upon it at some stage of its development, in either case its fortunes will be cast in with the biological unit with which it is associated and through this connection it will be vitally affected by all those hereditary and environing conditions which influence the organism. Professor Orr in his work entitled *A Theory of Development and Heredity* has made a very interesting contribution to the psychology of the hered itary and environing forces. His contention is that the nervous system stands as a necessary medium between the environment and the living organism, translating the forces of the former into nervous energy, in which form it becomes the working agent in every part of the system. Now, the nervous force builds the organism, especially on its functional side, by means of two psychological laws; namely, repetition and association, and Professor Orr shows in several chapters of his book how in the sphere of psychic activity the operation of these laws leads to the development of habitual responses to the forces of the environment and how these tend to become ingrained in the nervous tissue and to be transmitted by heredity as the organized physical basis of instinct and mental habits.

The logical import of such considerations as these seems on first sight to be the suppression of freedom and the re-instatement of strict mechanical necessity, and this is the conclusion drawn by physiological psychologists like Dr. Maudsley and Professor Ziehen, who dismiss freedom as pure illusion, asserting the connection between choice and its antecedents to be essentially the same as that between a physical cause and its effect. It would be useless to deny that from the common point of view these conclusions are not without some reasonable grounds. If the will of man is strictly predetermined by its antecedents; if its choices are but links in a chain of mechanical causation, it would seem that the fact that the form of choice is self-determination loses most of its value, and I am unable to see how a libertarian could continue to fight for it with much stoutness of heart. But the irony of the situation arises here in the fact that at this point the investigation is usually dropped and the inquirer goes his way thinking he has solved the problem. As a matter of fact he has only succeeded in stating some of its data and the solution is yet to be achieved. In the preceding investigations we have simply been getting at the two sides of our problem. We have demonstrated two conclusions. The first is that all choice is self-determination; that normal choice is the unimpeded and full expression of the individuality of the chooser. Nothing that we have discovered since has overthrown that conclusion. It still holds that man himself chooses and that his choice is not a function of some external necessity. The second conclusion demonstrated is that this self that chooses belongs to a mechanical series and has been helped to its present position by the forces of heredity and environment. Choice is self-determined, but the chooser is predetermined through heredity and environment.

We have to deal then with the two factors, mechanism and self-determination. Any freedom that is open to man must include both. It is clear that if freedom and mechanical causation are mutually exclusive terms, freedom for man is a chimera. Mechanism cannot be expelled from his activity, but is inseparable from its highest equally with its lowest phases. The freedom that is open to man must be one that can be realized through and in connection with mechanism. Is any such freedom possible? In seeking an answer it is to be noted in the first place, that the problem of freedom in this larger sense could only arise to a consciousness that had stumbled upon a dualism and had been brought face to face with the alternatives of a higher and a lower self. When the actual consciously faces the ideal whose claim to legislate for it by imposing upon it a law of duty, it recognizes, the question will inevitably arise as to the practicability of obeying the law of the ideal and realizing the higher life which it enjoins. This was the issue as it presented itself to Kant, and in his attempt to solve it he committed what seems to me to be his gravest theoretic mistake. Kant proceeds on the assumption that the ideas of mechanical causation and freedom are mutually exclusive and that the same system of reality cannot contain both, and he thinks, therefore, that in order to establish the reality of freedom it will be necessary to show that outside of the bounds of mechanism there is a sphere of psychic activity that is unaffected by mechanical conditions. The only conclusion Kant could reach from such grounds was the one he actually drew; namely, that while there may possibly be a transcendent region in which such activity is conceivable, yet so far as actual experience goes we never get beyond the reach of mechanical influences.

This conclusion is instructive not only as to Kant's state of mind, but also as revealing the morass in which so many contemporary thinkers are still floundering. Kant's trouble arose from the fact that while he had a very keen intuition of the mechanical conditions with which the mental life is begirt, he had scarcely any notion at all of psycho-genesis. Otherwise, those forces which seemed to him only to bind and circumscribe would have appeared in a new light as conditions of development. As it was, Kant could only sit and wring his hands and wish that the universe were different from what it is, until in a happy moment it was borne in upon him that the difficulty might be overcome by tagging freedom on to the end of a moral postulate. But this, at the best, turns out to be a sort of device by which morality may comfort itself, the actuality being different. It is not open to the contemporary thinker who has become disillusioned on this point to betake himself to the Kantian refuge, and it has not occurred to him, as yet, to apply the genetic idea to the question of the relation between mechanism and freedom.

The most pregnant application of the genetic idea to the basal problems of psychology that has ever been made is that of Aristotle. It arises through his translation of the ontologic ideas of Platonism into the formal principles of individual things, and his conception of these forms dynamically, as activities which tend to unfold from a mechanical state of mere potence or capacity toward one of actuality or a state of self-activity. This view is involved in his treatment of the three categories, *Δύναμις*, *'Ενέργεια*, and *'Εντελέχεια*. *'Ενέργεια* is the category of self-activity in its absolute form, while *Δύναμις* and *'Εντελέχεια* stand as a pair of correlatives which together embrace nature and relativity. They also represent the opposite poles of a process in which nature is conceived as passing from a stage of matter, or pure mechanical response to external impulsion, to that of soul, in which mechanism is subordinated to the form of self-activity. Soul, in Aristotle's view, is the climax of nature and embraces in its constitution a synthesis of passivity and actuality. This appears in his definition of it as the 'first Entelechy' of a body that has the capacity of life. The fine point of the definition is apprehended only when the dual significance which Aristotle attaches to the term *'Εντελέχεια* is kept in mind. This term, as he uses it, is a sort of watershed between potence and actuality, giving a reminiscent look toward mechanism as well as a prospective glance toward the self-activity of spirit. Soul, then, as the first entelechy of a potentially living organism, is to be conceived at any and every point of its life as embracing a synthesis of polar moments, passivity and activity, potence and actuality, and this synthesis may be regarded as grounding the relations which arise later between the categories of mechanism and spirit, determination by other, and free self-activity. But this is anticipating. Again, Aristotle's definition connects soul with life as a form of its actualization. The highest form of life is soul. This is Aristotle's doctrine. It escapes the dualism of the theory that soul is a distinct principle introduced into the living organism, and plants itself firmly on the ground that life is one, that it is not completely actualized, and that it does not reveal its true and complete nature, anywhere else than in soul. But the point of vitalest interest in connection with the special theme of this paper is the fact that Aristotle's conception of soul and its relation to life enables him to incorporate the principle of development into its very constitution in such a way that it can no longer be adequately represented under static categories. And it is here that the Aristotelian conception of the soul seems to me to furnish a much more adequate and effective basis for psychology than that of Herbart-Lotze, for example, in that it shows more clearly how the genetic method may be grounded in a real principle of psycho-genesis.

I mean by a real principle of psycho-genesis one that not only grounds development as a constitutional law of the psychic life, but also supplies some definite notion of what *psychic* development means. The Aristotelian concept helps to the formation of such a notion in this way. It asserts, not simply that soul-life is a development, but that it is a development of a particular species; namely, of a principle of self-active consciousness, from a state of potence or mere capacity up to a state where all its powers shall have become actual and its nature completely revealed. The nature of the psychic principle and the species of its development are thus to be determined in view of their outcome. If the actualized result is a self-active and self-determining consciousness, then we have the right to say, on the Aristotelian principle, that it was potentially that from the start, and that in every stage of its evolution it was going on to be just that. And without raising any question of transcendent teleology or design, we see how the process is immanently teleological from the beginning.

The value of the Aristotelian insight will be manifest in view of the fact that the two most pregnant ideas in the domain of psychology to-day are these of psycho-genesis and the immanent teleologic character of consciousness. The tendency of the one is to modify static conceptions and to view the soul-life as fluent and progressive; that of the other is to shatter the hard front of mechanism and to reduce it to the position of a servant to a teleological process. The Aristotelian insight enables us to ground these categories in the very constitution of the soul itself. So that when we find consciousness to be a selective principle which is everlastingly in pursuit of ends even when it does not know itself to be teleological, we can rationally ground the discovery in a doctrine of the nature of the soul as a self-active principle whose law is development from mere potence into the actuality of a self-conscious and self-determined life.* And when we find in consciousness a dualistic dialectic between an empirical will and an ideal which utters itself in conscience, we are able to trace this dialectic to the teleological law of psychic development, which is the law of the immanent ideal activity that the psychic process is ever going on to actualize.†

We conclude then that all psychic activity is in its essential nature teleological. What it actually is or realizes, never truly or completely expresses its nature. But its real character only comes out in the light of what it has in it to become, or what it is going on to be. Now in the light of this we ask why freedom should not be teleologically construed. In the former sections of this paper we demonstrated two conclusions; namely, that normal choice is a form of self-determining activity, and that in its connection with heredity and the environment, the self that chooses belongs to a causal series and is predetermined. In view of current modes of thinking the last conclusion seemed to swallow up the first and to leave the life of man in the clutches of necessity. But when

* The Aristotelian idea of soul thus seems to supply a rational basis for James's doctrine of the selective character of consciousness.

† I do not mean to assert that conscience is completely explained as the immanent ideal of the soul. In my work on 'Basal Concepts in Philosophy' I seek to show the relation of immanence to the transcendent. The point here is that conscience on its psychic side utters the immanent ethical end of the soul.

in the light of later conclusions we claim the right to put a teleological construction on the whole process, the clutch of necessity seems to be loosened. For the developing series then acquires a meaning outside of the mere determination of consequents by antecedents. Instead of a soulless corporation, it becomes animated with spirit, and we see that what has outwardly the appearance of dead mechanism becomes a fluent and living organism whose whole significance is the immanent potence which it contains and the immanent end or ideal which it is going on to realize.

It is clear that from the teleological point of view, whose justification has been shown to spring from a profound view of the nature of psycho-genesis, mechanism becomes the handmaid of teleology, and while it conditions, also furthers the immanent end. Heredity conserves the end by preserving and transmitting the gains of individual experiences, while the environing forces supply the necessary stimuli of development. And when we apply these considerations to the problem of freedom it becomes clear that the moment we subordinate mechanism in general to teleology, we thereby subordinate mechanism also to freedom. And instead of standing by and wringing our hands because predeterminism swallows up freedom we may go on our way rejoicing, since our new insight enables us to see that nothing of the sort happens, but that free self-determination is the end which all this hard and forbidding-looking mechanism has had at heart and has been realizing from the beginning. For, just as the end subordinates the means, so freedom subordinates the mechanical agencies through which it is achieved.

There is no reason why psychology when it has committed itself to the genetic idea should stubbornly persist in construing freedom in some absolute sense which is above man and then deny its existence because it is inconsistent with the mechanical conditions of human life. Why should not freedom be construed in harmony with development, and why should it not be teleologically conceived? The questions supply their own answer. The teleological idea of freedom is the only one that a genetic psychology can consistently entertain. For, to genetic psychology conscious activity *is* teleologic activity, and volition is the type of conscious activity on the practical side. Volition is self-determining activity, as we have seen, and self-determining activity is free activity. If free activity is the outcome of mental development and this outcome is the immanent end and meaning of the process, the conclusion naturally follows that the development only achieves its complete reality in freedom.

Now, if we identify freedom with self-activity and construe it teleologically, there are several senses in which the term may be used in its relation to mental development. As potence or capacity for self-activity, it will be a condition of development. As actual self-determination it will be the form of all normal choice; whereas, as the self-determination of the ideal it will be the end toward which development is tending but which it never realizes. But in each and all of these senses its vital relation to experience is evident. Freedom is not a speculative will-o'-the-wisp, but it is something that, in the words of Bacon, concerns 'men's business and their bosoms' in that the possession of it is the condition of their being men, while the realization of it is the great end of rational and spiritual activity.

The doctrine of freedom here developed has also another merit. It supplies a rational ground of distinction between the normal and the abnormal in the sphere of choice. Freedom can be postulated without qualification, only of normal choice. The normal function of heredity and environment is the development of free activity. In other words, the normal is the good. The abnormal will enter as some kind of evil or aberration from the normal standard, and while it will be negative, it will be also real. The abnormal will become a factor in both heredity and the environment, and it will operate as a kind of loading of the dice, and in the development of predispositions to evil, in diminishing and thwarting and turning aside the forces of development. The abnormal will embody itself in organic and functional defects, in ingrained hereditary evil tendencies, in environments which hinder and clog progress. The abnormal thus supplies a special problem to the psychologist as it does also to the moralist and the jurist. But to the psychologist as well as to the moralist and the jurist a correct diagnosis of the normal is a necessary condition of the rational treatment of the abnormal.

THE CASE OF JOHN BUNYAN. (III.)

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V.

The malady was now, after the passage of this acute stage, all the more certainly in possession of the man. The temporary remission was sure to prove deceitful. In Dr. Cowles's patient after once the morbid habits had become systematized, to a degree similar to the one now reached in Bunyan's case, there was apparently no way out of the gloomy labyrinth. Whatever devices were tried led, so long as the patient was under Dr. Cowles's observation, to renewed struggles with conscientious scruples and with ingeniously subtle inner temptations, and the sufferer, whatever her temporary stages of relief, was doomed to walk round and round the charmed circle of doubt, of temptation, of elaborate self-invented exorcising devices, of failure, of self-reproach, and of despair. It was to be Bunyan's good fortune to escape in the end from his tempter. How he was thus to escape, the next and most agonizing of his acute stages was to determine. The sufferer from such morbid systems is at best, as all the evidence shows, in a very serious position. That very strength of certain of his highest brain-functions which is one condition of the development of his weakness as to other functions, makes all the harder the task of teaching him wholly new mental habits. Yet without such wholly new habits he can never escape. Hence the evil prognosis which most observers now unite in attributing to this type of disorder, viz., to the chronic malady of insistent impulses with intercurrent acute stages. But there is one rather desperate chance which most writers on the subject have, as I think, generally neglected. Suppose there appears, in the life of the chronically affected patient, a new insistent impulse, such that yielding to this particular impulse brings the patient into some wholly new relation to his environment. Suppose, thereupon, that a novel and profoundly different life, even if this be a very painful life, is forced upon him in consequence of his yielding. The result may be a condition of things in which, diseased though he still is, the old cares and temptations are entirely set aside by the fresh experiences given through the new environment. If the patient has now strength enough to bear the pangs and the fresh and strongly contrasted nervous distresses of this changed life, he may actually have time to reform his mental habits before the old 'tempter' is able, for his part, to organize his own inimical nervous tendencies upon the new battle-field. The substituted pangs themselves may then pass before the old are renewed. Then indeed, some day, the old enemy will come back, but the patient will have become, meanwhile, another man, and the whole system of his formerly insistent opponents will have been broken up. He will thus find himself thrown back, in some sense, to the earlier stages of his own case; he will once more have only elementary doubts and fears to oppose. But these his experience will have taught him to circumvent; and so, at any rate with a certain degree of defect, he may have become cured. The elements will survive, but will no longer systematize.

This possible good fortune, to be won, if at all, by passing through the fiercest fire of painful impulse, Dr. Cowles's patient tried in vain to find, when she experimented at pretending to poison herself, or, later, deliberately wounded herself with a pistol, not hoping to commit suicide, but only seeking to expiate her faults, and to get peace from her tempter, through novel pangs. Bunyan, without dreaming of such relief, actually won it through what seemed, at the time, the most hopeless of all the woes that had yet beset him.

"For after the Lord had, in this manner, thus graciously delivered me from this great and sore Temptation . . . the Tempter came upon me again, and that with a more grievous and dreadful Temptation than before. And that was, *To sell* *and part with this most blessed Christ, to exchange him for the things of this life, for anything.*"

The new temptation had its own typical mental context, different from that of the previous stage. This was now no single member of a 'flood of blasphemies.' It stood nearly alone, as equivalent for all the rest of the earlier temptations. Still, however, the impulse to *sell Christ* was merely an imperative motor speech-function. No other word seems ever to have substituted itself for the word *sell*; and the only further act involved in yielding to the temptation was a purely formal inner assent to the 'selling.' The proposed transaction involved, as a matter of course, no actually conceived exchange whatever. Nevertheless, in a most interesting fashion, the imperative impulse now appeared as a reflex, which tended, in consciousness, to enter into a sort of 'agglutinative' combination (to use one of Wundt's well-known adopted phrases), with any object of passing perceptive interest; so that the special form of the experience was that the tempter moved Bunyan to *sell Christ for this* or *for that*, whatever the insignificant thing might be that Bunyan was at the moment attending to, or handling, or dealing with in any active way. The painfulness, the associated fear, and the violence of the thought, were all of the most intense sort; and this reflex character made the temptation infect Bunyan's whole life most horribly; "for it did always, in almost whatever I thought, intermix itself therewith, in such sort that I could neither eat my food, stoop for a pin, chop a stick, or cast mine eye to look on this or that, but still the temptation would come, *Sell Christ for this*, or *sell Christ for that; sell him, sell him.*"

The struggle this time very soon led Bunyan to that grave stage where the sufferer from insistent impulses resorts to apparently senseless motor acts that possess for him an exorcising significance. "By the very force of my mind, in laboring to gainsay and resist this wickedness, my very body also would be put into action or motion by way of pushing or thrusting with my hands or elbows, still answering as fast as the destroyer said, *Sell him; I will not, I will not . . . no, not for thousands, thousands, thousands of worlds.*" This kind of elaboration rapidly grew to its own hopelessly extravagant extremes. But in vain. A few added doubts, of the old inhibitory type, meanwhile appeared in the background, but the tempter had now, so to speak, learned his game, and had no need to waste his forces upon general devices of inhibition. This one suggestion was enough. The loathsome triviality of the motor impulse itself, in its pettiness, and the vast dignity of the eternal issues imperilled, as Bunyan felt, by its presence, combined to give the situation all the dreadful and inhibitory features that had earlier been spread over so wide a mental range of evil interests.

"But to be brief, one morning, as I did lie in my bed, I was, as at other times, most fiercely assaulted with this temptation, . . . the wicked suggestion still running in my mind, *Sell him, sell him, sell him, sell him,* as fast as a man could speak. Against which also, in my mind, as at other times, I answered, *No, no, not for thousands, thousands, thousands,* at least twenty times together. But at last, after much striving, even until I was almost out of breath, I felt this thought pass through my heart, *Let him go, if he will!* and I thought also that I felt my heart freely consent thereto. Oh the diligence of Satan! Oh the desperateness of man's heart!"

"Now was the battle won, and down fell I, as a Bird that is shot from the top of a tree, with great guilt, and fearful despair. Thus getting out of my Bed, I went moping into the field; but God knows, with as heavy a heart as mortal man, I think, could bear; where, for the space of two hours, I was like a man bereft of life, and as now past all recovery, and bound over to eternal punishment."

VI.

The nervous crisis thus passed served to introduce a condition of extremely lengthy, quasi-melancholic, but to Bunyan's consciousness wholly secondary, depression. The hopeless sin was committed. Like Esau he had sold his birthright. There was now 'no place for repentance.' This, the third stage of the culmination of the malady, was marked by an almost entire quiescence of the insistently sinful impulses; for what had the victorious tempter now left to do?

There were no more minor hesitancies, no loathsome motor irritations. One overwhelming idea and grief inhibited all these inhibitory symptoms. The insistent associative processes with the scripture passages became, however, for a good while, all the more marked, automatic, and commanding. Thus the whole mental situation was profoundly altered. The secondary melancholic depression expressed itself occasionally in precordial anxiety. "I have felt also such a clogging and heat at my stomach, by reason of this my terror, that I was, especially at some times, as if my breast bone would have split asunder." But Bunyan even now never long lost his dialectic skill; and hopeless as seemed his case, he from the first set about trying to think of a way of escape from destruction, being throughout 'loath to perish,'—a fact which, viewed in its results, indicates the relative intactness of his highest mental functions amidst all his gloom.

Except for the automatic processes with the scripture passages, Bunyan's condition of secondary melancholic depression had, therefore, despite its depth and its fantastic background, many of the more benign characters of normal grief. It had, at the worst, its occasional remissions. It left his reasoning powers formally unaffected. And it had the painful but really invaluable character that, just because his fate seemed decided, he had a long and almost total rest from the irritating motor processes, whose dependence upon his past habits of conscientious anxiety is thus all the more confirmed. For this restless anxiety, the pretty steady assurance of damnation was now substituted. This, as the event proved, Bunyan's heroic disposition was strong enough to endure, despite the 'splitting' sensations in the breast, despite the long days of grief and of lonely lamentation; despite his inability to get any comfort or help from his few advisers. The case was still grave enough, but this light melancholia proved to be a decidedly kinder disorder than the foregoing one, and it led the way over to recovery.

In the long tale which follows, in Bunyan's Autobiography, and which is largely devoted to the description of the inner conflicts amongst the scripture passages (of whose automatic evolutions poor Bunyan's consciousness was now long the merely passive theatre), there are but few things further to be noted for our purpose. But these are extremely instructive.

The gradual emergence from despair is obviously due, on the whole, to the *vis medicatrix naturæ*. Bunyan's general physical health gradually improved. His conscientious habits of life, freed now from the tempter's teasing interferences, had a chance to become healthily fixed and unconscious. He grieved too deeply to long for distractions, and never thought of returning to his youthful sins as a relief from despair. The doubts and other motor inconveniences were of course still in the background of his mental life, but it is interesting to note how, whenever they appear, they are now simply overshadowed and devitalized by the fixed presence of the ruling melancholic ideas. The tempter is thus at length known as a relatively foreign and mocking other self, whose power over Bunyan's will grows less even while his triumph is supposed to be final. He 'becomes humorous,' as Froude observes. Bunyan, so the tempter suggests in his old metaphysical way and with the old doubting subtlety—Bunyan had better not pray any more, since God must be weary of the whole business; or if he must pray, let it be to some other person of the Trinity instead of to the directly insulted Mediator. Could not a new plan of salvation be devised by special arrangement, the Father this time kindly acting as mediator with the otherwise implacable Son, to meet Bunyan's exceptional case? But such suggestions, which in an earlier stage would have been 'fearful blasphemies,' now have to stand in contrast to the fixed and central grief which constitutes Bunyan's own personal consciousness. Bunyan knows by the very contrast that these suggested words of the tempter are *not* his own. This is the mere fooling of the exultant devil. It is meaningless. For Bunyan is consciously on the side of the grief itself, and the humorous tempter is the sole owner of the blasphemies, which therefore serve all the more to 'confirm' the sufferer in his painful faith. A better device than this for the 'segmentation' of insistent questionings could not have been imagined by any physician learned in the cure of souls. The victorious tempter had unwittingly dug his own grave. He could never again get possession of this man's central self, nor use this brain as a foundation for systematized evil habits.

Another instructive aspect of the slow process of recovery lies in the fact that Bunyan was, towards the end, able, at some moments and despite his always busy dialectic processes, to win that attitude of complete resignation, of abandonment of all feverish conscious strugglings and pleadings with fate,— that attitude which, as experience shows, is so often the beginning of a final recovery from all forms of deeper mental distress. Such an attitude is consistent, as it was in Bunyan, with a good deal of cool consideration, and with much activity of thought, but it was still effectively assumed. There is, for such sufferers as Bunyan, and for many others, a mood of gentler despair that is often essentially healing, because, as compared to their old feverishness, it is peaceful. It is the sort of despair that Edgar Poe has put on record in the admirably psychological lines 'For Annie.' It is the mood that says, to the tempestuous striving self of former days, '*Ich hab' meine Sache auf Nichts gesetzt.*' One is lost; only eternal mercy can save; one finally is content to leave all to fate or to God, and to 'lie quietly,' like the conscious corpse of Poe's poem, glad a little that the 'fever called living is ended at length.' Bunyan is remote enough in type from Poe's lover; and he was never content long to lie quiet. But still, at moments, this essentially curative element also is present in this stage of his experience. The automatic play of the remembered scripture passages became with him more and more complex, imposing, unpredictable,—an inner fate that he often helplessly watched as one watches the breaking of great waves on the beach. Plainly God must be directing the process. Bunyan could only pray that God's will might be done, and hope that so many kind glimpses of light would not have been shown to an utter outcast. 'God and Christ,' he says, 'were continually before my face,' and, painful as the experience was, since he was facing his judge, this kept down, as he himself recognizes, all the old temptations to 'atheism.' At last "I saw . . . that it was not my good frame of heart that made my Righteousness better, nor my bad frame that made my Righteousness worse; for my righteousness was Jesus Christ himself, *the same yesterday, to-day, and forever.*" And 'now,' he says, in narrating this last experience, 'did my chains fall off my Legs indeed.' Such is the healing virtue of true resignation.

The episodes of this whole long final stage were of course numerous and of Protean character. There was throughout, despite the prevalence of the general despair, considerable instability of mood. Intervals of peace, resulting from this or that 'sweet glance' of a 'Promise,' alternated with the wildest fits of gloom. Two or three times the borderland pseudo-hallucinations of speech returned. Once, in particular, at a moment of this sort, the accompanying experience of calm "made a strange seizure upon my spirit; it brought light with it, and commanded a silence in my heart of all those tumultuous thoughts that before did use, like masterless hell-hounds, to roar and bellow and make a hideous noise within me." And this sudden transformation of mood, produced by a comforting voice that was 'as if heard,' was so great that, many years later, though writing in a very cautious and self-critical spirit, Bunyan could not refrain, in a later edition of the *Grace Abounding*, from inserting this incident, and adding his private opinion that this might indeed have been 'an Angel' that 'had come upon me.' Yet no element of actual delusion was, at the time, involved in the experience. As for the scripture passages, their automatic effects were such that Bunyan ere long found himself awaiting with interest what would happen when two, already known and often studied 'words' should, by chance, 'meet in my heart'—an event which might prove to him of the most critical importance, although, beforehand, he could do positively nothing to hasten or to effect this event by any voluntary consideration of the passages. Only when the suggested passages were numerous, and the 'meeting' had already often occurred, could he devote himself, with his accustomed dialectic skill, to considering with care the outcome and its meaning—a thing which, just before his recovery, he learned to do, in some cases, very coolly and with great deliberation.

The passing of this stage of despair was attended, at the end, with many of the usual exaltations and confusions of convalescence. "I had two or three times, at about my deliverance from this temptation, such strange apprehensions of the grace of God, that I could hardly bear up under it; it was so out of measure amazing, when I thought it could reach me, that I do think, if that sense of it had abode long upon me, it would have made me incapable of business."

VII.

The cure had come to pass, but it was, and remained, a cure with a pretty well-defined defect. The tempter could never again obtain control. The diseased habits were reduced to their elements, and were unable to systematize themselves afresh. The elements, however, proved, as one would expect in such a case, too deeply founded in this wonderful constitution ever to be eliminated. At the end of the *Grace Abounding* Bunyan, with the simplest humility, records the temptations to which his soul is now permanently subjected. His moods of spiritual interest and emotion are to a very considerable extent unstable, do what he may. There are times when he is 'filled with darkness,' however much, at other times, he may have been exalted. His heart becomes, at the dark times, 'dead and dry,' and he can then find no 'comfort.' He is also still occasionally tempted 'to doubt the being of God and the Truth of his Gospel'; and this is always the 'worst' of moods. Furthermore, in his preaching, the tempter often besets him 'with thoughts of blasphemy,' which he is 'strongly tempted to speak' 'before the congregation'; or again, a strange confusion of head comes upon him as he preaches, and straitens' him, so that he feels "as if I had not known or remembered what I have been about, or as if my head had been in a bag all the time of the Exercise." More subtle assaults of the tempter also come while he preaches,—condemnations of this or that which he knows it to be his duty to utter, or on the other hand movings 'to pride and liftings up of heart.' For a while after his malady, when he had joined the church, he was tempted to blaspheme during the sacraments. In any of his illnesses, peculiarly black and cowardly thoughts always come. At the beginning of his imprisonment he long felt himself to be a hopeless coward, unable because unworthy to suffer for the faith, and the tempter mocked this weakness with all the old subtlety.

But now—here is the important thing—all these permanent enemies are still, and remain for the rest of Bunyan's life, in no wise uncontrollable. His deeper consciousness is beset, but never overwhelmed, by them. His attitude towards them becomes objective, resigned. They teach him to 'watch and be sober.' They are useful to him, since 'they keep me from trusting my heart.' Of one of his later hours of darkness he says: "I would not have been without this Trial for much. I am comforted every time I think of it, and I hope I shall bless God forever for the teaching I have had by it. Many more of the dealings of God towards me I might relate, but these, *out of the spoils won in Battle have I dedicated to maintain the house of God.*" The words are typical of all the later inner experience of Bunyan; and it is to this spirit in the man that we owe his immortal works.

Of his mental regimen after his recovery a word may yet be said. A wise instinct guided the much-tried wanderer in the darker world to forsake henceforth his solitude, to join himself 'unto the people of God,' to try to be objectively serviceable, and to keep in touch with the needs of his brethren. His gift of speech hereupon soon discovered itself. He was ere long set to preach. His power won multitudes of listeners during all his years passed out of prison. In prison he wrote busily, and preached to his fellow-prisoners at every opportunity. The motor speech-functions, whose inhibition had led to such disastrously rebellious insistent habits, were never again suffered to remain without absorbing and productive exercise. The decidedly healthy self-contempt engendered by the experience of his own weakness only served to make him more objective in his whole attitude towards life. Henceforth he knows every man to be of himself naught. He has therefore, as Froude points out, no favorites, and portrays, in his literary work, Talkative, and Ignorance, and Mr. Badman, with as much cool devotion to the task and with as much artistic faithfulness, as Christian. He spares no one, himself least of all. Yet he sympathizes with every manner of human weakness, for his own inner life has furnished him with a brief abstract and epitome of all human frailty. His mastery is the mastery of the genius who has really entered the Valley of the Shadow and has passed through. Hence the seeming of the man in the eyes of those who knew him in later life, and who could not easily have suspected, in this modest yet commanding presence, the piteous weaknesses of his younger years, had he himself not so instructively told the wonderful story.

Our result can be briefly stated. This is unquestionably a fairly typical case of a now often described mental disorder. The peculiarities of this special case lie largely in the powers of the genius who here suffered from the malady. A man of sensitive and probably somewhat burdened nervous constitution, whose family history, however, so far as it is known to us, gives no positive evidence of serious hereditary weakness, is beset in childhood with frequent nocturnal and even diurnal terrors of a well-known sort. In youth, after an early marriage, under the strain of a life of poverty and of many religious anxieties, he develops elementary insistent dreads of a conscientious sort, and later a collection of habits of questioning and of doubt which ere long reach and obviously pass the limits of the normal. His general physical condition meanwhile failing, in a fashion that, in the light of our very imperfect information concerning this aspect of the case, still appears to be of some vaguely neurasthenic type, there now appears a highly systematized mass of insistent motor speech-functions of the most painful sort, accompanied with still more of the same fears, doubts, and questions. After enduring for a pretty extended period, after one remission, and also after a decided change in the contents of the insistent elements, the malady then more rapidly approaches a dramatic crisis, which leaves the sufferer for a long period in a condition of secondary melancholic depression, of a somewhat benign type,—a depression from which, owing to a deep change of his mental habits, and to an improvement of his physical condition, he finally emerges cured, although with defect, of his greatest enemy—the systematized insistent impulses. This entire morbid experience has lasted some four years. Henceforth, under a skilful self-imposed mental regimen, this man, although always a prey to elementary insistent temptations and to fits of deep depression of mood, has no return of his more system atized disorders, and endures heavy burdens of work and of fortune with excellent success.

Such is the psychological aspect of a story whose human and spiritual interest is and remains of the very highest.

A STUDY OF FEAR AS PRIMITIVE EMOTION.

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It may be considered as plausible that if the first feeling was pain (see *Philosophical Review*, vol. I. p. 433 ff.), the first emotion was also of the pain character. The first representation of an object as painful induced that reaction of mind which we term an emotion, and the painful emotion we call fear. That the first emotion to appear was fear, as fright, seems likely when we consider that the general alertness and defensiveness imperatively required in the struggle for existence is thereby most immediately and simply attained. The acquirement of the power to become frightened is plainly a most important requisite for self-preservation, and thus is indicated as a very early factor in conscious life. An animal being devoured by another may merely suffer pain without any perception of the object as pain-giving and to give pain; but if it attains this perception, there may be added to the stimulus of simple pain that of fright. The direct actual pain may be but small, and so inducing but feeble reaction, as when some less sensitive portion is being injured; but if there occurs a vivid representation of potential pain, fright happens and stimulates most strenuous endeavors, and so rids the animal both of the immediately and the prospectively painful. Thus emotion acts as a complement to simple feeling, and also secures practically anticipatory reaction. Animals which must receive actual injury before experiencing pain are clearly inferior to those which experience emotion-pain before the injury is actually received. Other things being equal, the most easily frightened have, in the midst of many destructive agents, the best chance of survival and of perpetuating their kind.

It is unnecessary to dwell at length on child life and savage life as illustrating the primitive quality and function of fear. The earliest experiences of the child with things are lessons of fear. The burnt child dreads the fire, and thus is enabled to preserve himself from threatened injury. Fear is a primary and most important motive to action in a very wide range of the lower mental life. Those who have observed animals and man in a state of nature are always greatly impressed with the constant and large part which this emotion plays in their consciousness. With the timid and weaker species, like the rabbit and squirrel, it is likely that a majority of their cognitions prompt to fear or are prompted by fear, and with some persecuted races of savages the same may be said.

The necessity and value of anticipatory reaction being acknowledged in the struggle of existence, we plainly see a primitive motive thereto in fear, and the earliest emotional life which we can clearly interpret likewise seems to be fear.

It is sufficiently easy to see the general function of fear and its primitive character, but we find it very hard to make a satisfactory analysis, and to show the exact steps of its evolution. It is obvious, however, in the first place, that fear like other emotions is purely indirect and secondary experience; it presupposes previous painful experience of the feared object. Pain experienced in connection with cognition of object is the basis of all fear. Animals that have not felt pain from man do not fear him. But fear while thus based on previous direct experience is always hindered by simultaneous direct experience, as, for example, sensation. Thus when we, whip in hand, say to a child crying from fear, 'I will give you something to cry for,' we imply the law that direct pain and sensation tend to supplant indirect feeling as emotion. This common expression emphasizes the essential representativeness of emotion, its imaginary nature, as also the supplanting power of direct real experience. The sight of the whip inspires fear in the child who has been whipped, but this fear is in the course of a punishment wholly eliminated by the direct pain endured. The direct experience is thus the basis of every fear, but only as it is cognized, and not felt.

The great difficulty in analyzing fear is in clearly apprehending the mode in which previous experience is utilized. If we could study in ourselves the genesis of a simple emotion we should doubtless be enabled to see the steps by which experience reacts upon itself so as to give a reflex form like the emotion of fear, but this is hardly possible. However, cognition is evolved at the instance of pain, and all objects are viewed not for themselves but in their feeling significance. Cognition is imbedded in feeling, and at first is a mere tone of feeling. Things are not at first known for themselves but solely as sources of pleasure and pain. Things are perceived in and through the feeling which has stimulated the perception. The immediate feeling value of the object is given by the very origin and process of cognition. When an animal is pained by contact with a sharp rock and this pain stimulates cognition of the rock, this is solely on the pain account. Repeated experiences enable the percept to arise at stimulus of less and less pain, and so the proper reaction is accomplished more and more economically.

But how does perception of object appear before any experience of pain from it? How is pre-perception of object in its feeling value accomplished? In emotion as of fear we have cognition attained before the paining and as sign of paining, a wholly premonitory function. But how can in any case the object be perceived before it painfully affects the perceiver? How does mind become conscious of a thing whose painful effects it is not at the time receiving?

Emotion certainly seems the reverse of primary cognition which comes only by stimulus of present pain from the thing, whereas the emotional form includes cognizance of objects before they affect consciousness in pleasure and pain mode. Simple primary cognition arises from the pain; in later forms, on the contrary, cognition conveys impression of the object's pleasure-pain quality, and there results the peculiar disturbance we term an emotion. Instead of rock causing pain before cognition of the rock is accomplished, cognition of the rock, in the emotional stage, causes a painful feeling, emotion of fear. The feeling is here sequent and result of cognition, and not *vice versa*.

How pre-perception of object—that is, perception before in any given case there is direct pleasure-pain experience of it—is gained, can only be determined by an analysis of the way in which representation has been built up.

It is evident that the cognition which is stimulated by a pain and coexists with it and by it cannot serve as the basis of emotion. The object is felt, but there is no feeling *about it.* The thing is given with the immediate feeling value, but there is no sense of potential experience which is above the basis of such early emotions as anger and fear. In consciousness which is only pain *plus* sense of paining object, there is no attitude of mind toward the paining object. It is scarcely possible for us, indeed, to realize this state of mind, for with us to have a pain and a sense of its objective source is to invariably awaken emotion; yet we must believe that at one period in the history of mind emotion was unevolved, and experience was wholly direct and simple.

Pre-perception, the perception of the thing before its pleasure-pain agency is felt, certainly this had some mode of evolution in slow stages through revivals and association. What is the feeling motive of the pre-perception? what is the nature of this perception of experience value in pleasure-pain terms of the cognized object? and how does this perception generate emotion? These are difficult questions. It is plain that the primary cognition which rises by and with the pain is merely cognition of the object, but not cognition of its pain value, which is certainly a complex and later form somehow based on the former. Injury and pain early involve cognition, but this cognition does not from the first involve emotion which implies representation. Knowledge is in its origin a search for pain-bringer, objects are discovered as vehicles of pleasure-pain, but the first cognition could not have been so complex as an interpretation of the pleasure-pain value of object paining, much less a pre-perception of object as about to pain. Cognition at first is merely with the pain, not of the pain; it is accompaniment of the pain, but it does not view the object in relation to the pain. The association of the cognizing and the pain is a mechanical, not a perceived connection which implies consciousness of consciousness. If cognition is, from the first, sense of experience value as well as sense of object, then emotion is indissolubly bound up with the whole course of cognition. That the pain incites to consciousness of object, and that there is experience of pain going on simultaneously is certain, but the pain is not at first attached by the mind to the object. The object is felt, but its significance is not known. Possibly in human conscious life we may have something analogous. If I say, 'I feel a tooth coming,' I may mean a peculiar combination of pain and cognition, a mere feeling the object without emotion. A tooth-ache of this form is certainly extremely transient, for we are soon aware of our experience as such, and interpretations from the past and fears for the future quickly enter, and there is the inextricable tangle of feeling, knowing, willing, consciousness of consciousness, consciousness of self, which is the normal state of ordinary human consciousness. However, there is a simple union of cognition and emotion, which is expressed by 'feeling hungry,' for instance, wherein feeling is so predominant that we naturally use the word 'feel.' In this sense all early cognitions are feelings, and we can say the object is felt when cognition is but a dim and slight event in a mass of pain.

The object is not then from the first invested with its pleasure-pain quality. The object is perceived merely, and is avoided or sought as prompted by the pain or pleasure. But there is no cognizance of the impelling feelings, and though the object its known by its pleasure-pain effects, it is not yet known *for* them. When we see a thing we always see in it, its pleasure-pain quality, or even if we do not know its particular form, we know it as having some feeling potency. This is an instinct in our knowing, and the outgrowth of an immense deal of experience. It is the sole value and function of knowledge that it should not simply apprehend the thing but its pleasure-pain-giving nature. But still the first moment in cognition must be termed bare apprehension of object, the feeling being behind the knowing and supporting it, and not a knowing of feeling quality, and a feeling about this. The first moment in all cognition is bare apprehension, which is soon followed by ascribing to the object its feeling significance.

How then from mere concomitancy of feeling and cognition of object is cognition of the feeling quality of the object achieved? How is object perceived as related to experience, as potent for pleasure or pain? How is the pain-giving object perceived as such?

It is commonly said that association and memory explain this. Upon seeing a hurtful thing we know it to be such from past experience when we cognize the same object while it was hurting, and so related pain to the object. But this sense of experience and relation is just what we have to explain; and we have also to show how the suggestive cognition is brought about, how we see the thing at all before it exercises any real feeling effect on us. There is no doubt that when any object often causes intense pain, cognition of this object is stimulated, and finally pre-perception occurs, but just how is quite obscure. A low organism will repeatedly be injured by some thing, as a sharp object, will plainly suffer pain and have some concomitant knowing to stimulate and direct will-effort, while yet no premonitory knowing or pre-perception is apparent. There seems to be no sense of the experience value of the object even while it is having pain and knowing object. It is pained but not frightened.

Cognition certainly occurs at instance of less and less pain, as also do volition reactions, but yet it must, whether as pre-perception or otherwise, have some feeling initiation, which must always be traced. Can we suppose now that a study of revivals will help us in this, or in determining how sense of experience-value originates? Repeated experiences with the sharp rock enable the organism at expense of less pain, but with clearer and fuller cognition, to escape severe injury. As a fixed sequence of experiences tend to recur together there will follow upon the cognition revival waves of pain before any actual increase of pain is really inflicted in the given case. These waves stand for, and are the echoes of, the former real pain sequences of cognition. Thus the perception of a great mass of ice will often cause a shivery feeling, a painful sensation, which was correlated with former cognition experiences. Even the image or representation, the purely and consciously ideal cognition, may bring in painful feeling, as when I say, 'It makes me shiver to think of it.' Here the sensation-bringing idea is cognized as such, but the representation here is the occasion of a direct painful sensation, and evidently does not imply fear or other emotion.

While not arising from actual injuries. revivals strengthen both cognition and volition. They have recurred before further hurtful experiences with the rock which originally incited them. These revival pains of previous sequences to the cognition, and which are carried along with the present cognition, are real enough in themselves, yet they are objectively anticipatory of actual injury. The whole order of previous experience is by the nature of mind and nervous system re-enacted before the actual injuries are inflicted. It is always a race between mind and nature, but it is a prime function of mind to anticipate practically the movements of nature. Mind by its revival forms accomplishes this, but if it lags in its work the real injuries are mercilessly inflicted by slow but sure nature. When the sequence of revival is quicker than the objective sequence, the reactions anticipate objective order, and thus is achieved a manifest economy. But pain revivals of this kind are not fear, nor is there a real pre-perception. Since the revival forms are, to the observer's point of view, incentive to anticipatory reaction, psychologists must often, especially with low organisms, mistake them for fear; the animal is often, doubtless, merely suffering revival pains when it appears to be fearing pain. Thus we may suspect that organisms which seem to fear shadows or real objects are often merely suffering revival pains brought up in conjunction with the cognition, and not really fearing as result of perceiving feeling quality inherent in the object. Manifestation of pain must often be mistaken for manifestations of emotion, and there is as yet no accurate objective determination for fear or other emotions. That we always see in things the possibility of feeling and sensation, and this inevitably and naturally, blinds us both to the problem and to its difficulty.

How we perceive a thing before it is in the least experienced in pleasure-pain terms is then wholly unexplained by revivals. Revival pains are not representations of pains as in some way coming from object. Emotion requires representation, and cannot occur in any presentation or re-presentation chain. True pre-perception is not merely perceiving the thing before its effects in feeling are experienced, but it is a *representing* the feeling quality of the object before, in any given case, this quality is directly experienced. This obviously rests on past experience, but the connecting of object with pleasure-pain experience is at all times, as before intimated, equally a problem. Emotion and representation are built not of revivals but upon them perceived as such. At some critical moment in some rather early period in mental development a consciousness which was pain *plus* sense of object, realized, under the pressure of struggle for existence the feeling quality of the object, and there arose painful emotion with the knowledge of object as pain-giver. And as soon as object is not merely cognized, but cognized as pain-giver, it may be feared. The moment that object was known as a pain agent then fear of the object came, and thus true anticipatory action arose. We are said, indeed, to fear objects, to fear men, animals, etc., but, in truth, the fear is never of the object as such, but only in view of its pain agency. The cognizing the experienced and experienceable as such seems then a peculiar and distinct process in fear and in all emotion, a *genus* apart which cannot be constituted by interaction of simple elements. The growth of mind is largely in multiplying and enlarging the signs of experience.

Fear as involving pre-perception of the painful nature of object is obviously based on some direct experience where the pain was immediately perceived as related to object. The genesis of fear is really in this immediate form; the moment that pain experience is connected with object, the moment the child playing with fire perceives that the object, fire, hurts, then fear is born. Fear originates as soon as pain agency of objects is realized in any direct experience. The germ of fear is reached whenever the knowledge expressed by words, 'it hurts,' is attained. The first stage of fear rests upon the pain relation which is being experienced from object, not a pain having been experienced. The present hurtful objects perceived as such incite to fear.

The connecting of object with pain once achieved, it becomes increasingly easy to cognize the feeling value of objects, and before full and extreme pain experience therefrom to pre-react through emotion. Thus emotion saves both direct pain and injury. As it becomes a permanent tendency and an impulse of consciousness to proceed from all pure feelings to cognition of object, so also to cognition of object in its feeling quality, and thus by inherent tendency it ultimately comes about that there is attaching of pain to various objects cognized, even when there is no immediate experience of pain to be connected therewith. Finally the precedent inciting pains to cognition become such minor factors, and knowledge arises with such apparent spontaneity, that emotion as involving pain significance becomes dominant rather than the immediate pain. An order of consciousness becomes established in which the notable event is cognition of experience values as bringing in emotion rather than an order of pleasure-pain inciting cognition. But at the first it is evident that fear was but a slight event in a consciousness which was mainly absorbed in immediate pain experience and some sense of object. It is so habitual and instinctive for us to perceive all things as having feeling value that it is most difficult to appreciate the standpoint of a consciousness which is just attaining emotion life.

The preliminary elements to simple primitive fear as expressed by any such phrase as, 'it hurts,' are at least four; pain, cognition of object, cognition of the pain, cognition of the pain agency of object. These operations as being at first successive do not necessarily imply, however, sense of time. The consciousness of a pain is certainly, at first, consciousness of pain really past, yet not consciousness of it as past. The pain stands as immediately antecedent act to the consciousness which is cognition of it, but sense of experience is not thereby sense of experience in time. The sense of time-relations of experiences is wholly subsequent to the simple sense of experience. All experience is, of course, in time, but far from being of time.

The earliest fear is not then in view of the experienceable, in view of experience as future fact; fear, at first, is manifested only when and so long as pain is being cognized as from object. As soon as this sense of immediate relation ceases, fear ceases, for its foundation is gone. So long as hurt is felt from object, object is feared. The higher and the ordinary form of fear in human psychic life is based on this lower fear. An organism which has repeatedly suffered knowingly from an object and so feared, attains at length the power of fearing antecedent to any real injury. This seems to be brought about somewhat in the following manner. If I in any way, as by a pin pricking, rouse a sleeping animal to a cognition of an object which has often injured it, and which it has often feared, immediately there would re-occur the original concomitants of the cognition in the previous cases, there would be pain, cognition of pain, ascription to object, and fear, all merely revivals, and happening most probably before any actual injury, etc., received in the present case. Now these revivals, as before insisted, do not and cannot in themselves alone form a new fear. This is only constituted when the revival pains are known as such, when they are not merely presented in consciousness, but represented as belonging to past experience of thing, and so to be experienced. The thing is thereby truly *interpreted* for its feeling value. Not merely pain as being experienced is connected with thing, but as having been experienced, and to be experienced. Thus only arises that sense of the experienceable, that real *apprehension* for the future, which is so valuable an acquisition in the struggle for existence. Feeling quality comes thus to be assigned as real and permanent property of things, and every cognition comes to imply representation of feeling value, and so to be a basis for emotion. But all sense of experienceability is founded on sense of experience; the sense of things as possibilities of sensation and feeling is based on actual relatings of feelings to objects in simple direct experiences.

Fear is in itself pre-eminently a painful state, and we have to inquire as to the origin and nature of this pain. The statement of the problem in general form is, how does that which does not yet please or pain, but is only cognized as about to do so, give immediate pleasure or pain?

We have already expressed the opinion that fear is based on more than mere pain revivals; there must be true representation, the revival must be appreciated as representation of past experience and indicative of future. The painful agitation consequent on prospect of pain seems, indeed, to include as pain element more than revival pain, but it is only seeming. Where does the pain come from which a person feels at the mere prospect of pain unless from the past? The pain is, of course, not the identical pain feared. Again, one cannot see how a cognition in itself entirely empty of feeling can cause a pain except as acting as a link in a chain of association whereby conjoined past pains are revived. So far as fear is pain, it is revival, for representation of pain is not pain and cannot cause pain. The pain which arises from cognition of pain to be experienced appears in a strict analysis to be wholly re-occurrence stimulated thereby, and not any new and peculiar mode of pain at pain. That this is the case is apparent from the fact that we can only have the pain of fear so far as we have experienced pain. Poignant pains experienced are the basis of poignant pain in fear. The knowledge that you are soon to re-experience an intense pain leads to an intense dread in which the intense pain is revived from former experience. There are, to be sure, in the phenomena of fear in highly developed consciousness complex pains which cannot be ascribed to revivals, reflexes upon consciousness of the great tension and agitation thereof, pain of loss of self-possession and self-power, and other modes which proceed from consciousness of consciousness, but this does not bear upon the question how mere cognition of pain as to be experienced can in itself give pain, how there arises from mere apprehension, a pain which is more than and distinct from the revival pains.

But, however we may be puzzled to see how mere cognition of experienceable pain develops a peculiar pain which is the essence of fear, yet we must acknowledge its production to be a fact. We may say, indeed, that the bare thought of pain even when conveyed by the printed word—the abstract sign of an arbitrary vocal name—is not without a tinge of a peculiar fear-pain which does not wholly consist of revivals. When preparing to go out into the storm on a very cold day I have pain in anticipation of the pain I am to receive from the bitterly cold wind. Now I may have preliminary shiverings, and there may be recurrent painful sensations as I look intently at the raging elements, pains which return from actual experiences which I have before undergone and at the time knowingly connected with wind and snow. But all these revivals, while the basis of my fear, do not give the distinct pain quality of the fear. The pain which I do experience when I actually step into the biting blast I know at once to be entirely distinct in quality from that which I before felt at the anticipation, the real pain, of fear. Again, when I say, 'I was deeply pained to hear of it,' and when I say, 'The noise pained me greatly,' I indicate that difference between purely mental distress and sensuous pain, between pain at representation and pain referred to presentation, which is to be emphasized in all our study of emotion. The tortures of fear with a man in the hands of hostile Indians are quite distinct in quality from the tortures actually endured. The agony of fear is a *genus* apart from the agony of physical pain.

Again, if the pain in fear were derived from revivals, then the nature of the pain in different states of fear would be as different as the sensations feared. But as a matter of fact the pain in fear of cold, fear of heat, of famine, of punishment, etc., is substantially of the same quality. I may fear one more than another, but the real mental agitation and pain which constitute the fear are in all cases essentially the same. If the pain in fear were sensation revivals, then fear of cold and fear of heat would be quite diverse and contrary in quality of pain value, but we all know that the dread of a cold day and of a hot day are in themselves essentially the same in nature. As far as the states are pure fear and have a pain quality, the conscious activity in both is entirely similar.

Further, if the pain in fear were wholly of revival nature, not only should we expect fear of different sensations to be correspondingly distinct, but we should also expect the pain in fear to never exceed in amount and intensity the pain feared as indicated by measure of past experience. But we know that our fears are often much more painful than pain feared and than our experience of past pain. The pang of fear, of sudden fright, is often more acute and intense than any direct pain we have ever experienced. The terrible convulsions of fear which we see in the insane give evidence of pain which could not have been reflection from direct experience. That excessive and sudden fear which turns men's hair gray in a few hours and transforms their whole physical system is plainly not any revival from the individual's past experience. As revealed by its effects it is often, perhaps, greater than the whole amount of pain they have ever suffered. Where, in the direct-experience form, pain is greater in the fear than the real pain suffered, we express the fact by the common phrase, 'more scared than hurt.' In all such cases the pain in fear is not the revival of past experiences of the object feared.

Fear is, in the main, the peculiar pain coming from consciousness of experienceable pain, but in general in all complex consciousness it is marked by dissolution and weakening of mental force. There is a shrinking of will, and a clouding of cognition, a general unsettling of all mental elements, a commotion or agitation which destroys the organic *consensus* of consciousness. But any excessive functioning of some element in consciousness, of emotion life, as fear, or of any other form, is unbalancing and detracts from normal activity of the whole. Fear, however, in its normal measure and form arose and was developed as a desirable stimulant; where it becomes paralyzing in its force, it is pathological in quality. Also where fear is pathologically intense it tends to disappear in sensation feared. Cognition becomes so weakened that sense of representativeness is lost, the thing feared is no longer brought before the mind in its potential quality, but is immediately apprehended as present in its influence—though really objectively absent—hallucination is produced, and fear naturally reverts to its earliest and direct form in immediate experience. As cognition is still further weakened the sense of object as giving pain is lost, and fear in any form entirely disappears. The pain is not felt which before was feared to be felt. Fear thus in the general order of its disappearance repeats the order of its appearance and growth.

Fear always includes some sense of object. The apprehension of something evil to happen is the basis of all fear, but the thing, or, subjectively speaking, the objectifying, may be extremely vague. We may fear that some harm is to befall us, but what and how, we know not. We must suppose that in early stages this bare objectifying of approaching pain was a regular incipient form, that an indefinite fear preceded every case of defined fear. We, as a rule, attain a full objectifying with such ease and rapidity that this form does not often appear.

A complete fear movement, then, with reference to cognition includes four stages: first, a very general sense of object as about to give pain; second, an increasing definition of object up to the maximum of clearness, thus marking the highest efficiency of the fear function; third, a decreasing definition of object till, fourth, a purely indefinite objectifying is again reached. Every fear, if it attains a normal life, will rise, culminate, and decline in this way. Even in man, where the full development of single simple psychoses rarely proceed undisturbed, there is yet observed a general tendency toward these stages. I awaken in the night at a sudden noise with slight and vague fear; suspicious sounds increase my fear and I listen and look more intently till I see clearly and quite fully crouching near the bed a dark body which I make out to be an armed burglar; as he approaches with his pointed weapon fear will most likely become so intense that I see less and less clearly, and a shot might terrify me into vague but very intense fear. If the object is discerned to be not a burglar but a chair, the fear quickly lapses. At a certain point of maximum clearness either a weakening or an intensifying of fear weakens cognition. Too much or too little pain is equally injurious to the knowing activity. Low psychisms examine and clearly define only that from which they have something to fear or hope.

The qualitative relation of the pain of fear to the pain feared varies greatly with the evolution of mind. Fear-pain could not have originated as a substitutionary function for the real pain except by being at the first somewhat less in quality than the pain to be endured, otherwise there would be no economy in the function. The progress of this function is to secure at less and less expense of fear-pain the suitable reaction. The function of fear being to escape a greater direct pain by a less indirect one, the progress of the function is in diminishing the amount of fear-pain for required effectiveness. The small original gain in the ratio is increased by small increments till in the highest minds proportion of fear-pain to pain feared might be represented by $\frac{1}{\infty}$. The pain in the usual fear which commonly induces me to step from the track before an approaching train, or which enables me after reading some advice on the subject to take precautions against the cholera, is evidently in infinitesimal relation to the pain feared. When fear is unsuccessful, as in anticipating a visit to the dentist, we, of course, suffer a double pain, both the fear-pain and the pain feared.

Often we must observe that the pain of fear is equal to or greater than the experience feared, and we have to ask how this disadvantageous excess could have been evolved. Often the pain of anticipation turns out to be far greater than the pain anticipated. However, a little reflection assures us that the excess of fear in many cases is only in appearance. We do not fear too much upon the judgment we have formed as to the coming pain, but we have by error of judgment assigned too much value to the pain. When a person being initiated into a secret society trembles with fear at being told to jump from a precipice, when he really is to jump but a few feet downward, his fear was perfectly just according to his judgment. If his belief is perfectly assured, the mortal fear will make him offer the most strenuous resistance and most likely secure his release from the ordeal. In all such cases the feeling is right enough, but the estimate of future experience is inaccurate. When an animal is terrified at its own shadow the fear is justly proportioned to the estimate of danger, which, however, happens to be erroneous. In the evolution of mind in the struggle for existence, more and more accurate calculations of possible injury are attained, and fear becomes more and more rational. Educated men fear only what is worthy of fear; they fear many things that lower minds do not, and do not fear many things they do. The true excess of fear is where we fear against judgment, as when, knowing the safety of travel by rail, I am yet constantly in fear while aboard a railway train. When I still continue to fear though I know the fear to be groundless, this is a true hypertrophy of fear. We constantly observe those who are fearful and timid against their own reason. When dangers known are compared with dangers obscure or unknown—and perceived to be unknowable,—the fear of the unknown often prevails against the fear of the known, and we prefer with Hamlet to fear the ills we have than fly to others we know not of.

I must in conclusion express my conviction that while the physiological and objective study of fear and other emotions is of very considerable value, yet it is only introspective analysis which can reveal the true nature and genesis of fear and all emotion. What fear is and what is the process of its development can only be determined by the direct study of consciousness as a life factor in the struggle for existence. This I attempt in the present paper with the main result that fear, as indeed every emotion, does not consist of pain or cognition-revivals in any form, but is a feeling reaction from the representation of the feeling potency of the object.

EXPERIMENTS IN SPACE PERCEPTION. (I.)

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In undertaking to describe some experiments in the perception of magnitude and distance, I ought first to mention certain peculiarities of my own eyes that are quite different from the majority of cases, and that exempt my results from the difficulties which most observers have to encounter and which often render their judgments doubtful or illusory. I do not refer to any structural peculiarities, nor to defects of any kind: for, as far as I have been able to ascertain, my eyes are quite symmetrically constructed and are free from all usual defects whatsoever. There are no traces of myopia, astigmatism, or similar obstructions to perfect vision, and hence for all distances my sight is remarkably good—a fact, however, which has no interest except to remove certain suspicions which *might* be entertained regarding the experiments I expect to describe below. But the peculiarities to be mentioned concern the dissociation of certain functions which are very closely associated in general experience. I refer first to the various degrees of convergence which I can practise without any interference with the normal functions of vision. For instance, I can cross the eyes and walk about the streets, the fields, or the woods with as great ease and freedom from error as in their natural position. No pain or discomfort accompanies the process, but only a feeling of effort and fatigue after some persistence in it. Still more noticeable is the fact that where most persons suffer from greatly blurred images, I am comparatively free from this disturbance. This is true, however, with qualifications which must be noticed. Thus the blurring is only slight until the fusion of similar images, when the vision becomes normally clear. Thus I may compare with mine the experience of others whom I have often besought to verify my results.

If two circles are drawn a short distance apart and the eyes crossed so as to produce fusion, I generally find that the images are so blurred by the failure to accommodate the eyes suitably, when others perform the experiment, that no reliance whatever can be placed upon their judgment of the results, no matter what they claim to see, while in my own case there is no blurring whatsoever. The images are as clear and distinct as if I had not converged my eyes. I can freely move my eyes from one to the other of the three circles in the field of vision and back, and up or down, without in the least disturbing the fusion, the clearness of the perception, or the apparent localization and perspective of the figures. I can carry on observations as well under these conditions as when the focalization is natural. Indeed so easy and free from discomfort is the process that, if the field of vision is constituted by some large surface covered with uniform figures, in a few minutes the new position becomes so natural that it is with difficulty disturbed and the normal condition restored. I can carry on the parallel movements of the eyes under these circumstances without any tendency to disturb the convergence, or the localization, which in these cases corresponds to the degree of convergence, or approximately so. I should not even know that my eyes were crossed but for the slight influence undoubtedly exerted by a surface which is not exactly uniform, or whose figures do not produce absolutely identical images.

So much for the effects of convergence within the plane of the paper upon which the circles are drawn. It is similar when I focus the eyes beyond the paper, so as to produce fusion, for all distances greater than about eighteen inches from the eyes. With others I find blurring exceedingly great under these circumstances. They suffer also from the still greater difficulty of keeping the fixation upon one point and the attention upon another either horizontally in the indirect field or in different points on the median line. But neither of these conditions presents any difficulty to my eyes, with the exception just indicated. If I hold the paper more than eighteen inches from the eyes, or place it at any distance within the limits of fusion by focussing beyond it, and these may be as much as fifteen feet from me, there is no blurring whatever, and I am free, not only to move my eyes over the field with ease and without disturbing the fixation, but also to carry on observations as distinct and clear as normally. Within the eighteen-inch limit the result varies. At first the lines of the circles are somewhat blurred, though the circles are quite distinct and observations comparatively undisturbed. But by a little practice or after a few minutes the blurring diminishes and often ceases altogether until the paper comes within twelve inches of the eyes, when it is noticeable again. But even here the effect produces very little influence upon perception and its accuracy, as the blurring is too slight to affect it. But with these qualifications my vision is as clear and as easy under the circumstances described as under normal conditions. The fact will attach some importance to the following experiments and observations.

I wish to put to the test certain theories of space perception and to ascertain how they conform to the facts of experiment. Ever since Berkeley, much has been made of the muscular and motor influences affecting the problem. But these have been almost wholly confined to the perception of distance or solidity, all parties being influenced by the fact that this quality could not be represented in the retinal impression. The problem has been, therefore, to devise either a sensorial or a motor explanation of this additional datum. The phenomena of convergence, binocular fusion, and translocation of images have been the starting-points of discussion and theory, and hence, finding certain coincidences between them and the localization of objects, the natural supposition was to connect the consciousness of distance with the motor functions involved. The whole question of magnitude, however, in connection with the same functions seems to have been wholly neglected, though its phenomena might be used by both parties to the controversy. On the one hand they seem to confirm the motor-sensation theory, and on the other to suggest such an anomalous relation to sensorial functions, and such a modified conception of the whole process of space perception, that they throw considerable doubt upon the very theory which they seem to confirm. I shall, therefore, take up first the perception of magnitude, which I find in all my experiments to be variable with the degree of convergence necessary to produce combination of images. Its peculiarities must be very carefully noted.

I shall first describe some very simple experiments with natural objects, which it is possible others can confirm without difficulty, though I have never seen any allusion to them in the literature of space perception. Thus if I close one eye quickly while looking at an object, say a lamp-shade, a plate, a sheet of paper, a window, a house, or anything whatever, there is a most decided impression of diminished magnitude. I cannot say how great the decrease of magnitude seems, but it is distinct and uniform enough to dispute the suspicion of ordinary illusion. Ever since I detected the fact the phenomenon has been absolutely without exception, even when I tried to overcome it by thinking the judgment illusory. The moment that the closed eye is opened the magnitude of the object is apparently enlarged. Nor is the effect due to relaxed binocular adjustment and disparity of images caused by closing and opening of the eyes, for the same effect is noticeable by shoving a piece of card-board between one eye and the object and removing it at once. Besides, the effect does not resemble that which is due to disparity of images. Of course this phenomenon does not illustrate the effect of convergence upon magnitude, as mentioned above. On the contrary, it rather illustrates sensorial influences and probably confirms Prof. James's doctrine of voluminousness in sensations. But it also distinctly suggests other than motor influences affecting spatial properties in our perceptions.

The next class of common experiments illustrates the connection between magnitude and the degree of adjustment. Thus if I cross the eyes and combine the images of two similar windows, say on the opposite side of the street, the diminution in the apparent magnitude of the windows is very distinct, a diminution that continues as long as convergence increases, until the window seems a mere miniature of what it is normally. The degree of convergence will, of course, vary either with the distance between the windows, or with the distance of the eyes from the windows. This fact requires no comment, but it indicates the conditions under which the observations can be most favorably made, because my vision is more clear and distinct during fusion than before or after it; that is, than when the images are not on corresponding points. But I must remark that the general effect is the same whether the images are fused or not. The diminution of magnitude begins with the change of convergence and continues with it without regard to fusion. Besides, the location of the window seems to be nearer than in the normal position, and this locus is more distinct and definite during fusion than at any other time. Neither one of these results can be confirmed by the ordinary student, and this is not to be wondered at when we consider his lack of experience in such matters. But even trained experimenters may fail to be assured of the effect, because the whole field of vision is equally affected by the process, and because of the cohesion between adjustment and accommodation which makes all images indistinct that are not normal. The phenomenon of localization in this case, however, would probably not be regarded as anomalous: for it is in reality the same effect as translocation in the case of binocular parallax in the combination of geometrical figures drawn for stereoscopic purposes, except that this parallax is not present in the instance described. Hence I need not dwell upon this part of the experiment. But the modification of magnitude is not so easily explained. Nor has any one alluded to it within my knowledge. We might expect accommodation to have a fixed relation to convergence and thus to diminish or enlarge the image according to the usual law by making it indistinct. But such is not the case, and the diminution may be due to the alteration of the pupil and not the lens, so that the image is actually made smaller by virtue of the decreased aperture for the light. This might take place from the habitual connection between accommodation and convergence on the one hand, and between accommodation and the modification of the pupil on the other. But I have no means of proving such a supposition, as I cannot observe my own eyes in the operation. I can only conjecture that habit and association may establish a more or less fixed relation between functions which are in their nature distinct, and whose effects become particularly clear when blurring does not occur from an altered accommodation. Accommodation in these cases is perfect, for there is no obscurity from unfocussed rays of light. Only the images appear smaller, and hence if the connection between the area of the pupillary aperture and the degree of convergence remains more fixed than that of accommodation and convergence, so as to decrease with the increase of convergence, we might suspect what the cause is. But as I do not find any alteration of the magnitude of images from the artificial dilation of the pupils, I may well doubt the influence of their modification, while also raising the question whether they necessarily undergo any change from the alteration of convergence and without any change in the brilliancy of the light from the objects. This suspicion is more than confirmed by the fact that in no instance, where I have changed the illumination upon an object, have I noticed any modification of magnitude follow the contraction or the expansion of the pupil, as the case may be. It is only when convergence takes place that this effect is noticeable and without any alteration of the light from the object. Further proof of this will be given again after further illustrations of the effect.

A very beautiful effect can be produced in the following manner. If I look at a large surface, say the side of a building having some regular and uniform decoration, and cross the eyes so as to combine certain similar portions of it, the windows for instance, the fusion takes place over the whole area, representing distinctly every point or mark in the field of vision, except on the margin of the indirect portion. But the figures are much diminished in magnitude, and this diminution, as before, increases with the degree of convergence, and becomes a comparative miniature of the real size. If the point of fixation is as near as two feet, the whole surface seems distinctly within reach though the wall be fifty or one hundred feet away, and I can put out my hand and touch it or write upon it as it were, the illusion being perfect except for the absence of tactual sensation. I can move my eyes about over the whole field and by retaining the fixation for two or three minutes can eliminate all feelings and associations of the real distance until I seem to be before one of the delicate models we so often see of various forms of architecture. The moment that I break the convergence the sudden change to the normal distance and size of the wall with its figures creates a kind of shock or surprise, so distinct is the contrast and sense of illusion. The same phenomenon shows itself when I cross the eyes to combine the figures of wall-paper, the size and titles of two similar books, the letters of two similar pages of reading, the squares of a screen, or any similar figures whatever. The magnitude is diminished in proportion to the convergence. Indeed resemblance in the form of the figures is not an essential condition of the effect, only it prevents rivalry and aids in an easy and agreeable retention of a given degree of adjust ment.

Now the next question is whether I can produce an apparent enlargement of size by focussing the eyes beyond the plane in which the figures to be combined may lie. Unfortunately there are decided limitations to the performance of this experiment. It is impossible to try it with objects at a considerable distance or more than a certain space apart from each other. Nevertheless I have been able to do so under favorable circumstances and within the limits referred to. Thus I have taken two books of the same kind and titles, and by focussing the eyes beyond them combined the letters and effected a very considerable enlargement of them. I get the same result from a similar combination of any figures capable of it. But both the diminution and enlargement of magnitude can be illustrated, as I have described them, by the following experiments, and perhaps some clue to an explanation obtained at the same time.

I take two circles as represented in Fig. 1, drawing them

FIG. 1.

only a short distance apart from each other. If I cross the eyes until the circles fuse there will be three circles in the field of vision, all of them perfectly clear and distinct. No suppression of the external circles takes place unless I concentrate attention very strongly upon the central circle, and then they may wholly disappear, to return again when attention has been relaxed. But the central of the three circles, which is the combination of the images cast upon the external halves of the retinas, seems distinctly smaller, and very generally somewhat nearer than the other two, but all of them smaller than the original circles. The following figure (Fig. 2) represents the appearance of the three circles after fusion, and, as nearly as I can make them, in the proportion between their several magnitudes, and also in the proportion between the magnitudes of the apparent and the magnitudes of the two original circles in Fig. 1. It must be remarked, however, that the magnitudes of the three circles vary with the degree of convergence required to combine the two, and hence is proportioned to the distance between the original figures, and it is most remark-

FIG. 2.

able that the distance of the eyes from the paper does not affect the apparent magnitude of the circles any more than it does in normal vision. This accords with the law of convergence, which increases and decreases, *ceteris paribus*, with the distance from the eyes of the objects combined. This incident will be the subject of comment later in the discussion. But some conception of the effect will be found by comparing Fig. 2 with Fig. 1, since the three circles represent, as nearly as I can determine it without mechanical measurement, which is impossible in the case, the modifications of magnitude accompanying combinations of the two circles in Fig. 1. So far from there being any illusion about it varying with the judgment of the facts, in the many thousands of experiments I have tried there has not been *one* variation from this result, the only difference being in the size of the original circles. The diminution is then proportional.

Now in regard to the explanation of the fact, it is clear that it cannot be attributed to a contraction of the pupil. This supposition might account for the decrease of magnitude compared with the circles in Fig. 1, but it will not account for the smaller diameter of the central circle in Fig. 2 compared with the external circles, because the pupillary contraction is the same for all of them. There is, of course, the conclusive fact already alluded to, that a modification of the pupil in normal vision is not followed by a corresponding alteration of the apparent size of the object. But in spite of this we might be tempted to consider such an influence as possible in artificial convergence and yet the anomaly in Fig. 2 effectually excludes this hypothesis. Nor does it fare any better with the supposition that the modification of the lens produces the effect. This influence might again account for the diminution in comparison with Fig. 1, but the same difficulty as before occurs when the difference between the central and the external circles is considered. The force of this supposition in the first instance, that is, in the comparison between the original circles and those after combination, cannot be lightly ignored. For, other things remaining the same, an alteration in the convexity of the lens will affect the magnitude of the image—a well-known optical law. Now if the mere habit of altering the lens to suit the direction of the rays of light and in perfect consonance with the degree of convergence is likely to affect its convexity—that is to say, if the association of a given degree of convexity with a given degree of convergence is likely to be strong, then the mere convergence of the eyes to produce fusion artificially may be accompanied with the corresponding degree of accommodation, and this increase of convexity in the lens would produce a diminution of the image and a corresponding decrease in the apparent magnitude of the circle. Whether such an associative connection between accommodation and convergence exists or not, I am not able to say. But the supposition, though probably true for most persons, as indicated by the indistinctness of the images, has its difficulties in my case. For the circles are as clear and distinct after convergence as before it. Now as we suppose accommodation to be instigated solely by the direction of the rays of light, and as these remain exactly the same under both conditions, the focus for the retinal image must be the same, and as the image is clear there is no optical reason for a modification of the lens. It would seem, therefore, that there must either be an obscurity or a blurring of the image, or the absence of all modification of the lens. Hence while associative influences might account for a real diminution of the retinal image, they conflict with retinal distinctness and the optical reflex which is the primary agency in accommodation. But whatever strength the supposition might have in comparing the decrease of magnitude with the magnitude of the original circles, as already remarked, it is fatally contradicted by the difference in magnitude between the central and external circles of Fig. 2, or the difference which I have described is an illusion. It is too distinct, however, for me to suppose the latter alternative, and I must adhere to the former conditions whether they offer any difficulties or not. Nor will the functions constituting convergence help us to explain the whole phenomenon, for we encounter the same difference between the central and the external circles while the adjustment remains the same for both. If, however, we can suppose that the apparently nearer localization of the central circle represents a correspondingly greater degree of muscular tension, or sensory effort at fusion, if sensory it be, than for the localization of the external circles, which is purely monocular, we might readily admit the influence of convergence in the case. For we might refer the diminished magnitude of the external circles compared with the originals in Fig. 1 to the contraction of the lens with the consequent diminution of the retinal image, and the magnitude of the central circle in Fig. 2 to this contraction of the lens *plus* the binocular tension involved in convergence and fusion.

A confirmation of all this is found in the reversed effects of combining the circles by focussing the eyes beyond the plane in which the figures lie. If I do this there are three circles in the field as before. But the central figure, which is the combination of those on the *internal* halves of the retina, appears larger than the other two, and all of them larger than the originals, while the central circle appears very distinctly both farther from the plane of the two on the paper, and farther off than the two external circles, ostensibly at the point of fixation. This whole result is illustrated in Fig. 3, which has been drawn under the same conditions and with the same proportions, if possible, as in Fig. 2. Here we have an effect which is just the opposite of convergence within the plane, and it illustrates what would take place could we apply the process to objects and surfaces in general as we can the convergence just mentioned. For the sake of brevity and clearness I shall speak of convergence within the plane on which the figures lie as *positive*, and beyond it as *negative*. Now if positive convergence shows a tendency to diminish magnitude both in the case of geometrical figures drawn on a plane and in that of real objects, figures, etc., on walls and large surfaces, we should expect an enlargement of this magnitude in the case of negative convergence, and so we find it, though there are limitations to the application of the process generally, which are determined by the limited extent to which negative convergence is possible. But there is every reason from what occurs in Fig. 3 and from the case of the book titles, already described, to believe that the effect of enlargement under negative convergence would

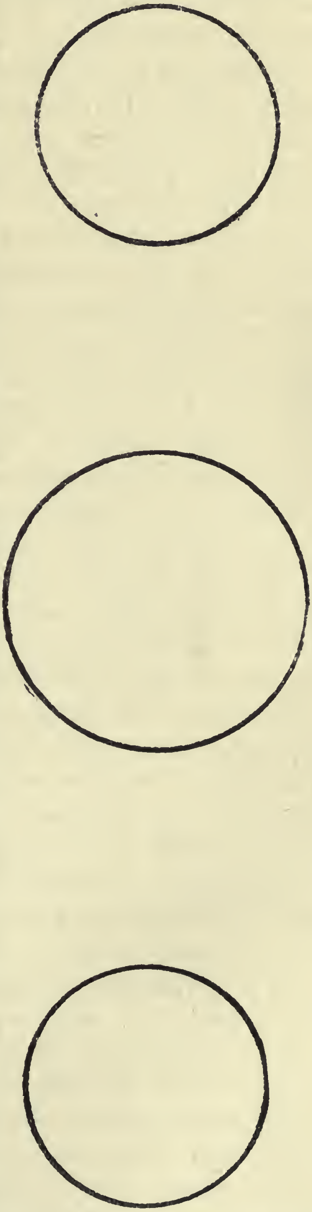

FIG. 3.

be as universal as diminution is under positive convergence, if only these limitations did not interfere. However we have here the whole principle illustrated, and that is a tendency to enlargement of magnitude with negative convergence.

Now as to explanation, it is not necessary to go through the whole length of criticising the supposition of pupillary expansion, which may be considered as rejected once for all. Nor need we emphasize the difficulties attaching to the differences of magnitude between the three circles: for we have only to suppose a combination of binocular adjustment and the expansions of the lens to account for this difference, and this hypothesis has fewer difficulties to contend with than the similar one under positive convergence, because within the distance of eighteen inches from the eyes the images are blurred, a fact that accords exactly with the possibility of a fixed, natural or associative, connection between accommodation and convergence. Beyond the eighteen inches the focus may be so nearly the same for all distances, and the modification of the lens so complex and peculiar, as not to affect the distinctness of images, so that no unquestionable fact would thus stand in the way of the supposed possibility mentioned. Confirming this, in a measure at least, is the fact that within certain narrow limits I can voluntarily contract the lens of the eyes without modifying or changing the degree of convergence, but beyond these limits the effort invariably results in convergent movements. The relation between accommodation and convergence is, therefore, not an absolutely fixed one, so that there may be variations often and large enough to account for the discrepancies supposably due to the ordinary laws of optics. If so, all the phenomena are closely connected with the processes, sensory or motor, of combination and adjustment.

A very interesting negative confirmation of this comes from the observations of some of my students who are fortunate enough to get any results at all. Usually I find that they cannot see the two external circles, but occasionally a student is found who can see them. But quite invariably they can distinguish no difference of magnitude between them and the central circle, nor any difference in the localization of the central circle in the third dimension. Besides, they find all the circles so blurred that a comparison would be somewhat difficult. I hope still to discover some who are free from this difficulty. But as long as this obstacle remains and they cannot detect a difference of apparent tri-dimensional distance between the central and the external circles, there should be no surprise at the failure to perceive a difference of magnitude. For, as indicated, this might depend upon a difference between binocular tension in convergence and monocular tension in the contraction of the lens, and we have no *a priori* reason to suppose that this either must or may take place. But, not to say anything of the influence of association and the development of the phenomenon by practice, the connection between accommodation and convergence may be so fixed in most cases, which the fact of blurred images in the cases at hand favors, that a difference of tension between the two functions might not arise. It is plausible, therefore, to regard the failure as a negative confirmation of the hypothesis advanced.

Nevertheless, plausible as such an hypothesis might be, it is not easy to sustain. In the first place there are two problems here; the general modification of magnitude, and the difference of magnitude between the three circles. If the general diminution is caused by convergence, all three circles should be of the same size and located in the same plane. This fact would also be true if the effect were produced only by accommodation, either the adjustment or the acccommodation being the same for all the images. But to suppose a difference between the tension of binocular convergence and monocular accommodation is only to suppose that what accommodation does for the external circles it must do for the central instance, and that would be to locate them in the same plane. Moreover, how little accommodation has to do with the effect would naturally be inferred from the enormous disparity between the reduction of magnitude and the degree of modified accommodation supposable. Thus if I look at an extended surface, fifty or one hundred feet distant, decorated with windows, frescoes, and other similarly symmetrical figures, and combine identical forms, the diminution of magnitude, if the convergence is considerable, is so great that we can hardly conceive that it is explained by supposing a corresponding decrease of the retinal image. Distance has more to do with this than accommodation. Windows that are twenty feet long and five or six feet wide and fifteen feet apart, at sixty feet distance will appear under fusion to be possibly not more than one fourth their real size, a mere miniature of the original. Such a reduction by an alteration of the lens, while the image remains quite clear, seems impossible. But a judgment of this kind is not proof and the matter will require much more careful analysis to settle it.

But I must first prove the fact and the amount of modified magnitudes. The layman's objection, and that of the scholar also who cannot perform the experiment, would be the query as to the evidence for what is here asserted. No direct comparison, other than one's own feelings, can be made between the size of natural objects or geometrical figures and that of the images seen under altered adjustment. The comparison has to be made through memory, and this is liable to illusions. But the first plain reply to an objection of this kind is the fact that when the objects or figures combined are a considerable distance apart, the modification by adjustment is so great that the charge of illusion would make memory unreliable for distinguishing the magnitudes of a silver dollar and a silver ten-cent piece seen separately at an interval of four or five seconds.

Fortunately I am not left wholly to subjective impressions and judgments for the evidence of modified magnitudes in the several illustrations chosen. I have been able to measure certain cases of them, and can state in inches or parts of inches the variation of size from that of the original objects or figures. This I effected in the case of the circles by passing a wire through the central circle of the three so as to form its diameter, one end terminating in the circumference, and then marking where the circumference intersected the wire at the opposite end of the diameter. This is done at the point of fixation where the central circle seems to be. This diameter can then be measured and compared with that of the original.

In Fig. 1 the circles are just one inch in diameter. Combining them by positive convergence at a distance of six inches from the eyes the central of the three circles has a diameter of $\frac{8}{16}$ or $\frac{1}{2}$ of an inch. At one foot's distance it is also $\frac{8}{16}$, and at two feet distance it is still the same, so that the distance from the eye does not affect the result. But if the two circles in Fig. 1 are placed farther apart—that is, farther from the median line, the decrease in magnitude in connection with convergence is much more marked. Thus when the circles are two inches apart, the diameter of the fused central circle, as represented in the manner of Fig. 2, is, as near as I can determine it, only $\frac{13}{32}$ of an inch, at the same distance from the eyes as before mentioned. This is only a little over one third of the diameter of the original circles. The difference or reduction is still greater when the distances between the circles is greater.

In order to ascertain whether the same law held true for negative convergence I drew the circles upon a plate of glass, making them $1\frac{1}{4}$ inches in diameter, as they were drawn with a fifty-cent silver piece, and to make the experiment practicable the circles were separated by only $\frac{5}{8}$ of an inch interval, so that the point of fixation would not be beyond the reach of my arm. Combining them by negative convergence, focussing beyond the glass, with the circles only six inches from the eyes, I first *guessed* the apparent diameter of the central circle and made it four inches, and found by measurement, as before, that it was exactly this. At nine inches distance the diameter was still the same four inches; at twelve inches the focus was beyond the reach of my arm, but by the aid of a second person the measurement was effected and the diameter was still four inches. This shows the same law as for positive convergence. In this case the magnitude of the external circles was not measured. But it was decidedly smaller than the central circle and apparently much nearer the eyes. But the magnitude of the external circles for positive convergence can be measured and compared with that of the central circle. This is effected by keeping the measuring wire in the same plane as the central circle, that is, in the horopter, and measuring off the apparent diameter of the external circle, which I can do by turning the eyes to the right or the left, as the case may be, without altering the convergence. In Fig. 1, I find in this way that the external circle measures $\frac{9}{16}$ of an inch in diameter. If the circles are larger the difference is more noticeable and more easily determined. There is great danger of error in the measurement of the external circles because the wire would necessarily show a greater diameter as we approach the plane of the paper. The same would be true of the central circle were it not for the facts that it is so distinctly located at the point of fixation, and that the wire appears double and beyond the circle if placed beyond this point. At that given point, of course, the wire would give only $\frac{9}{16}$ of an inch for the original circles on the paper, as actual measurement shows, so that the measurement of the external circles must be made in the same plane. Hence it appears that the diminution of the central circle compared with the external two is connected with fusion, while the diminution of the other two, less than the central circle, is connected with the general convergent condition.

This measurement of magnitudes, showing a difference between the central and external circles, and the remarkable ratio of decrease in size with the degree of convergence, taken along with the perfect clearness of the images, create considerable difficulty for the hypothesis that the effect is or may be due to associative contraction of the lens. For this contraction must be the same for both the binocular and the monocular circles, and hence ought to affect both alike. But it does not. In the second place we can hardly imagine that the contraction would be so great as to reduce the diameter of the retinal image by one half. And with circles farther apart this reduction may be to one third or even one fourth of the original. It does not seem that the contraction of the lens could be so great. Moreover, as more distinct proof of this, *the diminution of magnitude does not take place when one eye is covered or closed and attention fixed within or beyond the plane of the paper*, until a marked degree of convergent tension is reached, such as puts a strain on the eyes, except, of course, such diminution as I always remark on closing one eye, and which I have described above. *But blurring of the images in this case is always observed*, while within extreme limits no diminution of magnitude like that of binocular combination occurs. There is a slight illusion of it, as would be natural and to be expected from association. But the effect is so distinct from that of binocular fusion that the two cannot be compared, as any observer who can repeat the experiment will readily observe. The uniform indistinctness of the binocular images in this instance is the proof of one difference, and the marked difference between the monocular and the binocular effect is another fact of importance rather inconsistent with the supposition of associative ciliary influence and diminished retinal magnitudes.

(*To be concluded.*)

PERSONALITY-SUGGESTION.

BY PROFESSOR J. MARK BALDWIN,

Princeton University.

In a recent article in *Mind** entitled *Imitation*, I endeavored to show the order of rise of the child's various consciousnesses of himself, distinguishing in order three stages of development which I called 'projective,' 'subjective,' and 'ejective,' respectively. The first of these was grounded on so-called phenomena of 'personality-suggestion,' which I wish in this paper to submit to further analysis.

In the way of general definition, the following may be quoted from my earlier article (*loc. cit.* p. 40):

"One of the most remarkable tendencies of the very young child in its responses to its environment is its tendency to recognize differences of personality. It responds to what I have elsewhere called 'suggestions of personality.' (*Science*, N. Y., 1891, p. 113.) As early as the second month it distinguishes its mother's or nurse's touch in the dark. It learns characteristic methods of holding, taking up, patting, kissing, etc., and adapts itself by a marvellous accuracy of protestation or acquiescence to these personal variations. Its associations of personality come to be of such importance that for a long time its happiness or misery depends upon the presence of certain kinds of 'personality-suggestion.' It is quite a different thing from the child's behavior towards things which are not persons. Things get to be, with some few exceptions which are involved in the direct gratification of appetite, more and more unimportant: things get subordinated to regular treatment or reaction. But persons get constantly more important, as uncertain and dominating agencies of pleasure and pain. . . . A person stands for a group of experiences quite unstable in its prophetic as it is in

* New Series, III., Jan. 1894, pp. 26–55.

its historical meaning. This we may for brevity of expression, assuming it to be first in order of development, call the '*projective* stage' in the growth of personal consciousness."

The phenomenon of 'personality-suggestion' is so important in the growth of the child's consciousness of himself, of his belief in realities about him, and of his social life, that it should be closely scrutinized. This is the more important because such an analysis has never been made upon the basis of actual observation of children. The treatment which follows is based upon most detailed and watchful inspection of my two children H. and E., with especial reference to the development of the sense of their own relation to the persons who moved about them.

As outcome of this kind of observation, and with no intermixture of interpretation, which may be now left over, I find no less than four phases of experience involved in what afterwards becomes the so-called 'social sense.' I say 'afterwards becomes' because all of them belong in the 'projective' stage of the child's sense of self, i.e., they all go to furnish data which he afterwards appropriates to himself as 'subject.' These four phases are indescribably subtle and indescribably intermixed in the subjective *ensemble* of the growing child. So much so that I shall not attempt in all cases to cite actual situations to justify each point: rather the view I take rests upon innumerable situations, and their differences from one another. Just as one is utterly unable to give examples of his own phases of attitude expressive of the *nuances* of meaning which the actions of others bring out of him, so entirely a matter of insight and intuition must his sense be of what is in the child's mind in the various social situations which confront him from day to day. Nevertheless the drift of the infant's development is very clear to the sympathetic observer; and I think the instances which I cite will be sufficient to excite in all those familiar with little children a sense of the truth of the general portrayal.

1. The first thing in the environment of the infant which it notes—apart from the ordinary fixed and static stimulations, such as sounds, lights, etc.—are *movements*. The first attempts of the infant at anything like steady attention are directed to

moving things—a swaying curtain, a moving light, a stroking touch, etc. And further than this, the moving things soon become more than objects of curiosity; these things are just the things that affect him for pleasure or pain. It is movement that brings him his bottle, movement that regulates the stages of his bath, movement that dresses him comfortably, movement that sings to him and rocks him to sleep. In that complex of sensations, the nurse, the feature of moment to him, of immediate satisfaction or redemption from pain, is this —movements come to succor him. Change in his bodily feeling is the vital requirement of his life, for by it the rhythm of his vegetative existence is secured; and these changes are accompanied and secured always in the moving presence of the one he sees and feels about him. This, I take it, is the first and great association of the infant with other persons, the earliest reflection in his consciousness of the world of personalities about him. At this stage his 'personality-suggestion' is this *pain-movement-pleasure* psychosis: to this he reacts with a smile, and a crow, and a kick.*

Many facts tend to bear me out in this position. My child cried in the dark when I handled her, although I imitated the nurse's movements as closely as possible. She tolerated a strange presence as long as it remained quietly in its place: but let it move, and especially let it usurp any of the pieces of movement-business of the nurse or mother, and its protests were emphatic. The movements tended to bring the strange elements of a new face into the vital association, pain-movement-pleasure, and so to disturb its familiar course: this constituted it a strange 'personality.'

It is astonishing, also, what new accidental elements may become parts of this association. Part of a movement, a gesture, a peculiar habit of the nurse, may become sufficient to give assurance of the welcome presence and the pleasures which the presence brings. Two notes of my song in the night stood for my presence to H., and no song from any one else could replace it. A lighted match stopped the crying of

* Undoubtedly this association gets its value from the other similar one in which the movements are the infant's own. It is by movement that he gets rid of pain and secures pleasure.

E. for food,* although it was but a signal for a process of food-preparation lasting several minutes: and a simple light never stopped her crying under any other circumstances. So with this first start in the sense of personality we find also reasons for the differences of different personalities: but this constitutes the next phase.

2. It is evident that the sense of another's presence thus felt in the infant's consciousness rests, as all associations rest, upon *regularity* or repetition: his sense of expectancy is aroused whenever the chain of events is started. And this is embodied at this stage largely in two indications: the face and the voice.† But it is easy to see that this is a very meagre sense of personality: a moving machine which brought pain and alleviated suffering could serve as well. So the child begins to learn in addition the fact that persons are in a measure individual in their treatment of him, and hence that individuality has elements of uncertainty or *irregularity* about it. This growing sense is very clear to one who watches an infant in its second half-year. Sometimes its mother gives a biscuit, but sometimes she does not. Sometimes the father smiles and tosses the child; sometimes he does not. And the child looks for signs of these varying moods and methods of treatment. Its new pains of disappointment arise directly on the basis of that former sense of regular personal presence upon which its expectancy went forth.

This new element of the child's 'social sense' becomes, at one period of its development, quite the controlling element. Its action in the presence of the persons of the household becomes hesitating and watchful. Especially does it watch the face for any expressive indications of what treatment is to be expected: for facial expression is now the most regular as well as the most delicate indication. It is unable to anticipate the treatment in detail, and it has not of course learned any principles of interpretation of the conduct of mother or father lying

* Observation made in her fourteenth week.

† I have special observations on H.'s responses to changes in facial expression up to the age of twenty months. Her changes of attitude indicated most subtle sensibility to these differences—and normal children all do, I think. Animals show the same remarkable 'projective intuition,' if the expression be allowed.

deeper than the details. It is just here, I think, that imitation arises, as I have said elsewhere,* and becomes so important in the child's life. This is imitation's opportunity. The infant waits to see how others act, because its own weal and woe depends upon this 'how'; and inasmuch as it knows not what to anticipate, its mind is open to every suggestion of movement. Its attention dwells upon details, and by the regular principle of motor reaction which imitation expresses, it acts these suggestions out.

All through the child's second year, and longer, his sense of the persons around him is in this stage. The incessant 'why?' with which he greets any action affecting him, or any information given him, is witness to the simple puzzle of the apparent capriciousness of persons. Of course he cannot understand 'why': so the simple fact to him is that mamma will or won't, he knows not beforehand which.

But in all this period there is germinating in his consciousness—and this very uncertainty is an important element of it—the seed of a far-reaching thought. His sense of persons—moving, pleasure-or-pain-giving, uncertain but self-directing persons—is now to become a sense of *agency*, of power, which is yet not the power of the regular-moving door on its hinges or the rhythmic swinging of the pendulum of the clock. The sense of personal actuation—'projective agency'—is now forming, and it again is potent for still further development of the social consciousness.

3. With all this, the child's distinction between the persons who constantly come into contact with him grows on apace in spite of the element of irregularity of the general fact of personality. As before he learned the difference between one presence and another—a difference which was overcome in the discovery that every presence is of irregular value: so now he learns the difference between one *character* and another—the regularity of personal agency as opposed to the regularity of mere associations of movement. Every character is more or less regular in its irregularity. It has its tastes and modes of action, its temperament and type of command. This

* *Mind, loc. cit.* p. 41 f.

the child learns late in the second year and thereafter. He behaves differently when the father is in the room. He is quick to obey one person, slow to obey another. He cries aloud, pulls his companions, and behaves reprehensibly generally when no adult but his nurse is present, who has no authority to punish him. This stage in his 'knowledge of man' leads to those active differences of conduct on his part which give to imitation, and the discipline of obedience, a sword with two edges, one for good and one for evil. This general appreciation of character, together with the full-blown social feeling, which constitutes the fourth phase in my division, may be left for later discussion.*

To sum up: 'personality-suggestion' is the general term for the stimulations to activity which the child gets from persons. It develops through three or four roughly distinguished 'stages,' all of which illustrate what I have called his 'projective' sense of personality—i.e.: 1. A bare distinction, on the ground of peculiar pain-movement-pleasure complexes, *of persons from things;* 2. A sense of the irregularity of the behavior of these persons, which is the germ of his *sense of agency*, as opposed to the regular causal series of conditions which things go through; 3. His distinction, vaguely felt but reacted to with great exactness, between the characteristic modes of behavior or *personal character* of different persons; 4. After his sense of his own subject-agency arises by a process of imitation, he gets what is really *social-feeling:* the sense of others as 'ejective,' i.e., as like and equal to himself.

* I have noted in the article already referred to in *Mind,* pp. 44 ff., the function of this stage of personal consciousness in the genesis of the moral sense.

SHORTER CONTRIBUTIONS.

SENSATION-AREAS AND MOVEMENT.

BY PROFESSOR W. O. KROHN,

University of Illinois.

Mr. H., who in the fall of 1892 was the 'coach' of a university foot-ball team, had his left forearm broken in a hotly-contested game, while playing his eleven against that of another institution. The surgeon who was called did his work in such a bungling manner that, after the bones had begun to knit, the arm had to be broken over again in order to set it properly. To keep it in the correct position a plaster cast was made which held it firmly. This plaster-of-paris case extended from the knuckles to a point above the elbow. After three months the case and bandage were removed. Of course, during the entire period when encased in the plaster, the forearm had not been moved either at the wrist or at the elbow. I then endeavored to test the sensibility of the skin on this arm which had not been moved for so long a time. To do this I applied the points of a pair of dividers or compasses which were separated more or less widely, after the manner of the usual aesthesiometric tests. The forearm was divided into four different areas for purposes of more accurate comparison with the sensitiveness of the skin on the uninjured right forearm of the same person. Without going into detail, it should be stated that on the left forearm—the one so long immovable—when the two points touching the skin at a given locality were separated by as much as 55 millimeters, they were felt as one instead of two; while on the right forearm they would only have to be about 20 millimeters apart in order to be perceived as two. On the back of the 'lame' arm at a different locality from that just mentioned, it was found that even when the two points of the dividers were 75 and 80 millimeters apart, they were felt as one; while at a corresponding locality on the right arm the skin was so sensitive that points but 17 millimeters apart could be felt as two. It must also be observed that this particular person, with reference to a large number of activities, had always previously been practically ambidextrous—indeed, he never had to favor the left arm at all. In his position as gymnasium instructor, he could (before the accident) manipulate the dumb-bells, Indian clubs, play base-ball, hand-ball, and the like, with the left hand as well as with the right. It would seem, then, that the sensibility of the skin over the injured forearm was lost simply because that member was for so long a time immovable. This has an important bearing on the oft-mentioned principle to the effect that "the localizing power is delicate in proportion as the skin covers a movable part of the body."

ADJUSTMENT OF SIMPLE PSYCHOLOGICAL MEASUREMENTS.

BY E. W. SCRIPTURE, PH.D.,

Yale University.

Given the measurements $a_1, a_2, \ldots, a_n$, to find the best representative value.

The use of the arithmetic mean

$$a = \frac{a_1 + a_2 + \ldots + a_n}{n}$$

involves the assumption that the deviations from the average, $V_1 = a_1 - a$, $V_2 = a_2 - a$, $\ldots$, $V_n = a_n - a$, occur with probabilities according to Gauss's law,

$$y = w_0 \exp(-\pi w_0^2 V_2),$$

where y is the probability of any deviation V, and w_0 is the probability for $V = 0$.

Bernoulli's theorem justifies the substitution of the actual frequencies for the probabilities when n is very large, thus enabling us to test the law by actual experience. The law has been verified for careful measurements in physical and astronomical work. It does not hold good for statistical and psychological measurements.

The use of the geometric mean

$$a = \sqrt[n]{a_1 \times a_2 \times \ldots \times a_n}$$

involves the assumption that

$$y = w_0 \exp\left(-\pi w_0 [\log a_i - \log a]\right).$$

This distribution of deviations has been approximately verified for statistical and psychological results.

As n can be made large only on exceptional occasions, the choice of the representative value is thus left largely to the discretion of the investigator. To avoid this uncertainty Fechner has proposed the use of the median or central value, whose position in the series of separate results arranged according to size is given by

$$\frac{a_1^0 + a_2^0 + \ldots + a_n^0 + 1}{2} = \frac{n+1}{2}.$$

That is, if all the results are to be arranged in the order of their size, the median value will be just in the middle. Since with finite units of measurement there will be a number of results having the same value around the middle, the value a will be determined by interpolation. The importance of the use of the median lies in the fact that it involves no assumption in regard to the distribution of the separate deviations. When the results follow Gauss's law the median is the same as the arithmetic mean; when their logarithms follow this law it is the same as the geometric mean. The arbitrary assumption of either law is thus excluded.

This method of adjustment, which seems logically the only correct one, has been employed for some time in my laboratory. The calculation of the median can often be performed mentally where that of the arithmetic mean requires pencil and paper and that of the geometric mean requires logarithms also. Cases have frequently arisen where the arithmetic mean unquestionably misrepresents the results, whereas I have never found any objection to the median that is not common to all representative values.

DISCUSSION.

JUDGMENT AS "THE COLLECTIVE BECOMING ABSTRACT."

The articles by M. Egger in the *Revue Philosophique* for July and August upon 'Judgment and Similarity' are a striking illustration of that most salutary psychological law that every one who thinks is sure to refute himself. He says that the judgment is not contained even potentially in the concept, although it is evident that the chief object of his articles is to get the concept out of the judgment. Strange that he fails to see how real his success is. Also he opposes those who have asserted the reducibility of association by contiguity to that by similarity, but says himself that all judgments are associations by similarity, and this, unless I be very much mistaken, is exactly what his imaginary foes have always meant. Throughout M. Egger builds more wisely than he knows.

'The collective idea becoming abstract,' which is M. Egger's definition of the judgment, is just what modern psychology means by conception. M. Egger unfortunately thinks of a concept rather than of conception, and from the concept, as the abstract idea or logical class, which, like its blood-relations, the idea-centre and the spiritual soul, has been exiled from psychology, it is not strange that he is unable to derive the judgment.

The abstract idea, that passed for the concept so long, was the unifying principle of a manifold of experiences. As Kant could write of space as *a priori* form, before the psychology of space as founded on movement or action, on the association of muscular sensations and 'local signs,' had been reported, so in general the abstract idea, as a formal principle, could be recognized and seriously used in psychological theory, before the processes of organic functioning, as the subject-matter of physiology, were exposed. But, just as now *a priori* space means nothing but the power of perfectly free movement, so the abstract idea, as a principle of unity, stands for free action; the abstract idea is not an idea at all, but an act: conception, from being classification, has become organization ; it is co-ordination of, or adjustment to, not logical or abstract unification of, a manifold.

With this view of conception it is easy to see that M. Egger has hit upon a very important principle when he banishes all relations of extensity or contiguity to the *terms* of the judgment, refusing to recognize any but judgments by similarity, the copula naming only similarity. As he says, with less emphasis than we could wish, the process of judgment has too long been explained through its merely external expressions in language, in some particular language of course, in French, or English perhaps, or German, so that the differences between thinkers have had rather a grammatical than a psychological basis. The judgment makes but is not the sentence. Also,—and here is an idea only very darkly and deeply implied in M. Egger's final definition of judgment,—so long as words, sentences, and the like are conditions or incidents of judgment, the act of judgment, as, for example, the assertion that A is B, is as much a judgment of space as my now seeing yonder chair in the corner of my study is a judgment of space. Surely all symbols are spatial. The concept element, then, in any judgment is the functional movement or adjustment that the symbol represents. The terms of any judgment are the inhibited movements, or rather the organization or adjustment, which demands the inhibitions, as attribute or predicated quality—the 'abstract idea' of former days, and the spatial images, symbols, or characters which the inhibitions bring to consciousness as subject or substantive. Moreover, within the subject properly belong all the facts of spatial relationship or quantity or contiguity.

The judgment defined as *the collective becoming abstract* is but another way of saying that the limit of conception is second nature or habit, and that, so long as habit is not yet acquired, the judgment, that is, consciousness in general, exists. So long as habit is not acquired there is a tension between substantive and attribute, between spatial symbol and the 'abstract' organizing act ; with the acquisition the judgment passes into a definition.

As geometry is the complete definition of space, and as it is a possible science only to such organisms as possess the fully developed power, or habit, of free movement, so definitions in general, in which both subject and predicate are quantified, the tension having ceased, are real, or possible, only to such natures as have realized the freedom which is defined. Science in general, as purely quantitative, has its psychological foundation in habit, freedom, perfected adjustment.

In conclusion, and with some repetition, the real judgment is always the consciousness ; it is not, as logicians and psychologists, even M.

Egger among the others, in spite of his coming so near to the truth, have usually imagined, the external sentence, which may interest the grammarian, but is sure to mislead the philosopher who looks no deeper. Language is no longer the heaven-sent blessing that we used to imagine it. But, secondly, the judgment in consciousness is a tension, a 'becoming,' which has its 'abstract' limit in the definition. The tension, finally, as between the abstract idea, or quality, one extreme of M. Egger's natural judgment, and the collective and substantial object, which contains within itself more or less precisely determined spatial relations of contiguity and quantity, and is the other extreme, ceases as the concept becomes realized in habit. Of course habit, as the realized concept, the acme of abstraction, is one and the same with definition; it sets or establishes the relations on which all science of the contiguous and quantitative rests.

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PSYCHOLOGICAL LITERATURE.

Psychology: descriptive and explanatory. A treatise of the phenomena, laws, and development of human mental life. GEORGE TRUMBULL LADD. New York, Charles Scribner's Sons, 1894. 8vo. pp. xiii, 676.

The fairest way to judge this book would seem to be to take it as the middle section of a complete treatise on psychology of which the author's *Physiological Psychology* should stand for the first part. A 'psychology' planned on this scale is as yet unparalleled in literature, but there appears little reason to doubt that if the good years be accorded him, Prof. Ladd will complete the task. Not only is his characteristic untiring quality present in all the pages of this great volume, but it has a fresher style and spirit, it seems to us, than either the *Physiological Psychology* or the *Introduction to Philosophy*, and suggests a tide of energy more characteristic of morning, or at least of noonday, than of evening, in the writer's intellect. I begin by noticing this partial nature of the present work, because it is not advertised as prominently as it should be either on the title-page or in the preface; and many readers, finding it to give so little development to physiological conditions on the one hand, and to theoretical discussion on the other, may judge it defective, from failing to perceive that it is only, as it were, the second play of a psychological trilogy of which the crowning piece is yet to come.

The word 'descriptive' on the title-page covers more of the contents than the word 'explanatory.' For while the book is by far the most minute and copious description ever written of mental operations as they offer themselves to the merely introspective eye, it deals very little with what most people chiefly have in mind when they ask for 'explanation,' namely, with the logical grounds or the efficient causes of the facts described. This preponderatingly descriptive character also makes a summarized *compte rendu* both difficult and unprofitable. The author has almost no opportunity for original opinions sweeping enough to quote, and the faithfulness to detail, which is the virtue to which he is mainly held, is demonstrable only by reading the original. Nevertheless I will attempt a brief notice of the course of his argument, before passing to what may be said in the way of criticism or appreciation.

Expressed in the broadest terms, Prof. Ladd's aim may be called the *unravelling of the fibres* of consciousness. Consciousness, like a rope, has for Prof. Ladd a fibrillated structure. Any portion of it which we may examine "is complex with an irreducible threefold complexity: . . . *it is fact of intellection, fact of feeling, fact of conation.* . . . One of these three 'aspects' may be emphasized, as it were, at the expense of the others; but no one of the three can be destroyed without destroying the psychic fact itself as an object of discriminating consciousness. . . . Each of the three . . . is peculiar in quality, unique, not to be confused with the others, or expressed in terms of the others. Intellection cannot be described in terms of feeling; neither can conation" (p. 58). Every concrete 'psychosis,' without exception, however one-sidedly one may for practical purposes name it, as an 'emotion,' a 'cognition,' or a 'volition,' is constituted of all three elementary kinds of fibre, inextricably interlaced. Yet in spite of this inner complexity every psychosis is a "living unity, . . . a marvellous and indescribable fulness of active life." And the whole series of these 'fields of consciousness' forms "a *continuum*, for the total expression of which the meagre separateness of processes and faculties seems a totally insufficient account" (p. 661). This continuity of development of the spiritual being always trebly living in this way is kept in view throughout the book. As in Spencer's psychology, every feature that is later evolved was germinally and nascently there already. "Out of the unconscious, somehow, does the conscious seem to come; the organization of mentality out of the confused and chaotic material of sensation and representation. Science can never put its finger on any definite moment and say, respecting the truly psychological, 'Now it is, for the first time, there!' Psychological investigation—no matter how, or how faithfully, conducted—cannot describe the mode in which elementary faculties come to be, without implying that they have already begun to do their work. But then this is not a disadvantage (if it be, indeed, a disadvantage at all) peculiar to psychology. Every physical science has to assume much more than this; it has certainly to assume formed conscious faculty as already at work; its universal formula is: *In the beginning was Mind, already equipped to see and hear and remember and imagine and think.*"

Experience unquestionably does evolve in this gradual way, so that we find faculties stealing so insidiously upon us that we know not how they arose. An exposition that seeks, like Prof. Ladd's, to be almost as gradual as the facts, must throw away the old-fashioned sharp partitions; and although it may gain by this in veracity, it loses in distinctness and literary effect. For instance, just as an immense number of special affective, intellective, and conative fibres go to constitute by their several interlacements the three great primary strands of consciousness, so these strands, according to Prof. Ladd, interlace in the higher unity of the Self or Subject, which at the same time that it discriminates them from itself and from each other, also relates itself and them together. Yet in the chapter on 'Consciousness and Self-consciousness,' where all this is set forth in detail, I find it impossible to be sure from the text whether self-consciousness is said to be there from the start or not (cf. pp. 32–35). I find a similar difficulty in chapters VIII, XV, and XVI, which deal with the consciousness of space. The 'problem' here is to 'explain' how things come to be perceived outside of us and spread out and related to one another in space (321). "Strictly speaking," says Prof. Ladd, "what we are seeking has to be at some point in the course of our descriptive and explanatory science assumed as already existing. . . . At some point every investigator is obliged to confess that his data of explanation begin to fail him" (322). Just where this 'point' of realizing the distinctively spatial quality lies in the development of the mind the text does not make clear. 'Extensity,' as an original sensation-content, Prof. Ladd does not believe in (144, 151, 327, 353). All the space-determinations ultimately perceived grow up gradually "by a constructive and interpretative mental activity that has been developed through experience" and in which the influence of motor processes fusing with passive sensation-complexes plays the essential part. They steal upon us, in a word, unawares. Nevertheless at the successive stages of the 'stealing' they have a definitely objective form of some sort; and the really instructive thing for an inquiry like Prof. Ladd's would have been by a process of unwrapping, as it were, and peeling off the later layers of the finished space-intuition, to disclose to the reader what the successively earlier cores of it *felt like* when they were the last results achieved. The space at last so explicitly apprehended would thus be revealed in its nascency and implicitness. But the account which the book gives, minute as it is, hardly enlightens the reader at all in this respect.

Among the most original parts of the work are the chapters on Feeling and Emotion. Prof. Ladd disbelieves in all the current theories—as well indeed he may. Feeling is neither a cognition of the well- or ill-being of the organism under the stimulus, or of the ideas under each other's hindrance or furtherance, nor is it a matter of mere pleasure or pain, without specific quality. *We feel, as well as know, the flow of the current of consciousness* (178), *the mind feels itself* (565), in countless individual ways that psychologically are inexplicable *ex alio*, although physiologically Prof. Ladd suggests instructively their condition. He finds it in the 'semi-chaotic surplus' of action set up in the brain-centres by stimulations and conditions that have not yet grown so consolidated as to occupy perfectly definite paths. This vague irradiation of currents forms "a *mélange* which gives conditions to one's affective disposition, or mood, or temporary impulse, so far as it is a matter of bodily feeling. When this *mélange* corresponds with that to which we are habitually accustomed, we feel 'like ourselves'; when it corresponds to any one of several familiar characteristic types, we feel in one of our several 'moods'; when it is largely unaccustomed, we feel 'queer' and 'not a bit like ourselves'" (175). It is consonant with this physiological view that Prof. Ladd should think lightly of the teleological function of pleasures and pains. The severer pains usually come too late to serve as effective warnings, and we must "confess that neither the existence nor the purpose of the definite amounts of pain and pleasure connected with certain activities of body and mind can be satisfactorily explained by psychology" (199). Of the emotions, too, "it is doubtful, taking all the facts of experience into the account, whether they are on the whole 'life-saving' and 'growth-promoting' functions of body and mind—in the merely biological meaning of the words 'life' and 'growth.' In the excessively intense form in which they all tend to recur, unless checked by the forces of an ideational and ethical development, the emotions expend life and hinder growth. . . . They are all significant of an 'overplus' which quickly becomes an 'overflow' of cerebral disturbance. . . . If the final purpose of life were merely to conserve and propagate itself, there would seem to be as little use for so many and strong emotions as for so much and such qualitatively varied pain. At this point psychology is compelled to hand over to ethical philosophy rather than to biology the larger problems started by the study of human feeling" (558). Between feelings, emotions, passions, and sentiments no sharp line can be drawn (199), it is merely a matter of the less or greater complexity of psychic elements in the *mélange*, and of the position of the emphasis. Prof. Ladd does not believe in the theory which would explain emotional feeling by the sensation of the 'bodily resonance' which the object or idea sets up. The resonance is due rather to the emotion itself (544), but its effects, returning to consciousness, reinforce and strengthen the original feeling and give it a ranker and more physical quality.—'Sentiments' are emotions with this coarse secondary accompaniment left out. The sentiment of the 'ought' is perhaps the most unique one of them all (583). It attaches to all acts judged *right;* but we have "no special faculty of 'conscience' as a matter of pronouncing judgments merely. . . . In making up the judgment [of right, or wrong], any amount of reasoning is admissible, for it is an affair of evidence more or less" (580–1).

The intellective processes are treated by our author in a peculiarly concrete and thorough fashion. Every *real* 'state' of mind being a state of knowledge, feeling, will, all three in each state (264), he keeps all the factors well in hand in his description. An 'idea' is 'life-like,' for example, just in proportion as the same feeling-tone and the same motor-impulsiveness cling to it which cling to the 'sensation' of which it is a copy (246).—In the stream of ideation he allows for automatic (or non-associative) processes by which irrelevant ideas may suggest themselves (260) ; and he considers that as an abstract principle of association 'similarity' can claim no legitimate place, contiguity being enough for the formulation of the facts (275–6). Where like suggests like, it is only because previous notice of the likness has already made the ideas contiguous. Thus the mind's discriminative, relational, and selective activity is both constantly deflecting the lines of experience and as constantly breaking up and remodelling what otherwise would be a passively unfolded associative content (290). From 'primary intellection' onwards Prof. Ladd traces this attentive, comparative, and synthetic activity which from the very first consciousness is aware of exerting upon its 'content.' It begins by noticing *resemblance*, and ends by the most elaborated rational thought. The higher intellective processes, memory, conception, judgment, reasoning, etc., are described with great wealth of detail, but our space forbids a *résumé.* The author's treatment of 'knowledge' is a little peculiar, and to us not quite clear. It is not conceived dualistically as a relation between the mind and a thing outside the mind, nor yet purely psychologically as a relation between conscious activity and its 'content.' It is a subjective fact (511), a mental procedure (512), described as a 'belief in reality' (513) ; but the reality is not as in Bain and Baldwin, e.g., the mind's 'object,' but a '*being*, existing in some state,' and "the specific character of this belief, in contrast with other beliefs, may be brought out by calling it 'metaphysical'" (513). Thus knowledge is a transcendent function, not an immanent content merely, but a function also immanent, since there would seem to go with it consciousness of itself. It seems as if Prof. Ladd ought to have led us either farther, or not quite so far into the puzzles of *Erkenntnisstheorie.*

The chapters on Conation, generally, including attention, impulse, instinct, desire, and will, strike me on the whole as being the best part of the book. The account of the Will in particular is an admirable performance. Whether this be due to an insight more felicitous here than elsewhere on the author's part, or to the fact that the volitional life, with its hesitations, inhibitions, and decisions, has a dramatic character which lends itself easily to striking literary treatment, I know not, but I am sure that there has nowhere yet appeared a psychological account of volition as concrete and life-like as the one contained in chapter XXVI of this book. Volitions without effort have been described, and so have volitions with effort, by previous authors. It is strange that no one should have emphasized like Prof. Ladd the class of volitions with the exact *opposite* of effort. These, says he, "are marked by a wonderfully grateful sense of *relief*. The will to 'let go,' to 'surrender the struggle,' to 'yield to desire,' etc., are volitions of this sort. So also, in cases where deliberation has been long and painful, the making of the choice is characterized by the very opposite of the feeling of effort. Even where the task set by the volition is in itself a severe one, whether of obvious bodily movements or of the control of attention and the train of ideas, it seems lightened as it is voluntarily assumed—so conspicuous is the feeling of relief accompanying and following the resolution of the *nisus* and the perfecting of the deed of will" (615).

There are so many opinions concerning details expressed in Prof. Ladd's book that we make no pretence whatever of enumerating them. The vital thing about it is its consistent holding to the view that each individual mind is an organic life that develops its own destiny. This life, though integral, is complex, and conscious of itself, of its 'passions' (in the older sense of the word) and of its actions, in the same indivisible 'state' in which it is conscious of its 'objects' or 'content.' Its 'ideas' are not entities, any more than its 'faculties' are. *Its total self* is the only entitative reality; and all accounts of it by abstracted elements are mutilations and abridgments of the truth.

As regards the originality of this treatise, it is strictly true that it is *independent* from beginning to end. The period of assimilation is past for the author; the raw materials have been brought into solution, and have crystallized out again spontaneously and naturally in the form that characterizes his mind. In this sense his pages are mellow and alive, and full of native observation and expression of belief. But with all the concreteness, honesty, veracity, and shrewd humor that I find, I can, with the best will in the world, find no one idea or argument that abides with me as an unforgetable addition to the subject. What does strike me with the force of freshness is the amazing thoroughness with which Prof. Ladd realizes the intricacy of his facts.

It seems to me little short of wonderful that a man should be able to make so many subdivisions, and find so many distinct things to say on the descriptive level. In this sense he *is* original, for no one has yet attained to writing up the subject in as fine-grained a way as this. But to be perfectly frank—and here I fully realize that the critic writes down his own shortcomings even more plainly than those of the author on whom he presumes to animadvert with his subjective epithets—I find this whole descriptive sort of treatment *tedious* as few things can be tedious, tedious not as really hard things, like physics and chemistry, are tedious, but tedious as the throwing of feathers hour after hour is tedious; and I confess that when I think of the probable number of virgin-minded youths and maidens, hungry for spiritual food, who, through the length and breadth of this great land, will now certainly be led over all these pages of fine print merely to get back,

> "Statt der lebendigen Natur
> Da Gott den Menschen schuf hinein,"

all these terrific abstract words and sentences, I feel a sort of shudder at the violence done to human want. It is not that Ladd *quâ* Ladd is a tedious writer,—I could name many eminent psychologists who are more tedious to me than he,—but that mere description as such, mere translation into words of what we already possess in living fulness in our bosoms, is bound to be tedious under any circumstances. To speak more soberly, could not the words have been much fewer, and yet have contained all the abstract truth one needs to know?

These groans of mine no doubt proceed from the same idiosyncrasy that makes me demand that psychology shall be a 'science' in a sense different from that by which Prof. Ladd is satisfied. I desiderate 'conditions'; for Ladd 'analysis' and 'tracing of genesis and growth' are enough (p. 8). I cry for a 'Galileo or a Lavoisier' to lift us from this flat descriptive level, whilst my colleague says that he does not sympathize in the least with such "a confession of weakness—for example—because 'psychology is still in the condition of chemistry before Lavoisier,' nor look forward with the expectation that soon some Lavoisier will arise to rescue it from its depressed condition" (659). He thinks that all attempts to assimilate psychology to the other natural sciences are 'misleading' (*ibid.*). To me this lack of craving for insight into causes is most strange. Here is a flagrant mystery, that of the union of mind with brain, and we are apparently told that we must seek no reasons for it in a deeper insight into either factor!—told, in other words, that a mere narrative of the life of the spiritual being with its 'unique unity,' developing according to its equally unique laws, is the uttermost ideal of research—for Prof. Ladd's contention is hardly distinguishable from this. To me, on the other hand, it seems as if 'methodologically' the crudest cerebralistic theories, or the wildest theosophic ones about the seven principles of human nature, lead in a more healthy direction than this contented resignation. And as the theories of inheritance have killed the taxonomic and biographic view of natural history by merely superseding it, and reduced the older books of classification to mere indexes, so will the descriptive psychologies be similarly superseded the moment some genuinely causal psycho-physic theory comes upon the stage. Not that they will be judged false, but that they will then seem insignificant. Alas that my learned Yale co-editor will not join with me in saying :

> "Ring out, ring out, *our* mournful rhymes,
> But ring the fuller minstrel in" !

W. J.

The Senses and the Intellect. A. BAIN. Fourth Edition. (To be published in April 1894.)

After a considerable interval of time, in the course of which psychological investigation and discussion, both in Europe and in America, has been more actively carried on than during any former period of philosophical history, I now, for the last time, re-issue this work, with such additions, modifications, and emendations as have commended themselves to my mind. I have endeavored to take full advantage of the numerous suggestions in contemporary philosophical literature, and, while adhering to the main points of doctrine, and the general plan of arrangement, I have introduced improved forms of statement, and corrected what I deemed either inaccurate or imperfect in the expression.

In regard to the physiological portions, the chapter on the Nervous System has been entirely rewritten. This task has been executed by Dr. W. Leslie Mackenzie, medical officer of health for the counties of Kirkcudbright and Wigton, who has spared no pains to embody the results of the latest authorities. I have profited by his assistance, also, in improving the physiology of the senses. My conviction of the propriety of bringing these topics before the student, notwithstanding the adverse opinion of many, has been strengthened rather than otherwise. It is not merely that the definitions and the doctrines of physiology have a direct application, and that their absence would make psychology poorer in its own province,—it is, further, that the expression of mental states is, in many ways, aided by reference to their physical adjuncts. Even when such adjuncts are so imperfectly known as to have only a hypothetical rendering, the mention of them is still valuable in improving our scanty resources of subjective delineation. Perhaps it may be said that the student should refer to works of anatomy and physiology for this special instruction,—which is quite true. At the same time, the including of a suitable physiological selection in a treatise of psychology proper has high expository value.

It is now generally recognized that systematic psychology should be disburdened of metaphysics—that is, the problem of knowing and being—however closely they may be connected. To analyze subject and object is a strictly psychological task : the nature of our perception of a material world is something different and apart. Likewise what is now termed epistemology has psychological relationships, but is pursued into issues of a specific character, lying outside pure psychology.

The chapter on Instinct, which contains the fundamentals of pleasure and pain, together with their physical embodiment and expression, and the germ of volition, has been so far recast as to make more explicit the distinction between the physical and the mental, while assigning due force to each.

The supposed origination of our mental products, known to us only in their maturity, has entered largely into psychological inquiry. Whether certain fundamental conceptions—such as space, time, cause, the moral sense, the ego or personality—are instinctive, or grow out of experience and education, has long been the battle-ground of the philosophy of mind. The controversy may have a somewhat factitious importance ; at all events, it is regarded with more than merely speculative curiosity. The argumentative treatment, however, has assumed a new aspect from the doctrine of evolution, taken in the guarded form of the hereditary transmission of foregone aptitudes or acquirements. Instead of Kant's contention that the notion of space, as a 'form of thought,' is prior to any experience on the part of each individual, the question now is whether or not we possess at birth a large contribution towards the full realizing of the three dimensions of the extended world. Such a mode of looking at the problem changes the whole character of the research into origins ; depriving us of the right to define the absolute commencement of any of the great fundamental notions, and leaving us merely to watch their accessions of growth within the sphere of our observation, and to reason by analogy as to their probable course or manner of growth before entering that sphere. It may, however, be still argued without fear of rejoinder, that experience or acquisition is the remote genesis of what transcends our available sources of knowledge. The qualifications introduced in the present edition of this work, having reference to experience as opposed to instinct, have taken shape in accordance with the leading hypothesis above sketched.

The plan and object of the present work, as well as of its continuation, *The Emotions and the Will*, having been conceived more exclusively with a view to practical results, I have seen no ground for materially altering the expository order and the proportions, in the laying out of the details.

The retcntive power of the mind, which occupies the largest division of the intellectual powers, has received some additions, with a view to elucidate still further the more complex bearings of the recuperative process.

I recognize, in the broadest sense, the possibility of advancing psychological doctrines by means of well-contrived experiments. The researches usually called psycho-physical have already borne some fruits, and hold out still greater expectations for the future. They can, at best, cover but a small portion of the wide domain of psychological research ; but, if pursued with a clear recognition of introspective concurrence, they may accelerate the pace of psychological investigation, more especially on the side of practical usefulness.

The account of the psychology of Aristotle, contributed by Grote to the previous edition, having been embodied in his own posthumous work on Aristotle, is here omitted.

Subsequently to the publication of the former edition, I appended a postscript, containing a minute and exhaustive criticism of the psychological parts of Darwin on Expression. This has been retained in the present edition. It serves the purpose of expanding the treatment in the text, and also of illustrating at length the alternative positions as to the respective priority of emotion and volition in the order of development. [Preface.] A. BAIN.

ABERDEEN.

Grundriss der Psychologie. O. KÜLPE. Leipzig, Engelmann, 1893. pp. 478.

The psychological literature in Germany has shown during recent years a surprising lack of comprehensive expositions. While the English literature has produced the works of James, Sully, Ladd, Baldwin, Dewey, and others, Germany has only a few corresponding books. Appearances indicate, however, that a change will soon come. Psychologies by Külpe, G. E. Müller, and Ebbinghaus are announced, and, as it may be expected that all these works will be quite modern and yet written from very different standpoints, the blank will be filled in a short time and in the most desirable way. The book by Külpe is now in our hands, and it may be said at the outset that it is a very original, valuable, and suggestive contribution to modern psychology.

The chief part (400 pages) of the work is divided into two halves, the first giving the elements of consciousness, the second treating the combinations of psychical elements. Besides this main part, thirty pages on the purpose, methods, and literature of psychology form the beginning of the book, and thirty pages at the end are taken up with states of consciousness. The part on the psychical elements is then divided into a larger chapter on sensations and a smaller on feelings, while the part on combinations (Verbindungen) is divided into fusions (Verschmelzungen) and connections (Verknüpfungen).

The first principal division, then, discusses sensations. Their classification is made from a purely physiological standpoint; all other points of view are expressly rejected. Sensations are those simple processes of consciousness which are in the relation of dependence upon special nerve-organs, and the first appearance of which needs the stimulation of peripheral organs. As the latter is not necessary for subsequent repetitions, the sensations are classified into such as are produced by peripheral stimulation and such as are of central origin only. These sensations are separated into groups merely by the difference of the sense-organs. For instance, the skin sensations are only one group, since we do not know the anatomical differences between the organs for touch sensations and those for temperature sensations; if we knew these anatomical differences, these sensations would make up two different groups.

A sensation can have four primary characteristics: quality, intensity, extension, and duration; on the other hand, the tone of feeling is not a property of the sensation itself. Only quality and duration are characteristic for every sensation; extension belongs only to optical and tactual sensations, and intensity is wanting in optical sensations, since all the variations of the intensities of optical stimuli produce variations of quality. The optical sensations are therefore not mentioned at all in the whole chapter on intensities and on Weber's law, but the questions relating to them are discussed in the extremely original part on the quality of visual sensations. In the following chapter on sensations of central origin not only their qualities and immediate presuppositions are discussed, but also the general conditions of their existence, especially memory and imagination. A valuable criticism of the doctrine of association and a discussion of the motives and of the exactness of reproduction are here in the foreground. Külpe here brings forward a dynamic theory as opposed to the usual cell theory of association-paths ; it is not on the difference of anatomical relations, but on the difference of physiological functions, that the associative relations are based.

For the second group of psychical elements, the feelings, the possibility of a classification is denied. A detailed treatment is given to the elementary æsthetic feelings, which are interpreted as resulting from the relation of the perceived impression to the reproductions which it suggests. As a physiological condition for the feelings special central processes are presupposed. Opposition is made to the existence of a psychical element which corresponds to the will ; the processes of the will are represented in consciousness by sensations and feelings only.

The second part, which treats the combinations of psychical elements, describes them from the point of view which considers whether the elements in the combination fuse or not. A fusion exists especially for sounds, but also for light and color-tone, and finally in emotions and impulses. Emotions and impulses are all those states in which a fusion of sensations and feelings exists. In the objective emotions the bodily sensations preponderate, in the subjective the feelings, while in the impulses those sensations are predominant which result from the movements that correspond to the feelings. Those psychical combinations which are not in a state of fusion are mere connections. From this point of view the connections in space and time are studied and the extension and duration of sensations and their relations in space and time examined with much force. As extension and duration are just as much properties of the sensation as quality and intensity, the task is only to show the dependence of the detailed spatial and temporal factors upon our psycho-physical organization. The problem of space, which is reduced to the problem of extension and distance, is discussed at first for the tactual sense and then for the eye. Particularly is the attempt made to prove that the idea of space is independent of the movements of the eyes. The problem of time leads to a discussion of the duration of sensations, of the comparison of intervals, and of temporal order and frequency Next come the special mutual relations of elements which are connected in space and time. In the field of space the contrast of light sensations is emphasized, in the field of time the processes of simple and complicated reaction. Finally the last chapter discusses the state of attention, which is interpreted as a psycho-physical condition of inhibition.

This short review is unable on the one hand to do justice to the richness of valuable details, or on the other hand to accentuate all those points which to most psychologists will appear debatable or absolutely untenable. But even this glimpse of the book makes clear its principal points of weakness. As such I consider above all the narrow limitation of the material. The book is not a psychology, but only the middle part of a psychology. The book treats the psychical elements and the forms of their combination, but we miss on the one hand the description of the special combinations and on the other hand a general discussion of fundamental psychological notions. The book appears to me like the second volume of a psychology of three volumes, the first volume of which was to have discussed the general problems and the third the special presentations, judgments, emotions, will, personality. I miss especially a thorough treatment of those questions which are on the borderland between psychology and epistemology. To be sure Külpe is not at all one of those who consider a detailed discussion of epistemological problems superfluous, but all that he offers in this book is in a way left unfinished and indecisive (for instance, in the question how far a causal connection of psychical facts as such can exist at all) or, when it comes to a definite statement, often entirely erroneous. For instance, consciousness itself is, according to Külpe, given in space and time because it has the sensations of duration and extension, and the difference between the physical and the psychical world is that, while both are experiences, only the psychical experiences are dependent upon our psycho-physical organization. It seems to me that both worlds are experiences from an epistemological standpoint, but from this standpoint no psycho-physical organism exists ; on the other hand, from the standpoint of empiricism, which is presupposed in the acknowledgment of a psycho-physical organization, the physical processes are not at all experiences, but processes that are independent of experience.

If we confine ourselves to those questions of psychology which are really discussed, the general plan seems to suffer especially from the classification of the material. I think it wrong to classify the sensations from a purely physiological point of view, and to explain the whole doctrine of association, memory, imagination in the chapter which describes the characteristics of reproduced sensations. Above all I regard it impossible to treat all the complex phenomena from the standpoint of fusion. To be sure the notion of fusion is of highest importance, but it cannot be the basis of a classification. Notwithstanding the fact that Külpe gives only types of fusing and non-fusing combinations, each group contains entirely different things, while often the closest relations are severed. Harmony belongs to one main division, the sequence of sounds to the other, and so forth.

In regard to details, most erroneous of all seems to me the way in which he combats the doctrines of associationism. His own account of the facts suggests nothing else than a purified associationism. But in the place of this he tries to substitute a psycho-physical theory which gives up the advantages of associationism without gaining anything. It seems to me that Külpe does not recognize clearly enough the real difficulties. For instance, attention, which it seems to me he regards in the only right way as an inhibitory function, cannot be explained according to Külpe by the mutual influence of psychical elements, but only by the help of a special inhibitory organ. The very difficulty is indeed to explain how the functions of this organ are continually under the influence of psychical motives ; the inhibiting organ should have again a special brain at its disposal. In any case the simpler theory seems to be that the inhibitions result directly from the action of the psycho-physical elements. In a similar way I do not see what the dynamic theory of association can substitute for the theory of separate association-paths. Külpe is right in saying that the sensorial centres are dependent upon the sense-organs just as the muscles are dependent upon the motor centres. But that very fact opposes his theory. A succession of muscular movements results solely from a succession of central motor impulses. The corresponding fact would be that central sensorial processes follow one another only when successive peripheral stimuli are given. But the question of association is exactly this—how central sensorial processes can succeed each other when no peripheral stimulations are given. Between muscle and muscle, therefore, no direct connection is necessary, while the presupposition of a connection between the central sensory organs is the only possibility of a psycho-physical explanation, and such connection is possible only if the different psychical qualities correspond to local differences in the central system. Besides this a difference of elementary central function, corresponding to the different muscular actions, is acknowledged by the usual theories to exist in the varying intensity of sensation. The weakest part of the book seems to me the chapter on space. It is to be regretted that Külpe makes too little use of the non-German, especially of the English, literature.

More important, however, than pointing out the failings of the book is, it seems to me, the mention of its essential merits. As especially good I regard the chapters on the conditions and measurement of sensibility, on the quality of visual sensations, on the fusion of tones, on the qualities of the will, on the emotions, and on reaction. But in addition to the single sections I must mention certain general characteristics which are predominant in the entire work and which are especially important for psychologists. The book is extremely clear. The reader always knows exactly what Külpe means. In the second place it is always consistent in the use of the various conceptions; the conceptions are not worked up differently for the various chapters, but are resolutely retained, although the problems themselves are not always as resolutely thought out to the finish. In the third place Külpe knows exceedingly well how to separate the essential from the unessential, and how to save the reader from the latter. With the exception of the methods for measuring sensations, the treatment of which is too extended, the choice of subjects seems to me excellent. Even Hipp's chronoscope is not mentioned in the whole book. In all these respects the book seems to me to far surpass Wundt's treatment of the same fields. In other respects also Külpe's book is thoroughly original and independent. This appears first in the classification of the material and is seen on nearly every page. The book has nothing in common with a mechanical textbook. It has a thoroughly individual character, and no psychologist can afford to ignore it.

Unfortunately I am forced by the state of affairs to add a word in regard to a more formal aspect of the book. Heretofore Külpe has appeared mostly as a critic merely, partly in critical articles, and partly as a regular critic in the *Literarisches Centralblatt.* Even in the present book Külpe the critic makes himself strongly felt, often more by what he openly ignores than by what he attacks. Külpe's method of criticism, however, deserves the severest censure. To be sure he did not invent the method, it has grown up with the *Philosophische Studien*, and many younger members of the Leipzig school thoroughly concur with Külpe in this misuse of criticism. The essential feature of this method consists in the ability to act as if there is nothing but light in the writings of friends and nothing but darkness in the writings of opponents, although in both light and shadow may be equally divided; but above all in making a personal mention of friends only when agreeing with them and in bringing in opponents only when attacking them. If one reads Külpe's writings and criticisms, one learns, e.g., that Wundt and I represent the extremes of psychology, in the sense that Wundt, whose assistant Külpe is and to whom the book is dedicated and with whose praise the book is full, represents the highest point; while I, who have written only the most pitiable trifles, which are really worthy of no serious criticism, represent the lowest point of weakness in psychological literature. Whoever examines the contents of the book seriously finds that, with the possible exception of the doctrine of movements of expression, Külpe is really never in agreement with Wundt. Indeed the whole book is an energetic and, with the exception of the doctrine of space, a decidedly successful struggle against Wundt. On the other hand I am pleased to see that Külpe agrees with me in the most essential points and is now in harmony with me even on those questions in which not long ago he opposed my standpoint in a most unfriendly manner. To be sure such unjustifiable attacks trouble me (and probably everybody) personally very little. But in the interests of science it cannot be too energetically pointed out how much science must suffer when scientific and personal matters are confused. Of course I could easily have made use here of the same method and could without trouble have brought together everything in Külpe's book that is to be criticised and passed over in silence all that deserves praise. Instead of this I emphasize with great pleasure my belief that Külpe's book is one of the best psychological productions of recent years, and express my hope that an English translation will soon be forthcoming. H. M.

Psychologie des Erkennens vom empirischen Standpunkte. GOSWIN K. UPHUES. Erster Band. Engelmann, Leipzig, 1893. 8vo, pp. 318.

We are glad to welcome a new book by Professor Uphues, and are not disappointed in our expectations of finding it learned and thoughtful, as are his others. I need not here speak of his earlier works, which are so well known that to do so would be superfluous. Before plunging, however, into a criticism of his 'Psychology of Cognition,' it may not be amiss for me to mention (as it can hardly be known except to those directly interested in education) his extreme kindness to our American students who have wandered abroad in search of philosophical culture—a kindness which has laid not only those who have profited by it, but also their teachers, under no small obligation. Those who are acquainted with the facts to which I make this passing reference will feel quite as much indebted to him for his labors in this direction as for his literary productions, for we still believe on this side of the water that it is the function of a teacher to teach, and that time spent in opening a path for younger scholars is by no means time lost.

The volume before us is largely taken up with the problem of perception and the genesis of our idea of an external world. In the preface we are promised a second volume, which is to treat of the judgment.

Prof. Uphues rightly distinguishes between psychology and metaphysic; and maintains that in the former we study merely mental states, and are justified in putting to one side, as belonging to another discipline, what has been regarded as their implication, a 'transcendent,' an external correlate which is represented by them or in them, but which is itself wholly distinct from them and beyond them. The question of the existence or non-existence of such a 'transcendent' he thinks must be relegated to metaphysics, but the fact that we do have states of consciousness which we must regard as 'images' of a transcendent he accepts as a purely psychological fact, and one which must be reckoned with in any scientific investigation of cognition.

"The scientific investigation of knowledge has, hence, first of all to fix its attention upon knowledge of the transcendent. One may here raise the question whether we really know the transcendent—the question as to the truth of our knowledge of the transcendent ; and it is only in the answer to this question that we can find an answer to the questions concerning the possibility of a knowledge of the transcendent, and concerning the existence of the transcendent. All these questions are metaphysical and have to do with the transcendent which constitutes the object of metaphysics. But one may also raise the question, how this knowledge of the transcendent is effected, making complete abstraction from the questions whether this knowledge is a true or merely a seeming knowledge of the transcendent, whether knowledge of the transcendent is possible, and whether a transcendent exists. This question of psychological fact constitutes the object of the psychology of cognition. Knowledge of the transcendent presents itself to us immediately as a conscious state, and thus as an image of the transcendent. To this we can and must hold fast, even if there be no transcendent, if a knowledge of the transcendent be for us impossible, or if our knowledge of the transcendent lacks all truth and is a mere semblance of knowledge. That our knowledge of the transcendent is in this sense an image of the transcendent is a fact of consciousness which no one can deny. An image is an image only in virtue of its correspondence or agreement with something ; but the thing need not be real, it may be fictitious" (§ 45).

In every percept we are thus concerned with the transcendent, and our percept is *of* something external, it has objective reference. Metaphysic may decide there is no such external object, but our percept remains what it is, and still *seems* to be a knowledge of such an object. What this transcendent may be Prof. Uphues does not attempt to state. In analyzing our percepts he distinguishes between the impenetrability of an object and all its other qualities, reserving to the former the name 'thing.' He recognizes that the whole object as known (the 'thing' with its qualities) is a complex in consciousness, and contains no element which may not be described as a consciousness element. Nevertheless the percept is *of* the transcendent, and in it the transcendent somehow 'comes to consciousness.' I shall cite one more passage, which brings out clearly his position and contrasts it with others:

"In psychology we leave it wholly undetermined whether transcendental objects exist or not, and that not merely as regards objects which are not things, but also as regards things. For us, things and objects that are not things stand in this respect upon precisely the same plane, in contrast to the assumption of the common consciousness, which regards the sense of touch, in so far as it gives us information about things, as peculiarly the sense through which we gain a knowledge of *reality*. Our view is in contrast also to that of many philosophers, according to whom things and things only constitute the unknowable transcendent, while objects that are not things are merely immanent. It is in contrast, finally, to the view accepted in natural science, which distinguishes between the mechanical and mathematical and the 'secondary' qualities. According to it, the secondary qualities are subjective, that is, they are in themselves only sensations, while the mathematical (the extension, at least) and certainly the mechanical (resistance, density, weight) pass as objective, as the transcendent, real, so-called mechanical correlates of the secondary qualities. For us, all these qualities, the mechanical and mathematical as well as the secondary, are primarily sensations in which we represent to ourselves something transcendent, and which in so far constitute an expression of the transcendent, that is, of a something which is not a state of consciousness. I say primarily, for, since for our consciousness the sensations are inseparable from the qualities, the latter form the first element in the concept of the former. Yet the names which we give to the qualities can, properly and strictly, only apply to the transcendent, and hence we can best define the qualities as the transcendent, of which we become conscious in these sensations" (§ 97).

Thus we see that the external world is for Prof. Uphues simply a transcendent, a something closely resembling the Unknowable of Mr. Spencer. Locke's distinction between the primary and secondary qualities of bodies is not ignored, but the difference is not made to lie in the fact that certain qualities have external correlates while others have not. All qualities alike are, from one point of view, states of consciousness, and the only thing beyond states of consciousness is this vague and indefinite transcendent. States of consciousness and qualities are not distinct things, but the same thing regarded from different points of view. The statement that the names we give to the qualities can, strictly speaking, apply only to the transcendent must not be taken as favoring the Lockian view that qualities can exist beyond consciousness. It rests upon the familiar fact that we all speak of *things* as round, hard, heavy, colored, and never so speak of the *ideas* of the things. This fact is undisputed, and it has, of course, its significance, but one may well question whether the average man in so speaking ever dreams that he is applying his adjectives to a transcendent of the sort accepted by Prof. Uphues. His thought is probably much more in harmony with that of Locke, who held substantially the view accepted in natural science. If one accept external correlates to percepts—bundles of real qualities, so to speak, in a real external world—one may mean something definite in saying that external things are round, hard, and heavy; but if one hold that all roundness, hardness, and heaviness must be found in consciousness, and that no determinations can be given to what is beyond, it does not seem to mean anything in particular to say that the transcendent is round, hard, and heavy. One may thus express, if one choose, the belief that round, hard, and heavy are in consciousness, and that there is a transcendent without, but it does not appear to add to the thought to call the former an 'expression' of the latter.

In thus abandoning the natural-science point of view, Prof. Uphues has, it seems to me, abandoned the psychological standpoint and passed over to the metaphysical. He does, as we have seen, distinguish between psychology and metaphysics, and leave to the latter all questions relating to the existence of the transcendent and the way in which we come to know it. But he keeps his transcendent in psychology as a seeming at least,—it comes to consciousness (whether it exist or not), and its coming to consciousness is what makes certain mental states *knowledge*. I cannot but think, however, that this transcendent, which is grasped through sensations, and which is in no sense an external world as it exists either to common thought or to science —I cannot but think that this has no place in psychology at all, and that it must not be used to distinguish between some mental states and others. I have so lately printed in this Review a discussion of the psychological standpoint that I need not enter at length into the question here. Suffice it to say that the position taken by the psychologist appears to me to be substantially the same as that taken by the student of natural science, who accepts unquestioningly a real external world; not the world of a vague and indefinite transcendent, a *Ding an sich*, but the phenomenal world of matter and motion. The psychologist distinguishes between things in such a world and his ideas of them, regarding the latter as in some sense copies or representatives of the former. To him the copy and the original are not one thing looked at in two different ways, but two distinct things. With a transcendent underlying the external thing—the phenomenal thing—he has, as psychologist, nothing to do.

Prof. Uphues does not, as the citations given above show, accept this distinction between the thing as a bundle of qualities and the idea of the thing as a representative of this. In the conscious states we are to find both ideas and qualities, the distinction being rather a logical than a real one. Nothing lies beyond consciousness but the transcendent, and this is a mere postulate (§ 54) and in no sense contained in sensations (§ 78). Yet conscious states are to be distinguished as 'grasping' or not 'grasping the transcendent. What does this mean? It cannot mean simply that the mental states in themselves considered differ from each other, and mean no more than that. If that be all, why bring in the transcendent? What has a transcendent to do with the matter? The fact is that Prof. Uphues, after drawing a clear line between conscious states and a transcendent beyond them, overlooks the division and really makes the conscious states reach across and appropriate what is beyond. The language that he uses would almost unavoidably lead him to forget from time to time that the two spheres of being are supposed to be quite cut off from one another. He speaks of conscious states as 'directed to' the transcendent (§ 7), and of the transcendent as 'coming to consciousness' in sensations (§§ 37, 67, 75); it is a fact of consciousness that knowledge of the transcendent is an image of the transcendent (§ 45); some knowledge is consciousness of an object beyond consciousness (§§ 71, 72); perception is a representation of the transcendent in sensations (§ 74); both perceptions and ideas are directed to transcendent objects, and in both cases the objects are *really present* to consciousness (§ 79); the transcendent is grasped in its expression in consciousness, and is, in perception, inseparable from its expression, the percept (§ 77). In speaking thus it certainly seems to me that Prof. Uphues either drags the transcendent into consciousness, or allows consciousness to transcend itself and go out to the transcendent in some (to me) incomprehensible way. If the use of these expressions be justifiable, I do not see the need of a metaphysical postulate to guarantee us the existence of this transcendent. We are conscious of it, and what more do we want? But then, if we are really conscious of it, it is not a transcendent at all, and the distinguishing mark of a percept must not lie in its reference to a something beyond consciousness.

It may be objected that the psychological position, as I understand it, and as, I think, it is generally understood, admits one's right to thus reach beyond a conscious state and relate it to its object. This is true. But there is an important difference between referring a percept to its external correlative as one does in psychology, and referring it to a transcendent of the sort accepted by Prof. Uphues. The external world accepted by the psychologist is in no sense regarded as a postulate, a something *merely* assumed and for the existence of which no direct evidence is forthcoming. On the contrary it is held to be given in experience, and the psychologist's reasonings concerning his own mind and other minds proceed upon the assumption that it is so given. The psychologist who infers from the actions of another man's body the ideas in the other man's mind assumes that he perceives the other man's body, and he does not merely postulate it as a metaphysical entity underlying what he really perceives. And he feels justified in passing from the actions in question to the ideas they are supposed to reveal, because he regards himself as having direct experience of a connection of ideas with bodily expressions in the case of his own body. When he goes on to the conclusion that every consciousness is shut up to the circle of its own ideas, and can have no direct and immediate knowledge of anything beyond, he undoubtedly maintains what is inconsistent with his primary assumption, i.e., that he has direct evidence of the existence of his own body and other bodies. This inconsistency I do not think it is incumbent upon him as a psychologist to remove. He may leave it to the epistemologist. What is important to note here is that the psychologist, having drawn a distinction between ideas and external things, does not regard the things —the external correlates—as merely postulates, but bases his notion of the correspondence of idea and thing upon experience. He may say that any particular man cannot know whether his supposed percepts are really percepts or mere hallucinations, but his whole construction demands that both *percept* and *thing* be given in experience somewhere, and to some one, for all his reasoning is based upon such an assumption. With a transcendent which can only be gained by 'the leap of a postulate' the psychologist, as such, has nothing to do; its title is vested in the metaphysician.

It is because Prof. Uphues abandons the natural-science standpoint, and reduces his external world to a transcendent of this description, that I have accused him of leaving psychology for metaphysic. However, the position he takes is not an uncommon one, and he is in good company where he is. He certainly intrenches himself with skill in his position.

So much for the main thesis of the book. have criticised it without turning aside to pay the author merited but unnecessary compliments—merited, because the volume contains so much that must seem admirable even to one who dissents from the doctrine it contains; and unnecessary, because one knows what to expect in a volume from Prof. Uphues' pen. We anticipate with pleasure the publication of his second volume, and also that of the *Psychology of Volition* which he has announced. G. S. F.

Appearance and Reality: a Metaphysical Essay. By F. H. BRADLEY. London, Swan Sonnenschein & Co., 1893. pp. xxiv, 558.

The humorous aphorism 'once an advanced thinker, always an advanced thinker' is one of Mr. Bradley's titles to fame. This book is another. It is among the solidest of the contributions to philosophy in recent years. It is reasoned with a minuteness that is quite Teutonic—it is a pleasure to read an author who so bristles with points. Unhappily the book is, for the same reason, difficult in any brief space to review.

Appearance and Reality is a constructive argument for the Absolute, with answers to objections. In the barest outline Mr. Bradley's main position is this : experience (which is co-extensive with consciousness) is a maze of inconsistency, so far at least as any finite individual knows it. It is not therefore reality, but appearance merely; for reality must be consistent—*that* is its distinguishing mark. But on the contrary all experience must be reality, and reality is one. Hence the problem, how can a consistent whole be formed of parts that are inconsistent? The solution is in principle striking by its lack of novelty : the inconsistency in the parts is perceived upon but a partial view ; a complete view might well show a perfect harmony. But since this harmony must exist, and since it is not impossible that it should exist, therefore it does exist, though how it can, passes human understanding.

That all experience as finite individuals find it is a maze of inconsistency, is too large a subject to be dealt with here. Mr. Bradley handles it under a dozen such commanding heads as Relation and Quality, Space and Time, Causation, Activity. I shall simply say that on his own premises his treatment is strong ; but havoc might be made among his premises. The next point, however, is compact—something may be done with it. It is that consistency is the distinguishing trait of reality. We should suppose just the contrary—we require proof of that. Since all experience, as we know it, has just been proved inconsistent, and since it is undoubtedly real, the natural presumption would be that some reality, maybe all, is inconsistent. Mr. Bradley's reply is (p. 136) that reality must be consistent because we have all along assumed that it is. This sort of proof by previous assumption is not infrequent in the book. Mr. Bradley adds (pp. 136–7) that we are obliged to assume it—that either in endeavoring to deny it, or even in attempting to doubt it, we assume it, tacitly.

That one cannot help assuming a thing is ample *cause* for assuming it, but is not a warrant in logic for doing so. That one cannot doubt a thing without assuming it may in point of logic be the best of grounds for pushing doubt to the extreme. Those who urge the contrary are singularly short-sighted. Their stock example is the reply to the sceptic. "You try," they say, "to prove that human faculties are incapable of knowledge, and employ them to do so. But if they are incapable of knowledge, your conclusion cannot be true." "That is just the point," says the sceptic; "if human faculties lead to conclusions which cannot be true, they confess themselves unsound—they commit suicide." Mr. Bradley's reality confesses itself unsound—commits suicide. All reality we were told is consistent, and all experience is reality. Holding fast by these two assumptions, we reached a conclusion which contradicts them—viz., that experience is inconsistent. It would seem that we must doubt some part of our assumption. Either reality is not consistent or experience is not reality. Mr. Bradley finds it more logical to cling the more closely to the assumption which has failed him. I assumed it all along, he says; I cannot abandon it now. It must be true, for—it has proved contradictory. "What can be more irrational," asks Mr. Bradley farther down the page (137), "than to try to prove that a principle is doubtful, when the proof through every step rests on its unconditional truth?" One should rather ask, what can be more irrational than to cling to a principle the consistent assumption of which leads to its contradictory?

But Mr. Bradley anticipates this turn—to do him justice there are few turns which he has not anticipated. What he says in bar of your objections may not satisfy you—it may be merely a reference, a word, curt as you please—but he has at least anticipated you—considered the matter your way. He has done so here. "It would, of course, not be irrational," he says (p. 137), "to take one's stand on this criterion (of consistency), to use it to produce a conclusion hostile to itself, and to urge that therefore our whole knowledge is self-destructive, since it essentially drives us to what we cannot accept. But this is not the result which our supposed objector has in view, or would welcome. . . . He is not prepared to give up his own psychological knowledge, which knowledge plainly is ruined if the criterion is *not* absolute."

But I do not understand that this is the alternative. It is quite true that if the criterion of consistency is not absolute, one's knowledge of psychology and of everything else is logically bankrupt, but no Humean (and the objector may well be a follower of Hume) ever supposed that his knowledge was logically sound. Science, the Humean would say, does not pretend to logical soundness—it accepts the human faculties ; the basis of science in the last resort is not logical but psychological—that is the very point of departure that science makes with metaphysic. Metaphysic must be logically sound, science need not. Metaphysic cannot say—"Men think thus and so, it is the very nature of their faculties to assume such and such, therefore such and such shall be unquestioned." All other branches of knowledge do that—it is the distinction of metaphysic to do otherwise, to be more thorough, to question all presuppositions, to be logical or nothing. Science says to the intellect—'You are a laborer—work for me.' Metaphysic interposes and adds—'Perhaps you are a mountebank—come, lift yourself by your boot-straps.' The ambitious clown tries and fails. 'Away,' says Metaphysic, in derision, 'I will have nothing to do with you.' 'But I will,' Science says. 'But he can't lift himself by his boot-straps,' says Metaphysic in the person of Mr. Bradley. 'I don't require,' Science may reply, 'that he should. He is not perfect, but he's the best I can get, and I'm accustomed to put up with him.'

About the unity of the Absolute Mr. Bradley's remarks are too voluminous to be dealt with here. One may however say something of a single paragraph (pp. 141–2). He affirms that to be many is to be related, and to be related is to be dependent. One is moved to wonder whether Mr. Bradley ever settled in his own mind what he means by the Absolute : whether he means the utterly independent or that which is capable of being so ; that which has no relations or that which need have none ; that which actually is not relative or that merely which is not necessarily so. The first is of course a mere knot of contradictions—one cannot even declare it incomprehensible without making it relative as an object of curiosity, or speech, or uncertainty, or what you will. If it is for the Absolute in this sense that he is arguing, his reasoning refutes itself—the mere fact that he reasons about it at all destroys his conclusion. But if by Absolute he means the essentially independent, then his argument does not hold. One's own consciousness and that of some one—say in Central Africa—may be two and be essentially independent of one another. Things in relation do not necessarily cease to be absolute in the sense of independent. There are many relations in which the terms are independent of each other and of the relation, and in which the relation alone is dependent. X's consciousness is like mine, let us suppose, but I have never seen nor heard of X, nor X of me. In what sense is my consciousness dependent on his or on the likeness it bears to mine? No alteration in his consciousness will affect mine—it may even be destroyed, and mine will rest unchanged. There was no sign in mine of the relation while it lasted, there is no sign now that the relation is gone—in what sense did its existence make my consciousness dependent? In the sense that, as an object similar to something else, my consciousness varied with its variations, and with its destruction was destroyed? But my consciousness varied not at all and suffered no destruction; it was the relation only that varied and was destroyed—what better proof could be asked that my consciousness was independent, and that what was not independent was the relation? A change in either of the terms alters the relation—the relation can be altered in no other way; but a change in the relation has no influence on the terms.

But I confine myself, you will say, to my own consciousness. It is quite true that within that bound my consciousness does not vary with X's and with the relation that X's bears to mine. But I forget that it is not in my mind that the relation exists; it exists only in the mind of some third person, who knows both my consciousness and X's and their similarity; and that in that person's mind my consciousness does vary with X's and with the relation that X's bears to mine. To that person my consciousness is one thing while the relation lasts, and quite a different thing the instant the relation is changed; to that person my consciousness is at first a thing similar to X's, and afterwards a thing dissimilar to X's. But to this there is a very plain reply. It is an example of the psychologist's fallacy—it attributes to either term of the relation what belongs only to the thinker that knows them both. Within the bounds of my consciousness it was admitted that no change in my consciousness took place; but it did take place in the mind of a third person. Can a change in my consciousness take place outside of my consciousness?

But Mr. Bradley thinks that the mere fact that my mind and X's are known by a third would make them dependent. "If it [the mind] is known by another, then forthwith it cannot be self-existent, since this relation must clearly belong to its essence" (p. 143). Who was it said that when a metaphysician assumes what he cannot justify he states it with 'clearly'? A mind's existence is in no sense conditional upon its being known to another mind. It is enough if it knows itself. Whether it is known by another is the least essential of accidents—and in any strict sense one mind cannot be known by another; it can at most only be inferred or perhaps merely imagined.

But this, it will be said, destroys the possibility of minds being in relation at all—being similar, or different, or many; for, to be at all, a relation must be known, and how can it be known unless both its terms are in one mind? Let it rather be said that this destroys the notion that a relation to be at all must be known. "Plurality has no meaning unless the units are somehow taken up together," says Mr. Bradley (p. 141); it is quite sufficient, I reply, that they be capable of being taken up together—that my mind and X's are two, whether they are ever known to be so or not, providing they are capable of being known to be so (and capable, it may be, only through the defective medium of mental representations). In a word, the principle that *esse* is *percipi* does not apply to relations; to perceive that two things are different works no alteration in the things; if they are numerically different now, and have not changed from what they were before they were perceived to be so, they were numerically different then.

I cannot of course stay to justify this position in detail. Indeed my remarks have already taken on proportions that will tempt the editorial Procrustes, and yet there was another word I thought I should have room to say. "A separate real which is wholly self-dependent must . . . fall entirely beyond our knowledge. We have therefore no ground and hence no right to suppose it possible," says Mr. Bradley (p. 142). But this complaisant premise permits itself to be contradicted twenty times on the pages following. On every step in the argument for a wider harmony in which contradictions are reconciled, Mr. Bradley is perpetually telling us—"we don't know how—it is quite beyond our knowledge—but since it is not *im*possible, we must affirm it." But apart from this admission the fact is not obscure. It is quite beyond my knowledge that there are other minds than my own—my belief that they exist is founded on evidence confessedly inconclusive, worked up by a process of inference which is psychologically unavoidable and logically ridiculous. I cannot help making the guess, but I know it is a leap in the dark. Does Mr. Bradley mean to say that it is not only not proved that other minds than mine exist, but is impossible? "But if the being," he says (p. 142), "exists outside of all knowledge, assuredly *to us* it can be nothing." To us assuredly, but not to itself? "If it knows itself as what it is, then, since it falls within itself, it so far is the universe, and certainly is not [*so far ?*] one being among others" (pp. 142–3). If Mr. Bradley would add the words I have inserted in brackets, one might admit this, but where would his argument be?

A. L. Hodder.

THE DISCUSSION BETWEEN SPENCER AND WEISMANN.

The Inadequacy of 'Natural Selection' : Professor Weismann's Theories : A Rejoinder to Professor Weismann. HERBERT SPENCER. Contemporary Review, Feb., Mar., May, and Dec. 1893.

The All-Sufficiency of Natural Selection. A. WEISMANN. Contemporary Review, Aug. and Sept. 1893.

The history of biology during this century will be known in the next as marked by three great discussions relating to evolution. First, that between Cuvier and St. Hilaire, culminating in the famous debate in the French Academy in 1830, touching the methods of thought which were then leading to the discovery of the law. Second, that of 1858, caused by the revival of the law itself by Darwin and Wallace, and led by Huxley in England and Haeckel in Germany. Third, the present discussion which began in 1883, when Weismann first challenged the principle of Inheritance of Acquired Characters as a factor of evolution. This discussion probably culminates in the recent papers under review.

The supreme confidence of both parties is expressed in the titles of these papers—*The Inadequacy of 'Natural Selection,'* as Spencer's firm conclusion, is faced by *The All-Sufficiency of Natural Selection*, as Weismann's. The last word on each side is an unwavering confidence in the position originally taken, and a total rejection of the arguments advanced by the opposite side. Spencer's original article seemed so strong that his supporters in England imagined it would be conclusive, yet Weismann certainly gives an equally strong reply, bringing forward as a new and apparently irrefutable argument the case of the evolution of the neuter insects, in which most marvellous adaptations appear, for which the Lamarckian explanation cannot be advanced, because these insects leave no descendants. Yet in Spencer's rejoinder this argument is met in the most clever manner, by the assumption that these adaptations in sexless forms were established in the earlier social states of ants, bees, and wasps before they became sexless and while acquired characters could still be transmitted. In short, this discussion leaves every reader exactly where it finds him, because the honors in logic are evenly divided. Both sides are strong in attack and weak in defence.

While inconclusive it is most stimulating and has attracted wide attention, because the question bears with equal force upon problems of ethics and psychology as upon all lines of biological thought which run in the direction of the life-histories of organisms lower than man.

To no one is the issue so vital as to these two famous disputants. With Spencer we may say his whole system is at stake, and the permanence of the philosophy to which he has given his life. Weismann has a sudden notoriety to lose and a permanent reputation to gain, because if he is in the right, as the first opponent of the Lamarckian principle of inheritance, and, with Wallace, the champion of the exclusive selection theory of evolution, he will go down to posterity as the greatest biological thinker of this century after Darwin. Now in the issue itself there is no half-way ground ; the final result must be the total rout of one side or the other, and under such stress we should find in this discussion the strongest presentation possible of the two positions. This we do not find. It is the purpose of this review, therefore, not to follow arguments, which are so readily accessible and intelligible to all readers, but to show that the position of both Weismann and Spencer is defective—because neither has brought forward inductive evidence.

In fact, the reason these papers, interesting and able as they are, leave no final verdict in the mind is that neither meets the tests of scientific truth. When we look beneath the surface and recover from the first blinding effects of the brilliant style which characterizes both attack and reply, we see that both set forth mainly the modes in which nature may be supposed to act, rather than the mode in which nature does act. Nature, if anything, is illogical in many of her forms.

Weismann's strength lies in his exposure of the weakness of Spencer's position in relation to heredity. His weakness lies in the purely theoretical nature of the support of his own. He brings this charge against himself in the striking passage :

> "What is it then that nevertheless makes us believe in this progress as actual, and leads us to ascribe such extraordinary importance to it? *Nothing but the power of logic;* * we must assume natural selection to be the principle of explanation of the metamorphoses, because all other apparent principles of explanation fail us, and it is inconceivable that there could be yet another capable of explaining the adaptations of organisms, *without assuming the help of a principle of design.* In other words, *it is the only conceivable natural explanation of organisms regarded as adaptations to conditions.*"

Have thirty-five years of research under the stimulus of the selection hypothesis failed to bring forth a single fact or group of facts which Weismann, as one of the best informed of all living naturalists, can cite? He could have made no stronger array of evidence against his cause than his dependence throughout these essays upon theoretical considerations in support of his position that the selection principle *acts upon any and every minute shade of character in the organism.*

* Italics are our own.

In the absence of fact, he presents the group of speculations which have grown up in the Neo-Darwinian school, namely : the refinement and apotheosis of selection ; the utility or adaptiveness of, and intense premium upon, every variation ; selection as a sustaining power ; the cessation of selection or panmixia, and finally the reversal of selection :—all processes spun out of the human mind, without an iota of direct evidence in their favor.

This absence of evidence drives us to the conclusion that, so far as inductive proof is concerned, natural selection now stands, not as Darwin originally proposed it (in the extreme form revived by Weismann), but as Darwin left it in his later judgment, namely, as the arbiter of fitness in groups, species, varieties, individuals, and in single characters when they are of sufficient importance to weigh in the scale of survival.

When, however, we turn to Spencer as the champion of Lamarckism, we find him open to precisely the same criticism : he discusses probabilities rather than facts. This is partly because in the very broad field of science and philosophy which he has attempted to cover—so broad that in the present advanced state of knowledge one must sacrifice depth to breadth in the attempt—he has not kept pace with the progress of biology. His standpoint is exactly similar to that of his *Principles of Biology* published many years ago, and of his more recent essay *Factors of Organic Evolution.* This pardonable unfamiliarity with recent work is the apparent cause of his failure to give his attack upon Weismann the inductive basis, which was within his reach ; but more than this it has led him to a statement of his position which is absolutely fatal to his whole case and which Weismann has surprisingly overlooked. It occurs in the conclusion of his first article :

> "See, then, how the case stands. Natural selection, or survival of the fittest, is almost exclusively operative throughout the vegetal world and throughout the lower animal world, characterized by relative passivity. But with the ascent to higher types of animals, its effects are in increasing degrees involved with those produced by inheritance of acquired characters ; until, in animals of complex structures, inheritance of acquired characters becomes an important, if not the chief, cause of evolution."

Now the reverse of this principle is established by the researches of recent embryology ; for, as the physical and mechanical basis of inheritance has now proved to be the same from the lowest to the highest organisms, the transmission of acquired characters must either extend from the lowest to the highest, or, as Weismann believes, it must diminish from the lowest to the highest. When Spencer says that this transmission increases towards the highest organisms he is not only wrong in his facts but he cedes the case to Weismann, for adaptations in plants and in lower organisms are precisely the same in kind as those in the higher and the law of heredity must be the same.

The weakness in Spencer's attack on evolution by selection exclusively is further seen in his failure to take advantage of the evidence afforded by palæontology and by human anatomy that the fundamental postulate of the selectionists that adaptive structures arise out of the fortuitous play of adaptive and non-adaptive variations is negatived by direct evidence to the contrary. Palæontology shows conclusively that there is an adaptive trend in variation under the operation of some law; whether this is the Lamarckian law or some unknown law remains to be determined.

This discussion, at least in its theoretical phase, has reached its climax in this controversy. It must now enter the new phase of test by induction. Theoretically the subject is talked out, and Spencer and Weismann render their chief service in showing that ingenuity and logic on the two sides can be so evenly arrayed against each other. It is evident to the few biologists who have been able to keep cool in this heated period that we must now suspend speculation and turn to the more exact methods of science. The reaction towards the inductive tests of the problem is already well under way.

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HYSTERIA, ALTERNATING PERSONALITY, PARAMNESIA, THOUGHT-TRANSFERENCE.

Histoire d'une Idée Fixe. PIERRE JANET. Revue Philosophique, XXXVII. 121-168. Feb. 1894.

In this article Janet gives another of his monographic studies of the 'hysteric' condition. The patient was a woman of forty, of psychopathic heredity, who from early childhood had had fears and spasmodic attacks, and who, at the age of 17, in consequence of having assisted at the 'laying out' of two cholera patients, was seized with a haunting fear of this disease, which in three years made of her a wreck. Her worst feature consisted in frequent convulsive attacks of dread of the cholera, with vomiting, purging, and hallucinations of every sense. This condition had already lasted twenty years when Janet first saw the patient. The treatment consisted first in altering, by suggestions made during the attacks, now one and then another element of the terrifying hallucinations, until their whole character was changed. This cured the attacks; but the original state of waking *choleraphobia* returned.

M. Janet found that the nucleus of this emotion was the insistent presence of the *word* 'cholera' on the lips, as a phenomenon of motor automatism. He soon transformed the obsessive word as he had transformed the more complete hallucinations of the attack, by substituting other syllables, making chocolate, coqueluche, cocoriko, etc. The chronic state of fear departed with the dreadful word; but all sorts of other morbid ideas and impulses then showed themselves, partly due to accidental suggestions, and each easily cured by counter-suggestion, but each presently succeeded by another, so that the woman's condition remained on the whole deplorably abnormal. From this state of suggestibility, aboulia, and scatter-wittedness he rescued her by a year of pedagogic training, gymnastic and intellectual. His patience must have been great, for the whole narrative of treatment covers three years.—The article is full of acute psychology, e.g., the remarks on the nature of an 'idea' on p. 126, and on the definition of 'suggestion' on p. 141, and is of course most instructive practically.

Duplex Personality. R. OSGOOD MASON. Journal of Nervous and Mental Disease, XVIII. 593–598. 1893.

The case has been ten years under Dr. Mason's observation, the primary personality being a young neurasthenic woman, thoughtful, dignified, and exhausted by much suffering. The second personality, which first supervened spontaneously after a syncope when she was 24 years old, was that of a sprightly child with a limited and quaint vocabulary, free from pain, able to take food, and fairly strong. Its visits came at intervals through several years, and lasted from a few hours to several days. During these years the patient, whose health had improved, got married. Later 'Twooey' announced one day that she never should return, but that another visitor would take her place. Accordingly, after a syncope of several hours, a third personality calling itself 'the Boy' supervened, serious and more like the primary self in character. The second and third selves had less acquired knowledge than the first one. Neither of the two former seems to have had memory or other direct knowledge of the other. But 'the Boy' seems to be directly aware of both the preceding selves [?]. They all know about each other by hearsay, however, and 'Twooey' and 'the Boy' have been very anxious to make 'Number One' well. 'The Boy' has sometimes lasted for weeks. 'Number One' seems to have had phenomena resembling clairvoyance; 'Twooey' to have shown 'sagacity amounting almost to prevision'; 'the Boy' to have exhibited 'peculiar perceptive powers' (reading what people said by watching their lips, when stone-deaf).—The case would seem to deserve a fuller account than the rather provokingly incomplete one which the writer gives.

La reconnaissance des phénomènes nouveaux. B. BOURDON. Rev. Philosophique, XXXVI. 629–631. Dec. 1893.

M. Bourdon pronounces series of letters or of words before a subject who is required to say when he recognizes one that has already been pronounced. Errors are not infrequent: a word will be recognized as recurring which only *resembles* a previous word, and sometimes it will even be so recognized when no word that resembles it has gone before. The author concludes that the mysterious sense of 'having had the same experience before' is an error of identification. The present is judged the same with some more or less resembling real past, whose points of difference are overlooked, and the telepathic hypothesis of Lalande (see report *supra*, p. 94) is unnecessary.

A propos de la paramnésie. J. LE LORRAIN. *Ibid.* XXXVII. 208–210. Feb. 1894.

M. Le Lorrain maintains the same conclusion. The mysterious sense of having previously experienced the same is a real recollection, either of some merely similar experience, or of an identical experience unnoticed at the time but registered subconsciously. The resurrection of the thitherto subconscious record gives (apparently) the emotion of mystery to the phenomenon.

The Annales des Sciences Psychiques for Sept.–Oct. 1893 contains three articles on thought-transference.

Dr. Dariex, being in another part of Paris from a cancer-patient whose sufferings he was relieving by hypnotism, mentally willed that she should sleep. Three times running she fell asleep at the time when he was exerting the volition. The fourth time the effect was less complete; the fifth time there was no effect, but the conditions were here different, and the author inclines to admit that the first four experiments were probably cases of cause and effect.

Dr. Tolosa-Latour, who was treating a hysterical patient by hypnotism, relates how, one day, whilst in a railway-carriage in France, she being at Madrid, he willed that she should have a convulsive attack [!!!], which she had with great severity at about the same hour, such attacks being at that time rare in her experience.

Professor Tamburini reviews Gurney's 'Phantasms of the Living' at considerable length, concluding that the subject of veridical hallucinations merits the most careful study. The 'telepathic' hypothesis of their production supposes that a stimulus starting from the distant agent's mind impresses the mind of the seer, not necessarily with the content of the agent's consciousness, as in thought-transference, but often with another idea altogether. [Thus agent, whilst dying, has his mind filled with images of seer's person, but seer has a vision of agent's person, not of his own.] T. seeks to diminish this paradox by recalling those illusions in which the object perceived is unlike the stimulus, as when a noise made by birds will be heard as human speech. The most original part of the article is a suggestion that if the word telepathy do denote a real process in nature, it may be a process obtaining between different portions of one and the same brain, and that thus the train of our ideas may be partly determined by a sort of factor of which no account has yet been taken.—The author includes or appends eight unpublished cases of veridical hallucination, dream, or impression, one of them relating to a shipwreck, the seven others to as many deaths.

W. J.

Les phénomènes de synopsie (*audition colorée*). CH. FLOURNOY. Paris, Alcan, 1893. pp. 259.

Under this title M. Flournoy gives the principal results of an investigation begun in February, 1892, by M. Claparède and himself. The data collected are gathered up in a theory of colored audition (*audition colorée*), which deserves careful examination.

I. As is always the case, the greater part of the 2600 question-papers sent out were never returned; some 700 were filled out, half of which described phenomena growing out of colored audition. Even if we reckon all that were lost as negative, these figures give one case of colored audition to every seven persons. Bleuler and Lehmann had found before one case in eight. It is probable that further research will show an even greater frequency of this phenomenon, formerly considered more exceptional. It would be desirable to find the ratio for certain classes in society. Flournoy attempted this for professors and students, but without success, as the public do not understand the importance of these investigations. *Per contra*, the author was able to verify several of the answers by direct questions and thus eliminate from them the uncertainties attached to information obtained by correspondence.

The material gathered together might be treated in two different ways: it might be simply tabulated, and the theory afterwards erected upon it; or the theory might be presented at once and verified by mixing in fragments of the observations. The writer has chosen the latter method; this has the effect of giving greater prominence to a theory; from other points of view one might have preferred to see the data thrown more in relief, and to have each observation preserve its own characteristics without being cut off from the rest, or given piece-meal. Observations in many respects identical might have been superposed, as it were, so as to form groups which would be 'composite portraits' of various types of colored audition. This would not have overburdened the book, and each individual subject would have retained his physiognomy, his character, his type of mental imagery, which the reader would have been glad to examine in order. The author had all the data necessary for this, and readers will regret being deprived of them.

II. But these are criticisms of details. Let us pass to the work itself. As investigations of colored audition have increased in number, its domain has grown larger; to-day it includes various phenomena bearing merely a faint analogy to it. Still the terminology has been but slightly modified, often by chance alone, whence a confusion which Flournoy seeks to remedy by adopting a more precise vocabulary. If the word *synæsthesia* (*synesthésie*, σὺν αἴσθησις) denote well enough any association of different sensations, then *synopsia* (*synopsie*) would include such as rest on a visual basis. Cases of synopsia are divided in turn into: *photisms,* when a color is associated with a sound; *schemata* and *diagrams* (*schèmes* et *diagrammes*), when the phenomenon takes the form of a spatial representation realized at once or developing itself in series; *personification*, where the figures, etc., are represented in the form of persons.

Each of these phenomena may occur in varying degrees of intensity, from a mere thought (e.g., the logical schematism) up to an image localized and almost hallucinatory. The three classes may thus be developed and compared by a common formula. To render this comparison even more exact, the author advises us to distinguish in each case of synopsia between the *inducer* and the *induced:* the former determines the appearance of the latter, which is in some way within its sphere of attraction—'in its aureole.' This seems to us of particular importance to those who are endeavoring to find an objective sign of colored audition in the rapidity of association-time: it is not immaterial whether we associate the inducer with the induced, or the induced with the inducer. Such is the nomenclature proposed; the only question is whether the study of colored audition is far enough advanced for terms to remain without variation. If so, we believe it will be adopted, for it is very systematic.

III. Thus we find first of all *photisms*, the most typical phenomena of colored audition. Flournoy received specimens of every variety of it: the coloring of vowels and diphthongs, of consonants, words, and musical notes, of numbers, days, and even odors and tastes. He observes, also, that certain subjects, who do not themselves 'color letters,' are annoyed at hearing such and such a color attributed to certain letters ; this is less than the real 'colored audition' and more than philistine indifference. Flournoy calls it *negative photism.* The study of this type may help us to understand the coloring process in *positive* photisms. The utmost irregularity prevails in the coloring of these. Only the rule formerly found by Beaunis and Binet throws a little light upon the question : 'Of the vowels *i* and *a*, one is red, black, or white' about 95 times in 100.

The author appears to be more fortunate in his attempt to formulate what he calls the *law of brightness*, which he states as follows : 1. *i* and *e*—bright in a majority of cases. 2. *u* and *ou*—dark in a majority of cases. 3. *a* and *o*—medium, fluctuating between bright and dark.

This law is not sufficient to account for the coloring of vowels, but it reaches the principal factor, viz., light. It permits Flournoy to form, in accordance with the results of the principal investigations, a table of brightness together with a table of coloring, including the results of Fechner, Bleuler, Lehmann, and Claparède.

If we knew the law of coloring for vowels, we should have the law of their diphthongs also, for the one regulates the other ; relying on this fact, the author is able to formulate these rules of coloring :

1. *Juxtaposition* of colors : *o* yellow, *a* black, *i* red, gives *oi* yellow and red.

2. *Optical mixing* of colors : *a* red, *i* blue, *e* white, gives *ai* violet.

3. *Adoption* of one of the component colors : *ai* white, from *i* red and *a* white.

Does the knowledge of the theory of complementary colors, etc., influence the manner in which the subject colors diphthongs ? We have few details on this point. As to the coloring of consonants and figures, their laws are still more difficult to formulate than those of vowel-coloring.

Between photisms and schemata lies a distance such as separates a luminous sensation from a colorless representation. Flournoy distinguishes two kinds of schemata : the *diagram*, which unfolds itself in space, interpreting the sensations of changes experienced successively on reviewing a series ; and the *symbol*, which interprets rather the entire impression of a single thing. Of these two the *diagram* is the more important and the more varied. The author has given several figures of numerical, weekly, annual, chronological, etc., *diagrams.* As in the case of photisms first in the series we find localized and almost objective *diagrams*, which impose themselves upon the subject ; and farthest removed from them, on the other hand, are the *logical diagrams*, plastic, modified by the possessor in conformity to his memory, or for fixing dates, abstract ideas, the figures in a calculation, etc. These are real mnemonic aids.* In a curious chapter full of ingenious views the author shows us next how these *diagrams* usually appear in childhood, develop until maturity, and sometimes disappear in old age. Finally the carefully-verified study of a family nearly all the members of which had such *diagrams* reveals the importance of heredity in these matters. This is unfortunately very meagre, and the author wishes it could have been more thorough. The *personifications* form the subject-matter of the final chapter—quite brief, since this phenomenon touches colored audition only at certain points.

IV. It remains to speak of the theory of these phenomena ; it runs through the entire work, but is found especially at the beginning. It is well known that colored audition is dependent upon none of the 'classic' laws of association ; still it occurs too frequently not to obey some law. Flournoy proposes three, but they are of unequal importance. There is first a *law of affective association*, which unites two sensations on account of analogy in their emotional character, even when their objective contents have nothing in common. They affect the organism similarly, and that is sufficient to unite them. Next comes the *law of habitual association*, binding together two heterogeneous sensations which habitually occur together. Lastly, the *law of opportune association* (*association privilégiée*) takes account of a host of connections which gain a footing in our mind at their first occurrence, because they come in at an opportune time.

We may pass rapidly over the second law, to which the author attaches little importance, and which derives its results from other laws,—for the frequent coming together of two sensations is not a matter of chance, but has a cause. This cause, when it is not one of the 'classic' laws, would be sometimes their identical emotional character (law of affective association), sometimes certain affinities which are still obscure (law of opportune association). To these obscure affinities Flournoy allows much less importance than to analogies of emotional character ; yet they seem to cover an equal number of facts. In these affinities, sprung from the depths of our nature, where the past prepares them and calls them up, will be found the explanation of the artists' colored auditions. From this point of view they deserve to have indicated their important rôle in the psychical moulding of the child, as the author has done in a very happy, and we believe very

* It is rather curious to note that even the blind can make use of them. This fact is not the least striking of the surprises in an investigation on the *audition colorée* of the blind, the results of which the present reviewer hopes to publish shortly.

exact, manner. Still the chief principle in these associations is what Flournoy calls the identity of the affective coefficients. It is perfectly conceivable that two sensations absolutely heterogeneous and not admitting comparison of objective content (e.g., the color red and the sound *i*) may nevertheless admit of comparison and more or less resemble each other in their organic effects. This emotional factor which accompanies them becomes the bond of union between them. This is a very important point, and very well brought out by the author. Doubtless the proposed law is not yet so clear as future research will make it; it is still vague and quite general, for the data on colored audition are still insufficient. It shows indeed that bright colors in general unite with sounds of high pitch; but it does not show what color a given sound will choose. It also leaves untouched the question concerning the ultimate substratum of these associations. Are they of psychological or physiological origin? Most probably sometimes one, sometimes the other, according to the circumstances which give them birth. Nothing here is really established yet, as Flournoy warns us; the same must be said concerning the centre of such associations, the search for which is especially tempting to the psycho-physiologist, since he is here in the presence of a phenomenon which is the more conspicuous because it is not universal.

Flournoy merely notices these last two questions; he has, however, done his share to render their solution easier. His study, well furnished as it is with data and cleverly written, has the double merit of being at once pleasant reading and the work of an earnest psychologist.*

J. PHILIPPE.

SORBONNE, PARIS.

VISION.

Studies of the Phenomena of Simultaneous Contrast-color; and on a Photometer for measuring the Intensities of Lights of Different Colors. A. M. MAYER. Am. Jour. of Science, XLVI. 1–22. 1893.

Prof. Mayer obtains the colored shadow effect by means of a screen composed of an inner disk of thick white cardboard and an outer disk of translucent white paper, which is illuminated on one side by lamplight (to which different colors may be given by colored glass) and on the other by daylight. By this arrangement, the contrast-colors obtained are so saturated as to cause astonishment even in those who are familiar with similar experiments. The colors were matched upon the color-wheel, being brought close to it by mirrors, and it was found that the petroleum flame of a Belgian burner was matched by a mixture of

* Translated by H. C. Warren, Instructor in Psychology, Princeton.

chrome yellow and red lead with twenty per cent of white, and its contrast-blue by Prussian blue and a little green with thirty per cent of white. (The contrast-color was therefore, roughly speaking, quite as saturated as the actual orange-yellow.) The matched colors thus obtained were then combined upon the color-wheel, and found to be, sufficiently nearly, complementary. To show that the original colors are complementary, the following experiment was made. A grating composed of alternate strips of cardboard and of translucent white paper of equal width was illuminated on one side by lamplight and on the other by daylight, and then looked at through a calc-spar prism, which was rotated "till the blue bands of the grating are superposed on the orange bands, when, if the surface of the grating is equally spaced, the superposed surfaces appear white when compared with the white of the screen, W. Without the screen, the eye has no term of comparison and may take a yellowish white for white." But this experiment seems open to objection. The contrast-blue is not a color that has any objective existence—it is only produced upon the retina as a side effect of yellow; it cannot therefore be moved about by a calc-spar prism. What happens objectively, before any image has reached the eye, is that the yellow strips are exactly superposed upon the white strips; an even surface of yellowish white is therefore what throws its image upon the retina. There is no possibility of the production of any other color-sensation,—neither a real (strong) yellow nor a contrast-blue,—unless in the two end bands, which are seen singly. That the intervening portion looked purely white must therefore have been due to some other cause,—possibly to some judgment-illusion produced by the yellow and blue (?) end bands. Experiments were made to determine the time required for the simultaneous contrast-colors to be produced. The electric flash of a Holtz machine was seen reflected from the first and from the second surface of a piece of green glass; the latter image was green (being formed by light which had gone twice through the thickness of the glass), and the former showed a contrast-red. The interval between the flash and the perception of the colors was less than $\frac{1}{15}$ of a second; "on viewing the flash and the illuminated surfaces [of a similar experiment] at the same time, *no interval could be detected* by this mode of observation as existing between the instant of the flash and the perception of the colors, and we certainly could have detected a shorter interval than $\frac{1}{15}$ of a second had it existed." This interval of time is evidently too short for the observer to exercise his judgment and divide between the two images a difference of color which is in reality only a departure from white on the part of one of them, as the psychical explanation of the phenomenon would require him to do. There can therefore be no doubt (as has been shown before in many other ways) that what is called simultaneous contrast is a sensation due to an actual physiological process in the visual substance, and not to an error of judgment. This is a question of fact; Helmholtz has believed that it is an affair of the judgment, and Hering has believed that it is not, and it now appears that Hering is in the right. But fairness requires one to point out that Helmholtz' theory of color-sensation does not necessarily stand nor fall with his explanation of contrast; though Helmholtz himself, of course, does not yet find it necessary to take refuge in this fact, and it is quite possible that he would consider his judgment-illusions an integral part of his theory. Prof. Mayer says: "According to Hering's hypothesis of color-sensations, when a portion of the retina is stimulated, adjoining portions of the field of view are affected by a sort of inductive action; so that changes are produced which are antagonistic or complementary to those portions of the retina actually stimulated." But it is open to Helmholtz, if once he were convinced of the fact, to say the same thing, in the very same words. The changes in question can only be produced through the intervention of reflex nervous action, on Hering's theory; and by that means one process can be produced in the adjoining portion of the retina as well as another. If on any theory the sensation of yellow were due to a vibration of nerve-fibres to the east, then an induced sensation of blue could be accounted for as a vibration of nerve-fibres to the west; and this would be no harder for reflex nervous action to accomplish than a change in assimilation or in dissimilation. Hering has done much to establish the theory—it is almost better to say the fact—that contrast is of a physiological nature; and Prof. Mayer has here added important confirmation to his proof. But one should be on the guard against supposing that this is any confirmation of his theory as a whole. Prof. Mayer himself points out that the process induced in the adjoining portion of the retina may just as well be a complementary process as an antagonistic one.

BALTIMORE. C. L. FRANKLIN.

Theorie des Farbensehens. H. EBBINGHAUS. Zeitsch. f. Psychol., vol. v. 145–238. May 1893.

When Professor v. Helmholtz published the first edition of his *Physiologische Optik* in 1867, the theory of color-vision there developed represented the most advanced science. Now when the second edition is in course of publication the theory is antiquated. Nothing could bear more notable witness to the advance of science. The Young-Helmholtz theory of color-vision is pre-evolutionary and pre-psychological. The theory is pre-evolutionary because we must believe that in the animal series the discrimination of light and darkness preceded the perception of color. It is absurd to require three kinds of fibres and the like of the protozoa. The theory is pre-psychological partly because the special facts discovered or elaborated since 1867 cannot be adjusted to it, partly because the assumption that sensations of red and green or yellow and blue may be judged white is not now admissible. Professor Hering in 1874 saw the failure of the Young-Helmholtz theory in these two directions, and in a somewhat fragmentary fashion has developed one much more adequate. But the further progress of physiology and psychology has made the assumptions of Hering unlikely, and the newer facts do not fit naturally into his theory.

New theories have naturally resulted, and one advocated by the authority and ability of Professor Ebbinghaus deserves especial attention. Ebbinghaus begins with a searching criticism of the theories of v. Helmholtz and Hering. Against the Young-Helmholtz theory he points out especially the destructive testimony of the colorless spectrum of the completely color-blind and of the normal eye with faint illumination. He also notices the failure of the more exact color-equations of König and Dieterici to coincide with the requirements of the theory. Against the theory of Hering he adduces experiments of his own, showing that the intensities of grays of different composition do not remain the same when the illumination is altered. Mrs. Franklin has also made this observation with pigments, and indeed it must be evident to any one who has made color-equations. The writer has known for many years that when an equation has been made for a given illumination both intensity and color become different when the illumination is altered, and this would indeed seem to be a necessary consequence of the Purkinje phenomenon described in 1825. Ebbinghaus also points out the difficulty in the way of assuming antagonistic processes of assimilation and dissimilation, and the failure of Hering's theory to account for the types of color-blindness.

Ebbinghaus then proceeds to develop his own hypothesis. He identifies the visual purple or rod-pigment (Franklin's preferable name) of the retina with the 'blue-yellow' substance of Hering, and assumes another pigment ('red-green') complementary in objective color and present only in the cones—whence their lack of color. A third 'white' substance, present throughout the retina, is not objectively visible. The dissimilation of the last-mentioned substance gives the white and gray sensations. These also accompany dissimilation of the other two substances, but such dissimilation is in addition accompanied by sensations of color. In the case of the 'blue-yellow' substance (the rod-pigment), for example, the first part of the process of dissimilation gives rise to the sensation of yellow and its final destruction to blue.

Ebbinghaus thus follows Donders (whose theory he does not mention) in substituting partial decomposition of the visual substances for the antagonistic processes of dissimilation and assimilation assumed by Hering. The supposition that sensations accompany processes of assimilation is certainly unlikely; at the same time it must be allowed that this assumption does account for the neutralization of complementary colors, whereas Ebbinghaus simply states that 'sie haben etwas Antagonistisches und stören sich gegenseitig.' His theory like that of v. Helmholtz exactly fails to explain the fact which makes a theory necessary. It is certainly an advantage to identify the hypothetical visual substance with a real physiological substance, but it seems that the alterations in sensation scarcely correspond with the known reactions of the rod-pigment. The correspondence of the objective colors of the rod-pigment with the complementary colors in sensation is not exact, and is more specious than convincing. Ebbinghaus' theory fits better than Hering's with the facts of intensity and color-blindness, but when Hering is convinced of the facts he can readily adjust his theory.

This short notice cannot do justice to the careful working out of the details of criticism and construction which Ebbinghaus accomplishes with German thoroughness and German 'leisureliness.' The writer must, however, admit that he finds the theory less satisfactory than those of Wundt, Donders, and Franklin. It should be remembered that in all cases assumptions are made which must be verified by physiological research. All recent theories are, in spite of their names, not theories of color-vision, but hypotheses concerning the physiological processes which precede vision. So long as we are ignorant of these actual physiological processes the writer finds most satisfaction in assuming that the continuity of physical vibration is transmitted through the retina to the brain, where inertia, summation, and inhibition intervene to produce the changes which are correlated with consciousness.

J. McK. C.

THE SENSE OF TOUCH.

An Experimental Study of Simultaneous Stimulations of the Sense of Touch. WILLIAM O. KROHN. Jour. of Nervous and Mental Disease. March 1893.

The writer, in connection with Mr. Bolton of Clark University, performed a series of experiments "to determine the relative sensitiveness of different portions of the skin, to find the nature and direction of the errors in localization, and to study the influence of attention upon the localization and interpretation of the simultaneous touch stimulations." An investigation of the interesting problem of attention was also made, with especial reference to the question of how many sensations of touch the mind can attend to or grasp at one time. The effect of practice was also carefully examined. The touch stimuli were applied by means of small cork points attached to adjustable tambours, which were connected with a common air-chamber by means of separate pipes. Pressure brought to bear upon the confined air in the chamber caused the cork tips to touch the skin at various parts of the body synchronously, or practically so. Altogether about 2500 tests were made, perhaps the larger number upon students at the University of Illinois.

The following conclusions are substantiated as a result of these experiments. It was shown: (1) that the skin over the joints is more sensitive than elsewhere, permitting greater accuracy of localization; (2) that touches on the back are more distinctly felt, more clearly remembered, and therefore better localized than touches on the front of the body; (3) that on the left side touches are not so well localized as on the right side; (4) that localizations are more correct when the touches occur at points removed from the median line—touches on the median line being very poorly located; (5) that exposed surfaces localize better than portions usually covered with clothing; (6) that piliferous parts are more sensitive; (7) that errors in localization follow certain fixed rules; (8) that the influence of attention is very marked; (9) that the effect of practice is plainly shown; (10) that two pressure stimulations are often fused into *one single sensation*, localized at a point removed from either of those at which the stimulations were received; (11) that there is a strong tendency to perceive dermal sensations of purely subjective origin; (12) that bilateral asymmetry of function is plainly evident in dermal sensations. These experiments have opened a new and interesting field.

AUTHOR'S ABSTRACT.

EXPERIMENTAL.

An Experimental Study of Some of the Conditions of Mental Activity. JOHN A. BERGSTRÖM. Am. Jour. Psychology, VI. 247–274. Jan. 1894.

Mr. Bergström's study is excellent reading for those who credit experimental psychologists with an overweening desire 'to make the facts agree with their theories,' since the thoroughness of the investigation is no more marked than the modest conservatism of the conclusions. The problem was the discovery of the 'natural rhythm,' if there be one, of mental processes. The method consisted in the accurate timing of definite intellectual operations at different periods, about two hours apart, throughout the day and evening. The results, which are given in tabulated and in graphic form, include experiments on seven subjects of whom five were connected with the psychological department of Clark University: in all cases the daily routine was similar. The tested process, except in the case of one subject, consisted, in the first experiments, in sorting a pack of eighty cards, of eight distinct sorts, into ten piles. The experiments were continued through 17, 11, 11, 10, 8, and 5 days respectively, for different subjects; they certainly show the existence of a 'natural rhythm' of mental activity, but the periodicity is individual, not general. The most constant factor is the depression, shown by all the records except one, from morning to night. In the case of two subjects, Mr. Bergström compares the relative variation of different processes: reading, adding and multiplying numbers, and learning nonsense-syllables. The experiments show very slight changes, at different hours, for the simpler processes, and very striking variations for the more complex. Thus the mean averages of each test are 1.02, 1.55, 2.37, and 7.95 seconds respectively for the operations just named. A comparison of the 6 P.M. records after 'exciting physical exercise' with those taken at 4 P.M. shows, in general, a relative stimulation of the simpler processes but, in most cases, a depression of the more difficult: in the records taken after walking, the difference is seldom observed. An observation of the rapidity of the pulse-rate, in connection with the different records, shows in one case a change from 71.3 to 53, while there was no change in the rate of the mental processes, so that 'mental activity can evidently not be said to vary with the pulse-rate.'

In the second part of his paper Mr. Bergström tabulates and slightly enlarges the results of his earlier study (*Am. Jour. Psych.*, v. 3), and indicates the evident influence of the interference of associations, as shown in the averages of Ebbinghaus, for his eight memory-series, 105, 140, 142, 146, 146, 148, 144, 140 seconds. 'The great increase of the second above the first and the slight difference afterwards' accords exactly with Mr. Bergström's results, and is explained through the facts that the first series is always best learned, and that the effect of practice later counterbalances that of interference. In conclusion Mr. Bergström considers cases of 'surprising retardation' in thought, such as persisting errors, and emphasizes the prominence of interference as an influence which hinders mental processes, and the significance of the inevitable decrease of interference as a negative explanation of the reassertion of the mental energy. The theory is not put forth as a complete one, but it is certainly far more credible than the usual hypotheses which attribute the recovery of the correct associations 'to unconscious cerebration, to summation of stimuli, or to rest.'

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

MEMORY, IMAGINATION.

1. *La Mémoire des Joueurs d'Échecs qui jouent sans voir.* A. BINET.
2. *La Psychologie des Auteurs Dramatiques.* A. BINET et JACQUES PASSY. Revue Philosophique, Feb. 1894, 222–240.

1. M. Binet, in announcing a forthcoming work on cases of extraordinary memory, reports as a specimen and as typical of the class to which they belong, the experiences of Dr. Tarrasch. T. plays from six to eight games at a time without sight of the board, and he here tells how he does it. His main reliance is on a very vivid imagination of the board and of the arrangement of the men. The board, which is persistently present during the play, is visualized in a small diagram of about 8 cm square, with the squares light and dark ; the men are visualized with something of color and form, but with no regard whatever to material. Next in importance is the power to recall the progress of the game, and here the memory is greatly aided in the case of a good game by the logical order of events. A third factor is the memory of words, e.g. of the judgments interiorly formulated at different points concerning the character of the play. The main thing in playing several games at once is to keep the games distinct. When the opponent's move for a particular game is first announced, it is often found necessary to recall deliberately how that game opened and to trace its history in detail to the point where the stated move becomes significant ; but as the games develop they get more clearly differentiated and such detailed reproduction becomes unnecessary. T. seems to have a good but nowise remarkable general memory ; in mental arithmetic he reports himself as distinctly deficient.

2. The second article, designated as a study of the conditions of creative imagination, deals principally with the experiences of a comparatively new author, M. de Curel. M. de Curel's method is to invent a situation suggesting a problem, and then to proceed at once to composition. The problem is not worked out logically, but the play grows by a sort of 'crystallization': the original situation becomes very

likely changed in character and dramatic value, and the psychology only appears as an 'extract' from the facts. The author writes rapidly, the average time for a play being about three weeks. While composing, he hears his personages rather than sees them, having an especially vivid impression of the inflections of their voices as revelations of their character ; the scenes are those of real life, not those of representation on the stage. The most interesting feature of the author's state of mind while composing . its pronounced duality (*dédoublement*), approaching the morbid type. At first he is distinctly conscious of inventing his personages, and is interested in them and their fortunes ; later he becomes personally indifferent to their fortunes, the characters are formed as it were within him, and express *themselves*, so far as their sentiments are concerned, while he, the author, maintains voluntary control of all the rest. As he himself puts it, he is for the time being 'completely in their skin,' his pen is, in a manner, 'moved' by them, and even wl en writing is interrupted by a visit or conversation, the work of composition still goes on, 'because the interior personage continues his office.' The writers of the article find that M. de Curel alone of all the dramatists they have consulted exhibits this duality of consciousness in this form. They designate it as *dédoublement réel* as distinguished from *dédoublement littéraire* (where the author tries to obtain himself the sentiments of his characters in order to put them, with conscious art, into their mouths), and are surprised to find this mental 'disaggregation,' which French psychologists have hitherto regarded as a symptom of weakness, in an author of such power. The investigation is worth continuing. The present writer knows of at least one novelist with whom something of the same sort happens, and is inclined to suspect that, allowing for some exaggeration in description, the phenomenon is not so altogether uncommon. Even M. de Curel is not completely possessed by his imaginary personages, for he admits that sometimes he mingles his sentiments with theirs and is obliged later to make the necessary corrections.

H. N. Gardiner.

Smith College.

RHYTHM.

Rhythm. Thaddeus L. Bolton. American Journal of Psychology, vi. 145–238. Jan. 1894.

About one third of this paper is devoted to a review of the rhythmic processes discernible in nature both inorganic and organic, and of their artistic application in music and poetry, with special reference to

English verse of which several philosophies are cited. In the experimental investigation which follows, an attempt is made "to reduce rhythm to a more fundamental activity of mind." This task involves the solution of various problems, the first of which is to determine what the mind does with a series of simple auditory impressions absolutely uniform in intensity, pitch, quality, and time-interval. Such sounds are obtained from the telephone connected in an induction circuit, provided the primary circuit be broken at regular intervals when the secondary is closed, the secondary being open when the primary is closed. The breaks were effected by five arms fixed on the drum-shaft of a Wundt chronograph and playing upon ten keys, which, when pressed down, closed first the primary and then the secondary circuit. By properly arranging the arms and using extra coils, a more intense click could be thrown in at will.

"The subject was requested to group the sounds, not by voluntary effort, but only so far as it was found easy and spontaneous." The records of thirty observers are given, most of whom had some musical talent and training. A detailed protocol shows the effect of the click-intervals upon the grouping, and the various images suggested by the sounds. These interesting accounts would perhaps be easier to compare if the same intervals had been used for all the subjects. Wundt's statement that it is impossible to restrain the grouping absolutely is disproved by these results and explained by the variations in the metronome-beats which Dietze employed. On both points the author agrees with Schumann, though no reference is made to the Schumann-Wundt discussion. The conclusion drawn from the observations is that rhythmical grouping results from a sequence of acts of attention, in each of which the auditory impressions are so subordinated as to form a unit in consciousness.

In determining the essential conditions for constituting this unity, the first step was to ascertain how many sounds might be grouped. One test was the ease and pleasure which the subject felt in grouping. It was found that rhythmical grouping takes place between a lower limit of 1.58 sec. and an upper limit of about 0.115 sec. In nearly all the subjects there was a tendency to group by fours, the average length of the 4-group being 1.228 sec. For the 8-group, which was the highest tested, the average length was 1.16 sec.

A second test was applied by giving, at different rates, first a series of uniform clicks, then a series in which every sixth click was accented, and finally one in which every eighth click was accented. The results agree closely with those obtained by the other test, and accentuation seems to make little or no difference. Comparison, however, of the tables is not entirely satisfactory, as the record is complete for one subject only.

The inherent nature of a rhythmical group was investigated by three methods. In the first place, the subject was asked how he grouped sounds of uniform intensity, and upon what sounds the accent, if any, was placed. The answers showed that in the 2-group the first sound was accented, and in the 3-group and 4-group the first and third sounds. In this case the first was stronger than the third. When a certain grouping was suggested, any other was found difficult or impossible.

By the second method, a regularly recurrent set of sounds of different intensities was given, in order to see where the mind would most naturally divide them into rhythmical groups. Experiment proved that as a rule the group must have either a strong sound at the beginning or a weak one at the end ; but certain arrangements of the sounds conflict with this principle.

The third method, employed with special reference to poetical rhythms, consisted in varying the length of the sounds. By rotating a notched disk between a tuning-fork and its resonator, regular interruptions were secured, the length of the sounds depending on the number of degrees covered by the notches. It was found that the longer sound, the most important element in the group, comes last, just as the most common foot in modern poetry is accented on the final syllable.

The muscular movements and the associations which most observers noticed are, with Ribot, regarded as the conditions, not as the results, of rhythmical grouping. The pleasurable feelings that accompany certain intervals are due to the fact that the length of the group corresponds to the normal wave of attention. If the rate be much faster or much slower than this normal, there is neither pleasure nor organic unity.

The general principle is thus stated : "The conception of a rhythm demands a perfectly regular sequence of impressions within the limits of about 1.0 sec. and 0.1 sec. A member of the sequence may contain one or more simple impressions. If there are a number of impressions, they may stand in any order of arrangement, or even in a state of confusion, but each member of the sequence must be exactly the same in the arrangement of its elements." E. A. PACE.

CATHOLIC UNIVERSITY.

LOGICAL.

Jugement et Ressemblance. V. EGGER. Rev. Philos., XVIII An., Nos. 8 et 9. 1893.

All judgments are reducible to statements of identity, not to concepts. The concept does not contain the duplication that the statement of identity involves. Accordingly the commonly accepted theory is wrong in regarding judgments as analytical in the sense of being analyses of concepts. The concept does not contain the duplicating and identifying copula. The concept is but A, for example; the judgment is $A - A$, or A is A, or A is B, or A is $b + c$, B and $b + c$ simply being the analyzed A as attribute of itself. Of course the process of the concept is in the judgment, as from being a purely formal identification it passes into a definition; but the concept does not afford an adequate account of the identification itself.

Is the principle of identity *a priori?* Not at all. Psychology does not need even to harbor the Platonic realism that metaphysics exiled long ago. The natural forces of the mind, its most ordinary processes, offer a perfectly satisfactory explanation.

In children judgments exist as unexpressed associations. Adults supply the implied *is*. The copula only names the association, the resemblance becoming object of a special consciousness. Judgments are stated associations of things similar.

Judgments are never associations of things contiguous. Association by contiguity is not reducible to association by similarity. Contiguity in its proper character is without the *is;* it belongs wholly within the terms that the *is* unites; it is not an object of judgment, although it enters into thought as material of judgment. If objection be made that language, when similarity is really meant, uses some word of comparison, saying, not *A is B*, but *A is as B*, the obvious reply is that *A is B* includes *A is as B*, as the abstract includes the concrete, by having outgrown it.

As against Mill, whose 'verbal' and 'real' propositions are considered at length, we are told that judgment does not affirm of two terms in relation their relation; it affirms one term of another. A judgment of which the attribute or the subject is a relation is artificial. Mill, with his interest in induction, was too much of an artist. For him as for Hamilton propositions were only so many words; their conversion was only a purely verbal operation. Not relation, not contiguity, but similarity is the real object of the natural judgment.

Judgment develops psychologically in these stages: (1) Subject and predicate both individual. *This is that.* But the *that* tends ever to become more general and abstract. Cf. the child's unexpressed association of two concrete, individual things. (2) Subject individual, or distributive, or collective, but always quantified; predicate, or attribute, abstract, not quantified. A substantive for subject, adjective for attribute. Cf. Aristotle's propositions; also the theory of Rémusat, who regarded subject and attribute irreducible. (3) Two substantives, second having place rather than rôle of attribute. Both subject and predicate quantified. Mathematical propositions, definitions. Cf. Hamilton. (4) Two attributes. Theoretically, neither term quantified. Cf. the propositions advocated by Stuart Mill, who tried to relegate quantification to the copula. Natural language does not recognize these propositions; they are contrary to the habits of the mind. Indeed, natural language recognizes judgment only as in the second and third stages.

The form in the third stage is really a return to the first. As to the distinction in consciousness between subject and attribute, a distinction that is sharpest in the second stage, the judgment, by bringing together the two extremes of thought, the subject as extensive with intension implicit and the predicate as intensive with extension implicit, only shows their psychological continuity. The judgment is language's way of representing the class, or concept, as *a collective idea becoming abstract*, language being able to express this becoming only through the use of the extremes. A. H. Lloyd.

University of Michigan.

PATHOLOGICAL.

Zur Theorie der cerebralen Schreib- und Lesestörungen. R. Sommer. Zeitsch. f. Psychol., v. 305–322. 1893.

Sommer observed a 60-year-old peasant, who a year and a half before had had an apoplectic attack with resulting paralysis of right arm and leg. Paralysis disappeared after two weeks. Loss of ability to recognize certain letters and words remained, other letters being permanently recognizable, and still others intermittently so. Sommer's chief conclusions from this case are these:

Ability to read cannot be explained as result of recognition of letters and ability to retain their sounds in memory. The combination of sound-series into words is a psychologically separate function, without, however, a separate brain-centre. Such combination depends possibly on the ability to think the sounds in succession so rapidly that they unite into a word. It occurs in case of partial letter-alexia that the letters which can be recognized cannot be written to dictation. On the other hand, in writing to dictation, characters can be produced which cannot be recognized in reading. Moreover, coupled with almost complete inability to write down letters to dictation occurs the ability to write certain connected words. This proves that the presence in consciousness of the sounds corresponding to the singly produced letters is not absolutely necessary. The writer takes occasion also to answer some criticisms of his recent 'Zur Psychologie der Sprache' (*Zeitsch. für Psych.* II. 143). E. B. DELABARRE.

BROWN UNIVERSITY.

NEW BOOKS.

Basal Concepts in Philosophy. A. T. ORMOND. New York, Scribners, 1894. Pp. viii, 308.

Practical Lessons in Psychology. WILLIAM O. KROHN. Chicago, The Werner Company, 1894. Pp. 402.

Geschichte der neueren deutschen Psychologie. I. Bd. Von Leibnitz bis Kant. MAX DESSOIR. Berlin, Duncker, 1894. M. 13.50.

Pleasure, Pain, and Æsthetics. HENRY RUTGERS MARSHALL. New York, Macmillan & Co., 1894. $3.

Art in Theory: an Introduction to the Study of Comparative Æsthetics. G. L. RAYMOND. New York, Putnams, 1894. Pp. xviii, 266.

Social Evolution. BENJAMIN KIDD. New York and London, Macmillan & Co., 1894. Pp. 348.

Some Applications of Logical and Psychological Principles to Grammar. PETER MAGNUS MAGNUSSON. Minneapolis, University of Minnesota, 1893. Pp. 125.

Les émules de Darwin. A. DE QUATREFAGES. Bibliothèque scient. internat., 2 vols. 1894.

Zur Analyse des Apperceptionsbegriffs. J. KODIS. Berlin, 1893.

L'action. Essai d'une critique de la vie et d'une science de la pratique. M. BLONDEL. Paris, Alcan, 1893. Pp. xxv, 495.

Les caractères. FR. PAULHAN. Paris, Alcan, 1894. Pp. xii, 274.

Hypnotismus und Suggestion. M. BENEDIKT. Wien, Breitenstein, 1894. M. 2.

NOTES.

Messrs. Macmillan & Co. have published *The Proceedings of the American Psychological Association* (29 pp., 25 cts.). This contains accounts of the Preliminary Meeting (1892) at Clark University, the First Annual Meeting (1892) at the University of Pennsylvania, and the Second Annual Meeting (1893) at Columbia College. The Proceedings include short abstracts of the papers read (twelve at the first meeting and fifteen at the second), many of which have not as yet been published elsewhere.

The University Press of Columbia College will publish a series of contributions to Philosophy, Psychology, and Education under the editorial supervision of the instructors in the department. These contributions will consist of dissertations and monographs too long or too technical for publication in existing reviews.

Messrs. D. C. Heath & Co. have published part of a Laboratory Course in Psychology by Dr. SANFORD, Assistant Professor of Psychology in Clark University. The part issued (pp. 183) gives instructions for experiments on the senses with full bibliographies.

M. BINET announces the appearance next year of a new *Année Psychologique*, which will contain "*résumés* of psychological literature appearing in all countries during the year." The subscription (price 5 fr.) may be sent in advance to him at the *Laboratoire de psychologie, Sorbonne, Paris.*

M. BINET is also on the point of issuing (in April) an *Introduction à la psychologie expérimentale.*

Professor LLOYD MORGAN is preparing for the Contemporary Science Series a work entitled 'An Introduction to Comparative Psychology,' some extracts from which are included in an article in the April number of *The Monist.*

Professor JODL of the University of Prague announces a *Lehrbuch der Psychologie* to appear in 1895.

In January the first number of a new monthly, *Rivista di pedagogia e scienze affine*, was published by G. B. Paravia e Comp., Rome. The review is edited by Professor SERGI with the co-operation of the leading Italian writers on pedagogy.

Professor SERGI has in press a volume on *Pain and Pleasure*, which is the first part of a systematic treatise on psychology.

Professor LIPPS of Bonn has accepted a call to the chair at Munich vacated by the removal of Professor Stumpf to Berlin.

Professor PAULSEN has been promoted to a full professorship at Berlin, and Dr. KÜLPE to an assistant professorship at Leipzig.

The present number of THE PSYCHOLOGICAL REVIEW is somewhat enlarged. Between six and seven hundred pages will be printed annually, but the prompt publication of articles and reviews can be best secured by varying somewhat the size of the separate numbers.

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THE PSYCHOLOGICAL REVIEW.

REVERSE ILLUSIONS OF ORIENTATION.*

BY M. ALFRED BINET,

École des Hautes-Études, Paris.

I use this phrase to designate a group of facts which have not yet been studied methodically, but which have been cited by many authors and described in different words. These facts consist of illusions or hallucinations of orientation, which arise spontaneously either when we waken in the darkness of night, or during the day when awake. These illusions do not appear to be experienced by a great number of persons, as is shown by the fact that nearly all the individuals whom I have questioned for a long time have not been able to discover in their past experience any instance of this 'reversal' of orientation.

The first indication which I find is in a letter of Henry Forde, published in *Nature* (Aug. 17, 1873) following a communication of Darwin published in the same journal (Apr. 3, 1873). The following is Forde's letter: "In the wild parts of the state of West Virginia even the most experienced hunters who frequent the woody mountains of this wild region are subject to a kind of shock, so that they suddenly lose their heads and feel that they are going in a direction directly contrary to that which they intended to follow. It is useless for their companions to reason with them and show them the position of the sun. Nothing can conquer this feeling, which is accompanied by a great nervousness and by a general sensation of distress and of uncertainty. The nervousness comes only after the seizure, and is not the cause of it."

* *Le Renversement de l'Orientation.*

These observations are also cited in an article of M. Viguier,* who has added no personal contributions. I myself have published some observations upon this point in *Mind* (1884), in an article entitled *Vertigo of Direction*. This title is very inexact since there is never produced, at least to my knowledge, an illusion of a movement of objects, or a true feeling of vertigo. Again, M. Flournoy, in his recent work *Les Synopsies* (p. 188), speaks incidentally of these phenomena, which he has observed in himself at the moment of waking. "Who has not happened," says he, "to waken in the darkness of the night with the curious idea that the room is turned in some way. We recall ourselves, and know certainly that we are lying in bed with the right side toward the wall, and in spite of this we feel that we have the wall to the left and the room to the right and are amazed at the tenacity of this illusion, which remains for many seconds in spite of reason; until, extending the arms, the contact with the wall causes it suddenly to vanish and brings the mental images back to their proper position. It is to be presumed that this reversal at awakening from sleep is due to the prolongation of a dream in which one has thought himself in another chamber."

I may now report the observations which I have collected.† I shall follow these by my personal observations, and then review the whole by presenting a general description of the phenomena in question.

Observation I.

(By H. Beaunis, Professor in the Faculty of Medicine at Nancy; Director of the Laboratory of Psychology at Paris.)

"Formerly I often made the trip from Paris to Nancy, and each time I observed in myself the following illusion, very difficult to describe in a way to make it easily comprehensible to the reader. On leaving Paris I had, as every one has, a sensation of going in a definite direction; in other words, the direction of my passive movement sustained a definite relation to the landscape which the train traversed.

* *The Sense of Orientation and its Organ in Animals and Men* (Rev. Phil., July 1882).

† These observations have been collected among my immediate associates: they come from persons whom I see daily, whom I have long known, and in whom I place all confidence. All these persons have habits of close observation.

This sensation remained the same, fixed, until at a short distance from Nancy. Then suddenly, at a given instant, at the moment when I turned my eyes to the landscape, I had the sensation of a sudden change of direction, or rather I had a clear sensation that the relation of the landscape to the direction of my passive movement had suddenly changed. I cannot better express the sensation which I felt than by the expression 'being turned around' (*renversement*). It seemed to me that the landscape turned itself about, and that I travelled in a direction opposite to that which I followed on leaving Paris. This sensation was produced equally whether I rode forward or backward. I was living at that time in Nancy very near to the railroad, a little in front of the station ; and the sensation of being turned around was produced only when I reached a region which was entirely familiar to me.

"This sensation was not produced when I made the journey in the opposite direction, i.e., from Nancy to Paris. It was not caused when I travelled from Nancy to Strassburg or from Strassburg to Nancy—a journey which I seldom made, however.

"The principal point of explanation, in my opinion, is that the sense of being turned around was produced only when I entered a region in which the smallest details, the roads, the houses, the gardens, etc., were familiar to me ; and at the precise moment when I laid my eyes on this landscape. I have at different times tried to hold my eyes fixed on the landscape for a long enough time to see whether and by what means this sensation would be produced. But fatigue very soon obliged me to withdraw my gaze, and fix it upon the immovable parts of the interior of the car ; and the sensation appeared only at the moment when my eyes moved from the car to the landscape.

"A further explanation is this, that, owing to the location of the two stations at Paris and Nancy, the train in starting from Paris goes from south to north, while at the arrival at Nancy it is directed from north to south (approximately). It is possible that this change of direction impresses me only when I arrive at a region all the details of which are known. I should add that my faculty of orientation is not well developed."

We remark that M. Beaunis, in trying to describe the impression felt, has used precisely the word (*retournement*) employed by Forbes, although he had no knowledge of the description of the latter. M. Beaunis cannot explain the illusion. He states that it is always produced in the same external conditions, during a railroad journey and at the arrival of the train at a certain place.

Observation II.

(By M. Jacques Passy, Chemist.)

This second observer gives to the phenomena the name of disorientation, i.e., not a total loss of sense of direction. We shall see that it deals with precisely the same illusion, although the conditions in which it is produced are different.

"I clearly recall three cases of disorientation, together with the cause which produced them. But the effect has been produced oftener, perhaps nine or ten times altogether, without my having remembered the details. In general, I possess a good enough sense of direction; for it guides me in dense woods or in Paris. I also remember very well the names and appearance of streets.

"I was proceeding toward the Latin quarter, and had to cross the Rue de Reine, with the Mont Parnassus station on the right, the Boulevard St. Germain station on the left; and I proceeded with the idea of coming to the Rue de Reine. Probably lost in thought, I crossed this street without perceiving it. I continued my way persuaded that I had it before me. At the end of some moments, not recognizing where I was, I retraced my steps, reading the names of the streets. I came to the Rue de Reine with the station on the left, the boulevard on the right. My confusion was extreme, and I found myself for some time in a state of complete disorientation, and unable to comprehend the situation. This state did not at all resemble that which is produced in an unknown place—in a forest, for example, where one has gone astray and does not know where he is. Here you know very well where you are. You have a very clear sense of direction and you know perfectly where things ought to be; only this direction and this place are just the opposite of their real position. The station of Mont Parnassus ought to be on the right, and I did not understand why I did not see it there. What we have then in this particular case is a completely false orientation. All the objects and streets occupy positions exactly opposite to those which they should. When you recognize the objects you realize that you are under an illusion, but the illusion does not disappear. I remember that while a child, returning home one day from the Bois de Boulogne, I asserted to those who went with me that we were following a road exactly opposite to the right direction, and I found the direction which they forced me to take absurd to such a degree that I wanted to cry, and my error disappeared only when near the house. At the end of a certain period of conflict, three or four minutes perhaps, the illusion disappeared. And when it disappeared for one street or one object, it disappeared for all. I could then immediately take the road and direct myself correctly. Further, the same illusion is produced in identical conditions. I crossed, in the same way, the Boulevard Sébastopol without perceiving it, and the illusion was of exactly the same nature.

"Very lately, coming from the Place de la République, I went to a house situated about the middle of the left side of the Rue de Temple. The person whom I went to see lived in a court with many entries. My visit ended, I started to go out, when, all at once, instead of taking the right to return to the Place de la République, I took the left toward the Hôtel de Ville. How did I realize my mistake? Whether it was a simple distraction, or whether I had lost in the court the sense of direction, or had simply not recalled whether the house was on the right or on the left, I do not know. It is certain that while on my way I felt sure of meeting the Place de la République. Thus my confusion was extreme on coming to the Hôtel de Ville. As before, I was some moments in recognizing it. Then I recognized the Hôtel de Ville, without destroying the illusion. It disappeared, however, very quickly, and, I think, like the others, when I understood the cause of my mistake.

"In all the cases that I have cited, the imaginary direction was exactly opposite to the real direction. I do not recall having an impression of a direction at right angles to the real direction. We often learn of errors arising from failure to estimate angles; but these easily rectify themselves without giving place to anything which resembles vertigo, of which I have just cited three cases."

Observation III.

(By M. Victor Henri, Student of Science.)

I now record two observations of exactly the same kind as those which precede. The details alone are different. I attach particular importance to them because they come from a man who has the habit of analysis.

"First Observation.—In the month of September, 1893, I went with some people to the bridge of Austerlitz to take the boat and go either to Charenton or to Auteuil. This had not been decided on beforehand. On coming to the bridge we decided to go to Auteuil, and we took the boat on the left side of the Seine. The boat was turned in the direction of Charenton. We sat down in the cabin at the back of the boat. I did not notice how the boat was turned. In passing before the church of Notre Dame I saw it, for the first time, through the cabin-windows. I had a strange feeling akin to astonishment. I looked again and again, as if I did not see well, or as if I did not believe what I did see; for it seemed to me we were going in the direction of Charenton and not to Auteuil. When I was not looking through the window it seemed as if we were going to Charenton. Looking out, I was obliged to admit that I was deceived. It is a very painful feeling. When I went outside I saw with astonishment that the Palais de Justice was on the left and not on the right. When I descended into the cabin the illusion disappeared.

"Second Observation. (Made on the morning of January 1, 1894.)—In my chamber the bed is in front of the window. I have to lie on my *left* side to see the window. The curtains are thick and let in very little light. In the evening I lay on my left side, and do not recall having turned during the night. I slept well. In the morning I awoke, opened my eyes, tried to see if it was day, and looked fixedly before me so as to see the window. But I could see nothing at all, not a trace of light. Then, *suddenly*, I had the vague feeling that something was wrong, only I knew not what. I said to myself, 'I am lying on my right side: the door is there [I indicated the opposite direction], so that I ought to see the window before me; but I do not see it.' Then I made a movement with my hand and touched the wall. I felt it with astonishment, recognizing that I was very much deceived. Turning, I saw the window on the other side, and felt at the same moment that I had deceived myself as to the direction of the door. During the night I had turned over without knowing it. The result was the same, therefore, as if the bed had been turned 180°."

Observation IV.

(M. Philippe, Chief Assistant in the Psychological Laboratory at Paris.)

This observation differs from the preceding in that the illusion of direction which is here described is not relative to the objects present, but to objects represented in memory.

"When I think of a place, a street, a monument in Lyons, I myself being at Paris, I picture to myself a map of Lyons turned around. A certain side of Lyons is nearest to Paris, that by which I enter Lyons in going *as the crow flies* from Paris to Lyons. I invariably picture Lyons in such a way that the wrong part of it is brought around in the direction of Paris. . . . This habit of orientation takes such hold upon me that I prefer to arrive in Lyons at the depot on the side which I picture toward Paris, although the other depot would be more convenient."

Observation V.

(M. Courtier, Demonstrator in the Psychological Laboratory at Paris.)

In the following observation the illusion is very slight and it is not certain that it is of the same kind as the others.

"One night I took the train from Rouen to Paris. At the end of a half-hour I fell asleep. At Vernon I suddenly awoke and, going out, walked a few moments on the platform of the station. After entering the car again I had the sensation of returning to Rouen. It seemed as if I were going away from Paris, although I took the same seat. There were at the station, the instant when I descended, two trains from opposite directions; and although I had not crossed the track, and although I did not think I had crossed it, I asked myself for one or two minutes whether I had not been deceived in the train. I was somewhat anxious, and found it necessary to ask my neighbors as to the fact. When I had been assured that we were going toward Paris, the illusion still persisted for some minutes. It was half-past nine in the evening and very dark. I have made this trip a hundred times in ten years, and have suffered the illusion but this once."

Observation VI.

(By M. P. Thélohan, Chief of the Histological Laboratory at the Collége de France.)

"I have often felt complete change of direction on the railroad or in a carriage. It has been generally on waking from a more or less profound sleep, or after some degree of drowsiness, that I have had this illusion. For example, being asleep in the car, I was at a given moment awakened by the arrival of the train at a station, and by the noise in the depot. Then sleep began to seize me. The departure of the train caused an awakening more or less complete, and it then seemed to me that the direction of the journey was absolutely changed. I remember very clearly having had under these conditions a moment of very disagreeable anxiety, of real pain. It was only after some moments that, after thoroughly waking, I recognized my error, by comparing the relative situations of the different objects."

This observation seems to be comparable with those of MM. Beaunis and Courtier.

Observation VII.

(By M. Robb, Bachelor of Science.)

Although very succinct, this observation puts before us very tersely the fact that during the derangement of orientation we may cease for a moment to recognize familiar objects.

"One day I was going toward the Rue Royale from the side of the Rue de Provence. At the Madeleine, instead of going to the left as I should have done, I turned to the right. Reaching the Rue Tronchet, I did not recognize either the street or the gate of the Madeleine. Two or three minutes passed before I discovered that I had taken the wrong direction."

Observation VIII.

(By M. Portier, Demonstrator in Physiology at the Sorbonne.)

This curious observation suggests that of M. Philippe.

"I arrived for the first time at Croisic one morning at break of day. I had slept in the train on leaving St. Nazaire, and it was only a few moments before arriving at Croisic that I was wakened by the voice of one of my travelling-companions. He showed us the sea just coming into view in the distance. It was at this moment that from the direction of the river (only a small part of which I saw) and from the general direction of the railroad I deduced the direction of the north and the sea. I left the train with this orientation. Croisic is situated on a peninsula, so that in going through the town and its surrounding country I often saw the sea in different directions. I then chose unconsciously that one of the directions which coincided with the orientation which had been established in my mind at the moment I left the train. I repeated this many times, visiting Croisic and its surroundings, and comparing everything with this orientation. I discovered that this was false by seeing a boat start out from the pier for a certain destination which I thought lay in quite the opposite direction.

"By means of a compass I corrected the error. During the rest of my stay at Croisic I had two very distinct orientations: the first, the false one, which had imposed itself on my mind, persisted in spite of me; and the second, the true one, which I found only at certain moments thereafter by reflection with a marked effort of will. When I was in the streets of the town or in a house, I suffered no annoyance from this fact; but when I made an excursion into the surrounding country, or when some one mentioned to me the name of a distant place, Belle-Isle, England, New York, I suffered very great inconvenience in representing the place to myself in its true situation. This annoyance, this indefinable distress, slightly comparable to that which one suffers when dizzy, persisted during the whole time of my stay at Croisic (about a month and a half). This state left an impression in my mind so disagreeable that now, whenever I arrive in a place which I do not know, I take care to orientate myself properly."

M. Portier, in trying to show by a sketch the false orienta tion of which he speaks, compared to the true, finds that they are not exactly the reverse of each other, but show a relative displacement of from 60° to 100°.

Observation IX.

(By the Author.)

I have had these illusions of direction for a number of years. The first which I remember dates from 1876; and as lately as three weeks ago (March 1894) I experienced it again. I have taken note of about a dozen different cases which have occurred to me—all very similar. From 1880 to 1882 I often visited the Louvre and became quite familiar with the halls of painting and sculpture. But I have never taken pains to keep my direction in these rooms, and have made very many *détours* to right and left; so that, regularly, at the end of a quarter of an hour I lose the sense of direction entirely. The majority of the rooms receiving their light from above, or being lighted by windows opening into an interior court, one has very few indications from which to get one's direction. Further, I was careful not to keep any direction, my attention being concentrated upon the works of art. Now in these conditions this is what I observed on one occasion. By chance my walk brought me into a hall of which the windows opened upon the Quai de Seine, known to be on the right. I approached the window in order to look at the Quai a moment, and then suddenly I had a feeling of 'reversal.' I saw the Seine rolling before me from left to right; but it seemed quite wrong, for in the position in which I found myself the Seine ought, as I thought, to roll in the opposite direction: the landscape seemed to be turned around. This was a common experience. Often, after several intervening days, I have felt this particular illusion reproduce itself, accompanied by a painful sensation. Then I ceased to frequent the Louvre, and I did not have another opportunity to observe this. A year ago I tried it again. Going to the museum I made no effort to keep my direction, and at the end of an hour of walking approached a window opening upon the Quai de Seine, but the illusion was not produced.

About eight months ago (August 1893) I went to walk with my family in the forest of Fontainebleau. We abandoned the roads and turned into the woods to observe the lake (Mare des Marches). The sun was hidden by foliage, the atmosphere was obscure, and we had no known point of reference from which to get our direction. But we had left the road at a crossing where two roads met, one of which goes off to the Bois-le-Roi, and the other to the Corbuisson. These two roads intersect at a little less than a right angle, and, as we knew that the Mare des Marches occupied a region situated approximately equidistant from the two roads, we followed, in a straight line, the bisectrix of the angle formed by these two routes. At the end of a rather long walk which lasted nearly a half-hour, we came to a road without a guide-post—no indication of any sort. We found that this road ran into another on our right. We then discussed our position; one lady and myself expressing the opinion that the route continued to the right and ended at the Corbuisson, and we thought we could recognize the latter in the distance. Our conviction on this subject came from the idea that while we were walking through the woods we had followed a straight line, having the Corbuisson at our right and the Bois-le-Roi on the left. One lady of the company thought, on the contrary, that we had not walked in a straight line through the woods, but that we had described a semicircle toward the left, and the road followed to the right, would lead to the Bois-le-Roi. Two other persons, gentleman and lady, had no opinion. Testing the matter we found the road on the right to be the Bois-le-Roi. My lady companion and myself were therefore entirely wrong; but after verifying our position the illusion persisted. We could not drive out the idea that the Bois-le-Roi was occupying the position where the Corbuisson ought to be. Even to-day when I take this walk, I imagine the direction to be as I first conceived it, although

I know now that it is wrong; and it is only as I go on that, little by little, the sight of familiar objects corrects my error.

Several years ago I went one evening to attend a conference in the mayor's house at Neuilly. I then lived at Neuilly, and my road in coming back was to take the avenue which runs from the Pont de Neuilly to the Arc de Triomphe. In returning along this avenue, after leaving the mayor's house, I had a clear sense of being turned around. On coming into the avenue I had the Arc de Triomphe on my left hand and the Pont de Neuilly on my right. It seemed to me that as if by a stroke of a magic wand the Arc de Triomphe and the bridge had changed places. The illusion was accompanied by a very disagreeable feeling of distress—a state carrying with it a slight degree of stupor, as is expressed by the English word *amazement*. If I had been alone I do not know that I should have been able to find my way. The illusion disappeared by degrees, and, once over, I tried in vain to reproduce it in memory. This case impressed me very much, and I immediately wrote a description of it. It is exactly as if the world turned around with a man as a centre: when he opens his eyes he finds all the articles in his room displaced 180° by rotation.

The two following examples of this illusion contain each a very interesting particular. In an omnibus in Paris about four months ago I thought I recognized my terminus in the distance, a station situated in a street which ran toward the Seine. I was deceived. The street in question looked toward the west. I thought, therefore, that the street which looked toward the east did really look toward the Seine: an error of 90°, and not of 180° as in the earlier cases.

Tuesday, March 21, I took the omnibus of the Odéon to St. Sulpice. It was eight o'clock in the evening. I sat in the back on the right, so that the horses were on my right hand. When the omnibus arrived in the Rue de Richelieu I was conscious of a gradual turning of objects about me. Then I had the feeling perfectly distinct that the omnibus was going in the opposite direction (i.e., away from the Boulevard des Italiens, which is at right angles to the Rue de Richelieu). I was only a moment under this illusion. I was not the dupe of it, because I knew very well that the omnibus could not have taken the opposite direction without my perceiving it. I was very calm, and having been studying this phenomenon for some months I was very glad to get it again. I can now make this illusion artificially by thinking a moment of the state of the omnibus. Further I find myself incapable when I go back to it in thought, of imagining the omnibus going in the right direction. I find myself again in exactly the same mental situation, and I again experience the same illusion without being able to correct it. To continue the account: I descended with my friends to the station of the Boulevard des Italiens, and we were directed toward the Théâtre du Vaudeville on this street. To this day, although I am perfectly acquainted with this quarter, I am no longer able to keep the direction of the Vaudeville. When I think of a certain point in our journey, the illusion returns. I recall a magazine of arms illuminated by electric light; and I again see it with the false localization which I attributed to it then. The illusion disappeared little by little, without my losing consciousness of what was going on in me. I have not been able to understand how the rectification was made, yet I feel myself capable not only of reproducing the illusion, but of reproducing the rectification. On arriving before the Vaudeville, things were readjusted after a moment. The illusion had lasted in all nearly three minutes. It did not appear again for the rest of the evening nor the next day.

I have reported this observation at considerable length because it is the second instance in which the illusion of orientation was produced while the body remained unmoved. I was seated immovable in the omnibus for about a quarter of an hour: that is, I did not change my seat. This case then recalls that of M. Beaunis.

It remains to attempt some explanation. As all the observations have clearly indicated, the illusion rests upon the orientation of objects. One observes upon his right, for example, the objects which, the actual position of the body of the observer being given, ought to be on the left; or one perceives before him objects which should be behind. This is the basis of the illusion in all the observations which we have recounted.

We can distinguish three cases: (1) normal orientation—in which the points of reference recognized confirm the former sense of direction; (2) disorientation—one has no sense of direction at all, and if he meets a familiar point of reference he accepts it and orientates himself properly; (3) inexact orientation—one meets a point of reference, finds it in contradiction with his earlier system; the false system persists, even though he knows it to be false, just as an illusion persists. This last is the case now under discussion.

As to the force of the illusion, the experiences are slightly varied. Sometimes the illusion is relatively slight, and one recognizes familiar objects and is conscious of the contradiction between the two directions suggested. In one of my own cases, however, the illusion is so vivid that one has difficulty in recognizing objects well known and familiar. The illusion is accompanied almost always by distress and anxiety.

Generally the illusion is equivalent to the effect of a rotation of 180°, and that is why most writers compare their impressions to a turning or 'reversal.' But we have cited two cases in which the reversion of objects appeared to be only 90°. The illusion appears in the most diverse circumstances: in the waking state, after a walk, or during rest on the railroad, boat, or omnibus. The active movements of the body are not, then, necessary for the illusion. The most important thing to consider is that the illusion can be led up to by conscious or unconscious judgments. In one of the observations of M. Passy we have a very striking example of it: in coming from the Bon Marché he had to cross the Rue de Rennes; he was consequently convinced that by crossing this road he would have the Mont Parnassus station on his right. It was this false orientation by reasoning which imposed itself upon him in spite of the contradiction of facts, and created the illusion. And the same explanation appears to be suitable for one of the cases which I report—that of the forest of Fontainebleau—and equally to the case of M. Portier. Perhaps the remark which M. Beaunis makes in regard to the stations at Nancy and Paris may explain his illusion. In other circumstances we may suppose, with some appearance of truth, that the subject has made a subconscious or even quite unconscious orientation of objects, and that it is this which, when brought into conflict with actual perceptions, produces the illusion. Thus M. Henri, not perceiving that the boat had turned, unconsciously orientated the objects about him as if the boat which carried him had been moved only in a straight line. What was curious in this case was that the observer took no pains to orientate himself. The orientation was made automatically, while he was occupied with something entirely different. We can, by hypothesis, extend the same explanation to my personal case in the Louvre, supposing that during my promenades in the halls I had made an unconscious orientation of the objects. I localized the north in such a direction without thinking of it, and when I looked out of the window I was seized with the sense of its incorrectness. But there are cases in which the illusion appears suddenly, without any one being able to give account of the mode of production (for example, my personal case last cited, that of M. Thélohan, etc.). The anxiety which accompanies the illusion is explained by its gravity; it extends to the relation of all objects to the body. It has been remarked, and I think very justly, that the state of anxiety follows the illusion and does not produce it.

We still need to know whether the illusion is produced or not by a particular derangement of one sense-organ—possibly the semicircular canals of the inner ear. In experimental studies that have been made on the sensation of vertigo, no one has, to my knowledge, produced such illusions of the orientation of objects. The question is interesting and certainly deserves more study.*

* Translated from the author's MS. by J. N. Dodd, Fellow of Princeton College.

DIRECT CONTROL OF THE RETINAL FIELD.

BY PROFESSOR GEORGE TRUMBULL LADD,

Yale University.

There are not a few problems in experimental psychology —and some of these by no means the least interesting and important—which require no apparatus and comparatively little expenditure of time. Among such, one was brought prominently to my notice several years ago while endeavoring to discover how far visual dreams depend upon the arrangement of the light- and color-spots in the retinal field. (See *Mind,* Second Series, No. 1.) As, at first, an incidental affair, I then found out that their arrangement could, in my case at least, be brought under control of the will. This power grew rather rapidly with continued practice. That is to say, I was soon able, by attentively willing for perhaps some three to five minutes, to cause a cross, or a circle, or two concentric circles, or some other simple figure, to appear in the retinal field.

I have recently been experimenting with a class of sixteen advanced students—three seniors and the remainder graduate students—to see whether this power of control over the retinal field is at all common. The results have been exceedingly interesting and, in some respects at least, decisive. Let it be understood that all these experimenters understood the problem perfectly well and, having studied psychology from two to six years, were quite competent to answer it intelligently. What they were asked to do was briefly this: to close the eyes, allow the after-images completely to die away, and then persistently and attentively *to will* that the color-mass caused by the *Eigenlicht* should take some particular form,—a cross being most experimented with. They were to notice the effects of time and fatigue, and were also to see whether the color as well as the form of the object thus willed was at all under control. It should be borne in mind that here was no question of the effects merely of imagination, or of visual hallucinations projected into space. The primary question was, *Can the retinal sensations which arise with the eyes closed and motionless be made to respond to volition with respect to the form and color which they assume?*

The results obtained were as follows: Of the sixteen persons experimenting with themselves, four only reported no success; nine had a partial success which seemed to increase with practice and which they considered undoubtedly dependent directly upon volition; and with the remaining three the success was marked and really phenomenal. It should be said, however, that of the four who reported 'no success,' only one appears to have tried the experiment at all persistently.

The nature of the partial success attained by nine of the class can best be understood by quoting from my notes of the reports made to me. Miss C. at first had no success at all. But by persistent trying, lines corresponding to the limbs of a cross began to appear in the retinal field; and once she got a complete cross. This experimenter thinks that her failure at first was largely due to looking too near; for several times, after she had tried in vain for some minutes, the limbs of the cross would suddenly start up in the distance, as it were. Miss C—n succeeded two or three times in getting the vertical bar of a cross which would remain for a second or two, then scatter, and then gather again. Mr. M., too, not infrequently succeeded in getting the vertical bar, but could only get the horizontal bar in a 'flickering' way; and then, while he was trying to make it lie over the other stationary bar, it would disappear. In his case the effect of fatigue in diminishing success was quickly apparent. Mr. S. also succeeds invariably in getting the vertical bar of the cross, but finds the horizontal bar more coy and flickering. The color of the figure in his case is uniformly light at first, and then changing to dark.

Mr. M—y reported that at first he could do nothing in the way of making the desired cross appear. But by persistent effort he soon succeeded in making two complete dark crosses arise in the retinal field, one of which he thought he traced to the retina of each eye. A yellow upright cross with a short horizontal bar, on one occasion, started up at apparently about two feet distant. Mr. C. also, on practice, could produce at will a perfect cross, with the vertical bar darker, which would after a brief time disappear and then reappear with the bars of a complementary color. Mr. D., like several of his colleagues, had on the first trial no success whatever. But, on the second trial, he by persistent willing developed a vertical bar which remained stationary; while the required horizontal bar, although it appeared to order, persisted in sliding up and down the vertical bar. The third time, however, he got a perfect square cross which finally changed to an × shape. He could also obtain a circle at will. This experimenter found that, to obtain the best results, he must not be fatigued by more than five to seven minutes of trial; and that frequently the desired image 'jumped' into being all of a sudden when he was about to cease trying. Of the three others who had a partial success, one could sometimes get the vertical bar; another could get both bars separately, but could not get them to cross; and the third could generally get an upright and perfect cross within three minutes of beginning to try.

I have reserved the three most remarkable cases for a somewhat more detailed statement. Of these three Miss S. is very extraordinary in her control over the *color* of the object produced at will; her control over the shapes is less complete. An unusual color-sense appears, indeed, to be congenital in the family, having been possessed by the father and several of the children. This includes the power to match very delicate shades of color, almost infallibly, by memory. At first Miss S. obtained only a 'flickering' cross; but after trying daily for several days, she became able to get this figure, or a circle, every time at will. The period ordinarily required for the complete control of the effect was from ten to twenty minutes of as steadfast willing and waiting as was possible. Then at least one particular form of a cross could be got, at will, in all the principal colors *except red.* The violet and the purples were, however, easiest to obtain. An intensely bright but not a dark blue could always be got; the green, on the contrary, was always dull, and the yellow and orange imperfect and tinged with brown. The violet cross was especially brilliant and seemed to start into the field suddenly, after the requisite period of trying. At my suggestion, the eyes on being opened after these voluntary crosses were obtained were immediately focussed on a sheet of white paper, and the cross found to appear on the paper in the complementary color.

Mr. B—r is also somewhat unusual, not to say abnormal, in his powers of vision. His eyes have some defects, especially in localizing promptly and correctly. His success is best at night, when a perfect cross or circle can readily be produced at will. The color-mass which the volition arranges into this shape is usually of a grayish color. The form generally appears to be located very near; but it may at will be set at a great distance, and then the cross appears as though seen through a long tube. Upon the cross a circle can be projected at will, which is usually best secured when the intensity of the color-mass is 'moderate.' This circle can then, by somewhat persistent willing, be separated from the cross and located near the bottom of the vertical bar on either side, but with varying degrees of difficulty.

A square cross is generally selected as 'most pleasing'; but the cross can be made to assume the figure of an ×. This experimenter noticed that the strong light from a gas-jet falling on closed eyes is not an uncontrollable color-mass; but a definite brilliant cross can be planted within it by an act of will. Mr. B—r finds that the effect of fatigue is to make the cross persistent in the retinal field; and this effect is sometimes so marked that he is obliged to open his eyes in order to dispel it.

Mr. D—s, who has studied art considerably, has a very vivid color-sense. He can produce a perfect cross, of various forms and colors, at will, almost instantly. He, too, like Miss S., found the complementary color appearing on a white background upon opening the eyes. The cross 'has a tendency' to appear in red of a very intense and brilliant hue (the complementary color was described as a 'very bright silvery blue'). So strong is the effect of gazing at this voluntarily produced figure that the experimenter feels an almost irresistible tendency to vertigo, and can sometimes scarcely avoid losing consciousness. The colors of the cross are apt to run through the order of red, orange, yellow, green, and blue. They can be arrested by opening the eyes; and after closing the eyes again, they can be made to begin at any point in the series. Mr. D—s thinks the emotional conditions have a great influence on the result. He also finds his eyes in a condition of strain, and the result of fatigue with him is to make the images grow fainter and fainter. His testimony is that he has thus seen crosses of all shapes, sizes, and colors; and apparently he has no difficulty in their construction at will.

I leave this very interesting and, I think, fruitful subject with others to continue the experiment, after noting two or three particulars. One trivial circumstance is that the vertical bar of the cross seems much the easier to produce and to hold steadily in the retinal field. Again, all the experimenters are agreed that the phenomenon is one of *will*, and that the power grows somewhat rapidly with practice. None of them —with the exception, possibly, of Mr. D—s, who speaks of a feeling rather of muscular 'strain'—experience in the production of these images any movement of the eyes. The effect of fatigue seems to vary somewhat; while with most it impairs the result, we have seen that it may operate with one experimenter to make the images disagreeably persistent.

Finally, I venture to affirm—at least in a tentative way —that we have here an experimental demonstration of the unique and inexplicable power of the volition of the ego to induce changes in the cerebral centres and the connected organs of sense,—and in this case, apparently, without any use of the muscular system to control the nature of those changes.

PSYCHOLOGICAL NOTES ON HELEN KELLAR.

BY PROFESSOR JOSEPH JASTROW,

University of Wisconsin.

During the past summer I had the opportunity of making a few tests and observations upon Helen Kellar, the blind and deaf girl whose life and education, in many respects, offer a still more interesting and attractive subject than the remarkable career of Laura Bridgman. These notes are altogether meagre and fragmentary and offer nothing more than indications of the special development of her faculties. I am urged to print them simply because no more thorough study has as yet been undertaken. The tests were made for the most part in the Psychological Laboratory in the Anthropological Building at the World's Fair, Chicago; and for a more complete description of some of the apparatus used I would refer to the Official Catalogue of the Anthropological Building, pp. 50–60.

My first tests related to her powers of touch and movement. For the pressure sense two series of weights, the first increasing by $\frac{1}{15}$, the second by $\frac{1}{30}$, beginning with a standard weight of 300 grammes, were to be arranged in order. Both sets were correctly arranged, and in a rather brief time. As these weights were raised by the hand the main sense involved was the muscle sense; about one third of all persons tested with these weights were able to arrange both sets correctly. The test accordingly indicates nothing more than a muscle sensibility at least normally delicate. Accurate tests would have required more time than was at my disposal.

An æsthesiometer applied to the tip of the forefinger of the left hand indicated that with a distance of 1.5 mm between the points they were clearly felt as double, while points separated by 1 mm felt like one broad point. The sensibility was not finer than this on the tip of her tongue. On the palm of the hand points 3 and 4 mm apart were felt as distinct. The normal sensibility for these parts of the skin is differently given by various writers; but if I may trust to the averages obtained from experiments upon a general public, Helen's finger-tips and the palm of her hand (a region interesting because it is here that the impressions of the manual finger-alphabet which she 'reads' are in part received) are decidedly more acute than in the average individual.

In the next test two series of five graded surfaces were presented to her forefinger (right hand), and by passing the finger across the wires she was to obtain a notion of their relative roughness or coarseness, and indicate their order in this respect. The surfaces were produced by tightly wrapping brass wires of various grades around an iron form. In the first series the wires increased in diameter by $\frac{1}{4}$, beginning with a wire .051 inches in diameter, and in the second series they increased by $\frac{1}{8}$. Helen arranged both series in order correctly and with considerable confidence in her judgment. Less than one fourth of all persons tested succeeded in doing this, and there was rarely any confidence in the correctness of the result.

I attempted a more accurate test of the delicacy of the 'form-sense' of her finger-tips by means of the very serviceable touch-apparatus which has been devised by Prof. Münsterberg. In this instrument small wire forms of several sizes and shapes are applied to the skin in order to determine to what extent the form can be distinguished. The best evidences of acute sensibility that I obtained were as follows: a right angle 10 mm on each side was correctly called such and distinguished from an angle of 60°; a set of 8 points set upon a wire circle 10 mm in diameter was at first called 10 points and then 8, set in a 'round'; a series of 10 points set 3 mm apart, with the two central ones separated by 5 mm, was called 'nearly ten,' and some 'not the same distance apart.' A few tests with raised types were made, but with no noteworthy result; the statement of her teacher, Miss Sullivan, that Helen is not a rapid reader is interesting in this respect.

Furthermore a few observations with tuning-forks were extremely suggestive. The vibrations of a tuning-fork with a pitch of 1024 were distinctly, almost painfully, perceived when the finger-tip was placed lightly on the prong; and the same is true of one with a pitch of 1365, while the vibrations of a fork of about 5000 were not perceived. With the one of 1024, particularly, the vibrations could be felt by the finger when $\frac{1}{2}$ or $\frac{3}{4}$ of an inch away from the fork. This suggests a sensitiveness to the vibration-sense or sense of jar which has frequently been noted by the deaf, and has been well described by Dr. Kitto in his 'Lost Senses.' Further experiments in this direction are desirable. Helen's motor faculties seem not unusually well developed and are doubtless far surpassed by many blind persons. Miss Sullivan has observed that she is not skilful in finding her way about nor in knowing where things are in a familiar room. In the apparatus for testing the accuracy of the perception of lengths by finger-movements, the task is to arrange in order two series of five lengths, the one advancing by $\frac{2}{15}$, the other by $\frac{1}{15}$, from a standard of 150 mm. The first series was correctly arranged, in the second there was one error, and in both there was considerable hesitation and uncertainty. I next arranged a board about 2 ft. square, ruled off in inch squares. A needle was set in a convenient wooden handle, and a thumb-tack was placed at various points upon the board. Helen's finger was first guided to the tack, then taken away, whereupon she attempted to place the needle upon the tack. She was seated with the centre of the board opposite to the centre of the body and moved the needle on an average through a distance of 12 to 15 inches. Her errors in four trials were 35, 15, 15, and 25 mm, a degree of accuracy which may well be equalled by a seeing person with his eyes closed.

The rapidity of her movement seems also below normal. A single test indicated a maximum finger-movement of about 2.5 per second, where the normal (for adults) is about 5 per second. Helen is right-handed, and the attempt to move the two hands simultaneously to an equal extent, away from the centre of the body, indicates the same fact. For left-hand excursions of 133, 138, 169, and 99 mm the right-hand equivalents were 210, 168, 253, and 162 mm. The attempt to draw lines of equal length, or mark off equal distances by making a series of dots on a strip of paper, showed about a normal degree of error.

Tested with Prof. Cattell's pain-tester she declared a just-perceptible degree of pain when a pressure of 3.75 kilogrammes was brought to bear upon the tip of the forefinger of her left hand (average of three trials.) The usual result for adult women is about 5 kilogrammes, but the variation is large owing to the subjective difficulty of indicating the pain limit.

I attempted also a few tests of the quickness and scope of more complex processes. Beginning with a simple reaction-time, I touched her left hand and required her to respond by touching a key with her right hand. The times, in hundredths of a second, measured by a D'Arsonval chronoscope, were 36, 17, 16, 34, 16, 14, 15, 25. When the functions of the right and left hand were reversed the times were 28, 32, 16, 16, 20, 22. In the first series the two long reactions were clearly due to an awkward manner of closing the key. Omitting these, the first series gives an average time of 17 hundredths of a second which for a child of 14 years is probably a quick reaction. In the next series if I touched her right shoulder she was to press a key with her right hand, if I touched the left shoulder the left-hand key with her left hand, thus involving a distinction of the location of contact and a choice of movements. The times in hundredths of a second were 18, 20, 25, 22, 16, 36, 29, 24, 22, 26, 32, 29, or an average of 25, making a difference of 8 hundredths of a second for the combined distinction and choice. Compared with the average record of persons unused to reacting this is a decidedly creditable record.

My final notes deal with various memory tests, which were performed by the aid of Miss Sullivan, who spelled upon Helen's hand the letters, numbers, or words which I dictated, whereupon Helen would speak vocally the letters, numbers, or words as she remembered them. It should be mentioned that Helen is so entirely accustomed to vocal utterances that this mode of speech seems to have taken the place in her mental habits of her more primitive mode of answering in the finger alphabet. This was shown by her very strong tendency to murmur the words or letters as she interpreted the movements of Miss Sullivan's fingers. Such motor innervations clearly offered an aid to the memory, and it was with difficulty that she succeeded in repressing this tendency when I requested her to do so. It should be added that her control of the finger alphabet is remarkable. She accepted with great glee my challenge to speak with her fingers Longfellow's "Psalm of Life" as rapidly as possible, and succeeded in forming nearly seven letters in a second throughout the recitation. This is a rapidity sufficient to test the utmost capacity of a sign-reader to keep up with it. Helen had not at the time a set of single signs for the numerals; to convey to her 1, it was necessary to spell o-n-e. She at once learned a set of signs for use in my tests, but the newness of the acquisition clearly acted to the disadvantage of her memory. I shall therefore omit the tests with numerals, which show about a normal memory-span.

Beginning with letters I have the following, in which the columns O are the original series and R the recalled series.

O.	R.	O.	R.	O.	R.	O.	R.	O.	R
b	*b*	*c*	*c*	*k*	*k*	*c*	*c*	*h*	*h*
m	*m*	*z*	*z*	*l*	*l*	*l*	*l*	*a*	*a*
o	*o*	*l*	*l*	*r*	*r*	*o*	*o*	*y*	*y*
s	*s*	*t*	*t*	*p*	*p*	*f*	*f*	*z*	
i		*v*	*v*	*o*	*o*	*k*	*b*	*r*	*r*
k	*k*	*w*	*u**	*t*	*t*	*b*	*r*	*p*	*p*
r	*r*	*k*	*k*	*i*	*i*	*r*	*k*	*k*	*l*
y	*y*	*s*	*s*	*p*	*p*	*y*	*y*	*c*	
b	*b*	*c*	*c*	*a*	*a*	*a*	*b*	*f*	*y*
c	*c*	*b*	*b*	*y*		*c*	*a*	*y*	*f*
v	*v*	*o*	*o*	*c*		*u*	*o*	*g*	*g*
				f			*u*		
				w	*w*				

With less than 10 letters in a set there were rarely any errors, the series being correctly reproduced in *order*. With the above series of 11, 12, and 13 letters there are a few errors. It is interesting to note that the tendency to recall the first

* In this series she was told there was one error, and immediately corrected the *u* by a *w*.

members of the series and the last is as marked in this variety of tactual motor memory as in the auditory or visual.

I also tried nonsense syllables, but these seemed very confusing, six syllables being as many as she could repeat. With monosyllabic words, such as the following, *gate*, *bell*, *moon*, *foot*, *nest*, *kite*, *meal*, *chair*, *nail*, *toy*, she several times succeeded in repeating thirteen words correctly and in order; while with ten or eleven one could count upon a faultless reproduction. A few of these memory-tests were made in the evening; in the morning of the following day Helen was still able to repeat correctly the series of thirteen words she had learned the evening before, but had repeated them to herself a few times during the interval between the two trials. I have collected comparable data for a few hundred individuals, but they have not yet been finally computed. However, upon the basis of a preliminary survey of my material I have no hesitation in pronouncing Helen's verbal memory decidedly above the normal, and particularly when the correctness of the order is taken into account. How far this may be due to the concentration of her attention upon one sense, and to her acquiring through verbal means what to us is visible or audible, is an open question. The account of her mental habits given by Miss Sullivan (Helen Kellar: The Volta Bureau, Washington, 1892) amply corroborates the extraordinary powers of her literary memory.

I cannot conclude these notes without commenting upon the remarkable alertness and receptivity of mind displayed by her in visiting the exhibits at the World's Fair. By the courtesy of the officials the universal admonition 'Do not touch' was disregarded in her case; and it certainly was most interesting to observe the rapidly-varying expressions of her animated features, and listen to her comments, as one specimen after another from the ethnological collections was placed in her hands with some brief description of its character communicated to her through Miss Sullivan. The acuteness of intellect, breadth of interest, wholesomeness of emotional sensibility, along with such confined avenues of intercourse with the outer world, could not but impress the psychological observer as an admirable illustration of the relative functions of the senses, and the faculties that interpret and assimilate the facts of sensation in the economy of the mental life.

My obligations are due not only to Helen Kellar herself for her cheerful compliance with my somewhat arduous demands, but to her able teacher Miss Sullivan, and to the distinguished scientist who has so generously espoused her cause, Dr. Alexander Graham Bell.

PSYCHOLOGY PAST AND PRESENT.*

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I. HISTORICAL.

Modern psychology has had its principal development in Great Britain, Germany, and France. Germany has undoubtedly had greatest influence in this movement, considered in all its branches. The two main currents of development previous to the rise of the new so-called 'scientific' psychology, designated as 'speculative' and 'empirical,' had their initial impulse, as well as their fruitful pursuit, respectively in Germany and Britain. German psychology down to the rise of the Herbartian movement was a chapter of deductions from speculative principles; English psychology was a detailed analysis of the experiences of the individual consciousness. Kant, Fichte, and Hegel may sufficiently represent the succession in Germany; James Mill, John Stuart Mill, Hume, Reid, and Bain, that in Great Britain.

The work of Herbart and his school tended to bring a more empirical treatment into German thought, and its significance was twofold: it excited opposition to the speculative method, and it prepared the Germans for the results of English analysis. It is further a legitimate supposition that the spirit of experimental inquiry which has swept over Germany in this century was made more easily assimilable by workers in this department, also, by the patient and extraordinary attempt of Herbart to construct a 'mechanic' and 'static' of mind in his 'Psychologie als Wissenschaft' (1824).

* Part of the material to be used in different form in a 'Historical and Educational Report,' prepared by the author (by request) in his capacity as 'Judge of Award' for this subject at the World's Columbian Exposition.

To German thinkers also belongs the credit due to originators of all new movements which show their vitality by growth and reproduction, in that the experimental treatment of the mind was first advocated and initiated in Germany. But of this I write more fully below.

The contribution of France to psychology has been decidedly of less importance; yet the work of its writers has also illustrated a fruitful and productive movement. It has been from the side of medicine that French work has influenced current wide-spread conceptions of consciousness. Mental pathology and the lessons of it for the theory of the mind have come possibly most of all from France; or at any rate—not to disparage the admirable recent work of English and German investigators—the tendency, so to speak, of the French treatment of consciousness has been to approach mental operations from the abnormal side.

In America the influences which have tended to control psychological opinion have been mainly theological on one side and educational on the other. The absence of great native systems of speculative thought has prevented at once the rationalistic invasions into theology which characterized the German development, and the attempts at psychological interpretation which furnished a supposed basis of fact to the idealistic systems. In Germany various 'philosophies of nature' sought to find even in objective science support for theoretical world-dialectic: and psychology fared even worse, since it is, *par excellence*, the theatre for the exploitation of universal hypotheses. But in America men did not speculate much: and those who did were theologians. So naturally the psychologists were theologians also. Jonathan Edwards had a doctrine of the agent because free-will was a question of theology.

The educational influence was auxiliary merely to the theological. The absence of large universities with chairs for research; the nature of the educational foundations which did exist under denominational control; the aim of education as conceived in the centres where the necessity for supplying growing towns with pastors was urgent; the wholesome fact for our civilization that the Puritans had traditions in favor of the school and the religious school—all these things made it only necessary that books sound in their theological bearings, or affording homiletic lessons in living, should be written, in a topic of such central importance. Even the term 'psychology' is only now getting domesticated: 'mental' and 'moral' philosophy were the titles of courses of instruction on the 'soul.'

The type of philosophy which these conditions encouraged was, it may easily be imagined, realistic; and it is probably for the reasons which I have indicated that the Scottish Natural Realism was the American type of thought, and is now, except in the great university centres where systematic philosophy has become an end in itself apart from its duty to theology and education. As far as psychology was concerned, this realistic tendency was a great good. It led to a magnification of mental reality, to a reverence for the 'utterances of consciousness,' to a realistic interpretation of the 'immediate knowledge of self,' to the firm settling of the great 'intuitions,' cause, time, space, God, etc.; and in as far as this led to the direct examination of consciousness and to the testing of philosophical claims by consciousness, it prepared the way for a better and broader method. This tendency is marked even in the more influential works in theology. Channing and Emerson no less than Smith and Charles Hodge lay the corner-stone of argument again and again in the proof 'from consciousness.'

This tendency to a psychological view of philosophy and its basis in the religious motive is seen also in Scotland, the home of realism: and it is there a part of the British method of thought which I have already spoken of. The works on psychology written in America up to 1880 were, as we should expect, from the hands of theologians and educators, usually both in the same person; for it is a further proof of the association of psychology and theology that the mental and moral philosophy in the colleges was almost without exception put in the hands of the president of the college, and he was by unanimous requirement a preacher. So were written a series of works which are landmarks of American scholarship, props of evangelical theology, disciplinary aids of the highest value to the growing student, and evidences—to revert again to my argument—of the twofold influence I have indicated. Edwards's 'Freedom of the Will' (1754), Tappan's 'Review of Edwards' (1839) and 'Doctrine of the Will determined by an Appeal to Consciousness' (1840), Hickok's 'Rational Psychology' (1848) and 'Empirical Psychology' (1854), Porter's 'Human Intellect' (1868) and 'Moral Science' (1885), McCosh's 'Psychology' (1887) and 'First and Fundamental Truths' (1889)—these and other books like them show the psychology of America up to about 1880.

Speaking for psychology alone, not for philosophy, it is easy to point out their merits and defects, not in my individual judgment, but as compared with the standards of the present year of the Exposition. It is necessary, however, rather to show this by sketching the present and showing the new elements which have modified the American work and whence they came.

Coming to the present state of psychological thought, my task is made easier by reason of the divorce which has been forced between psychology as a science on one hand and metaphysics on the other. As was said above, Herbart, while failing in his attempt to apply mathematics to mental 'permutations and combinations,' yet prepared the way for a new treatment of mental phenomena. After his attempt it began to be seen that the facts of conscious life were first in order of importance and were capable of treatment in a detailed way quite independently of the questions of Being, the Absolute, and the like. The works of Volkmann, 'Lehrbuch der Psychologie' (4th ed., 1894), and Lipps, 'Die Grundthatsachen des Seelenlebens' (1883), illustrate this.

This was only to begin to do what had been doing in England since Locke. But the Germans now went further: they asked the question—which had been groped upon before by Descartes, by Leibnitz, and by Reid—how can psychology be a science when one of the evident conditions of the flow of mental states, of their integrity and their trustworthiness, the brain, is left quite out of account? What is the law of connection of mind and brain? And is it possible to modify the brain and so to modify the mind? If so, then that great instrument of scientific work, experiment, may perform a part for the psychologist also, and his resources be magnificently enlarged.

This is the question of Experimental Psychology. It was answered in Germany in the affirmative. Lotze, in my view, deserves the credit of it, the credit of the great-minded constructive pioneer; and Wundt is the founder of the science in the sense that he first realized the expectations of Lotze's genius by actually planning and executing experiments on a large scale which made the affirmative answer an irreversible fact of history. Lotze's 'Medicinische Psychologie' appeared in 1852, Wundt's 'Grundzüge der Physiologischen Psychologie' in 1874. Between the two, however, came Fechner, whose theoretical construction of the new work and its methods shows all the exactness of treatment of similar discussions of natural-science principles by electricians and chemists, and published the formulas in which he attempted to give universal statement to the discoveries of E. H. Weber on the intensity of sensation-states. Fechner's 'Elemente der Psychophysik' appeared in 1860.

Apart from the actual development of this new method—a point to be spoken of later on—it has profoundly modified the general conception of psychology even where its validity as a method has been denied. There has been nothing less than a revolution in the conception of psychology since the publication of the works just named. One of the motives of this revolution came thus from Germany. The other—for it has two great phases—is due to English thinkers: the evolutionists, of whom Herbert Spencer ('Principles of Psychology,' 1855) is the chief. These two influences are seen in two great points of contrast easily made out between the psychology of to-day and that of yesterday in America. The latter I have described above. Its two main characteristics, for purposes of the present contrast, are first, its character as so-called 'faculty-psychology'; and second, its character as holding to what I may call a 'ready-made' view of consciousness—technically an 'intuition' view of consciousness. In opposition to these characters, current psychology is 'functional'—holding to mental 'functions' rather than to mental faculties; and finds this function to be 'genetic' rather than intuitive — the functions 'grow,' instead of being 'ready-made.'

The old conception of 'faculties' made the different phases of mental process in large measure distinct from one another. Memory was a 'faculty,' a 'power' of the mind; thought was another, imagination a third. The new functional conception asks how the mind as a whole acts, and how this one form of activity adapts itself to the different elements of material which it finds available. The old terms 'memory', 'thought,' etc., are retained; but with the distinct understanding that they do not stand for divisions in the mind, or different processes, one of which may be held in reserve when another is acting, etc. On the contrary, the process in consciousness is one; and it is a psycho-physical process as well. The particular way in which this one function shows itself is a matter of adaptation to the changing conditions under which the activity is brought about. This transition is due in part also to the insight of Herbart and to the demand for unity insisted upon by the evolutionists.

The other point of contrast is equally plain. The 'genetic' point of view in current discussion is opposed to the older 'intuitive' point of view. The mind is looked upon as having grown to be what it is, both as respects the growth of the man from the child, and as respects the place of man in the scale of conscious existences. The understanding of mental facts is sought in the comprehension of their origin as well as their nature: and the question of the validity or worth of 'intuitive' beliefs in consciousness is subordinated to the question as to how the mind came to have such beliefs.

Both of these points of contrast have been further defined by the progress of general philosophy in America. The demand for unity in mental interpretation has not come from naturalistic evolution alone (John Fiske, 'Outlines of Cosmic Philosophy,' 1874; Thompson, 'System of Psychology,' 1884); an equally pressing demand has come from idealistic metaphysics, which seeks for continuity in the natural series as zealously as does the advocate of evolution. The influence of Hegel, as interpreted in the works of Green, and later in those of Caird, has been potent in effecting this transformation. It is easy to see also that the same union of forces is quite feasible as respects the genetic development of consciousness, although the new idealists have not done justice to this growing tendency in modern psychology.

The line of cleavage, in the current discussions of general psychology, is drawn on the question of the interpretation of mental 'function': both sides claiming the same full liberty of genetic research and the same resources of analysis and experiment. The 'Associationists,' on one hand, carrying on the tradition of the British empiricists, construe mental function after analogy with the ordinary interplay of forces in the objective world; the 'Apperceptionists,' on the other hand, hold that mental function is a form of irreducible cosmic process. Apart from original monographs on special topics, no work on psychology to-day commands much attention, either from psychologists or from students of philosophy, which does not show itself alive to this main issue. The works of Lotze and Wundt have had great influence upon Americans in the direction of this general statement of the problems of psychology: and it is especially the philosophy of Lotze which is replacing by a reasoned and critical realism the earlier theological dogmatic view so long prevalent in the United States by inheritance from Scotland.

On the literature of present-day psychology I can do no better than quote the following passage freely translated from the most recent German work on general psychology, itself fully representative of the present state of knowledge—'Grundriss der Psychologie,' by Professor Külpe of the University of Leipzig (pp. 27 ff.):

"About the middle of the nineteenth century experimental and psycho-physical psychology began its course in Germany. While Herbart recognized a threefold influence of the body upon the mind, . . . it was Lotze who made a thorough beginning in the employment of the data of physiology. Lotze, indeed, began his work with certain metaphysical expositions after the manner of the older German writers, and is very far from the recognition of a universal psycho-physical parallelism. But he does not hesitate to speak of the nervous conditions of mental processes, and he had the good fortune to suggest hypotheses of value where exact knowledge was wanting. The real foundation of Experimental Psychology was laid, however, by G. T. Fechner, who sought to carry out in a thorough-going way the conception of a functional relation between mental and physical processes. Although the mathematical form which he gave to this relation . . . does not hold, yet he gave to the exact science of psychology an extraordinary impulse, by reason of the new conceptions which he introduced, the methods of procedure which he both formulated and applied, the working over which he gave to the material he had in hand, and the observations and researches which he himself carried out. . . . The union of the experimental and psycho-physical was finally accomplished by Wilhelm Wundt . . . in his classical 'Grundzüge der Physiologischen Psychologie' (1874, 4th ed. 1893). By this unity of conception and by his comprehensive treatment of all mental phenomena . . . he has made the current phrase 'modern psychology' applicable. . . . Wundt gave a further important impulse to the cultivation of experimental psychology by founding the laboratory in Leipzig in 1879, and establishing the 'Philosophische Studien,' a journal devoted mainly to the publication of researches from his institute.

"Additional works may be mentioned of very recent date, which must be reckoned in their character as belonging to the modern psychology thus founded by Wundt, although they differ more or less essentially in system and in theory from him and from one another: Höffding, 'Psychologie in Umrissen,' 2d ed., 1893, German translation from the Danish (English translation, 1891); Ladd, 'Elements of Physiological Psychology,' 1887; Sergi, 'La Psychologie Physiologique' (translation from the Italian, 1888); W. James, 'The Principles of Psychology,' 1890; Ziehen, 'Leitfaden der physiologischen Psychologie' (1891; 2d ed., 1893); Baldwin, 'Handbook of Psychology,' 1891 (2d ed.; 1st ed., 1889–90); J. Sully, 'The Human Mind,' 1892.

"We may mention also certain periodicals which represent the same current of psychological thought: 'Philosophi sche Studien,' edited by W. Wundt (vols. 1–8, 1883 ff.); 'The American Journal of Psychology,' edited by G. S. Hall (vols.

1–5, 1887 ff.); 'Zeitschrift für Psychologie und Physiologie der Sinnesorgane,' edited by H. Ebbinghaus and A. König (vols. 1–5, 1890 ff.)."

The part taken by American students in the present psychological movement is seen in the fact that of the seven works thus cited by Külpe three are by Americans, and to them must be added 'Psychology: Descriptive and Explanatory' (1894), by G. T. Ladd, and the journal 'The Psychological Review,' edited by J. McK. Cattell and J. Mark Baldwin (vol. I, 1894). Another important French work of recent date is 'La Psychologie des Idées-Forces,' by A. Fouillée (1893). The position of psychology in the American colleges and universities is described in a further section below.

Other important contributions to Experimental Psychology —apart from the long series of monographs and research articles published in Germany and America—are Helmholtz, 'Physiologische Optik' (1867, 2d ed. f. 1886, French translation), and 'Tonempfindungen (1863, English translation); Stumpf, 'Tonpsychologie' (1883–90); and Münsterberg, 'Beiträge zur experimentellen Psychologie,' Parts I–IV (1889–93).

The contribution from the side of mental pathology has become important on account of the *rapprochement* which has obtained in recent years between the alienist and the psychologist. The works of Pierre Janet, 'Automatisme psychologique' (1889) and 'L'État mental des Hystériques' (1892–93), and of Bernheim, 'Suggestive Therapeutics' (English translation, 1889), and 'Études de la Suggestion' (1892), are most important. To them should be added the works of Ribot, 'Diseases of the Will,' English translation (5th French ed., 1888); 'Diseases of Memory,' English translation (5th French ed., 1888); 'Diseases of Personality' (2d ed., 1888; English translation, 1891), together with the many original contributions on the subject of hypnotism and aberrations of personality published in the 'Revue Philosophique' (edited by Th. Ribot, vols. I–XXXVI, 1876 ff.) and summed up in part in 'Les Altérations de la Personalité (1893) of Alf. Binet.

Further, the treatment of psychology in accordance with the British tradition, from the point of view of description and analysis, has been carried forward by Ward in the article

'Psychology' in the Encyclopædia Britannica, 9th ed. This type of research has also had its organ of publication in 'Mind: a Journal of Psychology and Philosophy,' edited by G. Croom Robertson (vols. I–XVI, 1876 ff.) and by G. F. Stout (New Series, vols. I–III, 1892 ff.).

Finally, the genetic treatment of consciousness has been advanced by the works of Spencer, 'Principles of Psychology,' 1855 (3d ed., 1880); Romanes, 'The Origin of Human Faculty,' 1884–1888; Morgan, 'Animal Life and Intelligence' (1891); and Galton, 'Inquiries into Human Faculty' (1883) and 'Natural Inheritance' (1889).

II. The Method and Main Divisions of Experimental Psychology.

To say that this is the age of science is only to repeat what is now trite and what no student either of philosophy or of history needs to be told. It is the age of science because it is the age of devotion to science and of results in science. But it is a very different thing to say that this is the age of scientific method. Former ages have seen devotion to science and results in science, but I venture to say that no former age has, as an age, realized a scientific method. So prevailing, however, has the new method now become, and so customary to us, that it is only by historical study that we are able either to see that it is new, or to work ourselves into that degree of intellectual sympathy for the old which the earnest endeavor and unflagging patience of the heroes of philosophy in the past rightfully demand for all time.

In characterizing our time by the word 'scientific,' as regards method, I mean to say something which is true in philosophy, politics, literature, as well as in the investigation of nature; and to dwell only on the department of thought in which such a method has been, and is, most difficult to realize. In philosophy it is not fully realized; and yet I believe that any class or school of philosophic thinkers who do not face toward the scientific east are steering up-current and will be absent when science and philosophy enter a common barge and together compass the universe of knowledge. For it is a part of the same conviction as to scientific method that neither science nor philosophy will ever succeed in compassing it alone. However painfully this advance may have been won and however loudly the dogmatists may deny its justification, it is sufficient here to signalize the fact that philosophy has in the present half-century thrown open her doors to the entrance of critical and empirical methods, and that the results already accruing are evidence of the bigness of her future harvest.

In general philosophy what has been called scientific method is better known, as I have said above, in a twofold way, as empirical and critical. Retrospectively what we now have to rejoice in in philosophy is due about equally to two traditions, represented by Hume and Kant. The burden of current idealism, as far as it is worthy of consideration in our time, is to purify and conserve the work of Kant. And the burden of empiricism, under the same restriction, is to refute Kant with the only weapons which he himself considered of worthy temper. The battle is drawn at these close quarters, and round them both is thrown a common ring of scientific procedure.

In psychology the modern transformation comes most strongly out. Here we find an actual department of knowledge handed over to a new class of men for treatment, so remarkable is the demand for scientific method. It is no longer sufficient that a psychologist should be familiar with general philosophy and its history, or capable of acute logical criticism of systems; it is necessary, if he would deal successfully with the new problems and gain the ear of the advanced philosophical public, that he should reason from a basis of fact and by an inductive procedure. In short, he must not bring his philosophy as speculation into psychology, but must carry his psychology as fact, in its connection with physiology, ethnology, etc., into general philosophy.

To illustrate this change, and its effect on general theories, recent discussions of the idea of space may be cited in comparison with its earlier and more speculative treatment. The reasonings of James, Wundt, Bain, Spencer, differ so essentially from the argumentation of Kant and earlier men that it is almost impossible to find common ground between them. No one among those who accept Kant's results depends in our day very largely upon his reasons: the question is shifted to another field. The physiologist has as much to say about it to-day as the psychologist, and the speculative philosopher must recognize them both.

This tendency of the day in philosophy may be expressed by a chemical figure as a 'precipitating' tendency. We are endeavoring, and successfully too, to throw all questions which are capable of such treatment to the bottom, as a precipitate—a psychological precipitate—and are then handing them over to the psychologist for positive treatment. As long as our data remained in a solution of ninety parts water (which, being interpreted, means speculation), it was difficult to handle them scientifically. While admitting the utility and necessity of ontology in its place, current psychology claims that its place must be better defined than formerly it has been, and that whenever we can secure a sediment, a residuum, a deposit, apart from a speculative solvent, this is so much gain to positive science and to truth.

One of the ideas which lie at the bottom of the so-called 'new psychology' is the idea of measurement. Measurement, determination in quantity and time, is the resource of all developed science, and as long as such a resource was denied to the psychologist he was called a scientist only in his function of description and classification, not in the more important functions of explanation and construction. And the justification of the application of measurement to psychological facts has come, not from theoretical considerations—for they were all opposed, and still are, in many of the books of the new idealism—but from practical attempts to do what philosophy declared to be impossible. That is, experiment has been the desired and only 'reagent.' It is true that theoretical justifications are now forthcoming of the application of experiment to consciousness, but they are suggested by the actual results and were not in sufficient currency to hinder the influence of Kant's ultimatum, for example, that a science of psychology was impossible.

By experiment in this connection is meant experiment on the nervous system with the accompanying modifications it occasions in consciousness. Efforts have been made in earlier times to experiment upon states of consciousness directly. Descartes deserves credit for such efforts, and for the intimation he gives us, in his theory of emotions, of an approach to mind through the body. But the elevation of such an approach to the place of a recognized psychological method was not possible to Descartes, Kant, or any one else who lived and theorized before the remarkable advance made in this half-century in the physiology of the nervous system. And even as it is, many questions which will in the end admit of investigation from the side of the organism are still in abeyance till new light is cast upon obscure processes of the brain and nerves.

A little further reflection will show us that the employment of experiment in this sphere proceeds upon two assumptions which are now generally admitted and are justified as empirical principles, at least by the results. They are both assumptions which the physical scientist is accustomed to make in dealing with his material, and their statement is sufficient to exhibit their elementary importance, however novel they may sound to those who are accustomed to think and speak of mind as something given to us in entire independence of organic processes. The first of these assumptions is this: that our mental life is always and everywhere accompanied by a process of nervous change. This is seen to be necessary to any method which involves the passage of mind to body or the reverse by the interpretation of effects. Which is cause and which effect, the mental or the physical change, or whether they both are effects of an unknown cause, is immaterial—to consider such a question would be to introduce what I have called the 'speculative solvent.' It is sufficient to know that they are always together, and that the change in one may be indicated in symbols which also represent the change in the other. The second assumption is based upon the first, viz., that this connection between mind and body is uniform. By this is meant what in general induction is called the uniformity of nature. Any relation sufficiently stable to admit of repeated experiment in the manipulation of its terms is in so far uniform. Experiment would be useless if the relation it tends to establish were not stable, since the result of such experiment would give no antecedent likelihood as to the result of others under similar circumstances. Experimental psychology, therefore, rests upon the assumption that a relation of correspondence—be it coexistence or causation—once clearly made out between a mental and a nervous modification, it must hold good under any and every repetition of the same experiment under the same conditions.

These two assumptions made, we have at once the possibility of a physical approach to the facts of consciousness. The result is a relative measurement of such facts in terms of the external stimulation of the nerves, in regular and normal conditions of the activity of attention.

Further, it is apparent that such a means of experimentation may become available either under artificial or under natural conditions, according as the nervous stimulation is due to an external excitation, or arises from some unusual condition of the organism itself. All cases of brain or nervous disease, on the one hand, offer opportunities for boundless observation; the unusual manifestations being changes due to the organic disturbances of disease. Here nature has arranged and actually performed the experiment for us; the only difficulty being the physiological one, that the cerebral states may be as obscure as the mental states which they are used to explain. All such cases of mental changes due to internal organic changes are classed together under the name of Physiological Psychology. It includes all questions which relate to nerve physiology and pathology, illusion, hallucination, mental disease, hypnotism.

On the other hand, experiments may be arranged for the normal stimulation of the sense-organs—skin, muscles, special senses—under artificial conditions as explained in part below. This is Experimental Psychology. On these lines modern experimental psychology falls into two great departments. As the normal properly precedes the abnormal, it is well to consider the line of researches based upon external experiment, confining ourselves to a more or less cursory view of results of historical interest.*

*In the official report, sections are included on 'Psycho-physics' (Weber's Law) and 'Mental Chronometry' (Reaction-times).

III. The Exhibits in Psychology at Chicago.

We are now prepared to consider the exhibits made in the interests of Experimental Psychology at the Columbian Exposition. It is evident that departments in which progress is in the main abstract and immaterial—such as the social, moral, and theoretical sciences—cannot show their work to the eye, and so have heretofore appeared at the world's great expositions only as their results have been embodied in more practical life, in education, and in institutions. It is, however, unfortunate that this should be so: for the more ideal and spiritual aspects of a nation's life are just the aspects in which popular instruction is defective, and these are the aspects which should least of all be omitted in a survey of the conditions of present-day civilization. Yet it is so; and it becomes easy to see, therefore, that it is only as psychology has become experimental and so has found it possible to state her problems and results to a degree in forms which allow of diagrammatic and material representation that she is able to 'exhibit' herself. What psychology showed, therefore, at the Chicago Exposition was the experimental side, as I have sketched its problems and methods in what precedes.

The exhibits bearing on psychology in its scientific aspects —as apart from the educational aspects, of which I speak later on—may be placed in order thus:

(A) A collected exhibit made by the department of Anthropology, of which Professor F. W. Putnam of Harvard University was chief, under the immediate direction of Professor Joseph Jastrow of the University of Wisconsin, consisting of a Psychological Laboratory in operation with all its accessories.

(B) A collection of instruments shown in the German Educational Exhibit under the heading 'Psychophysics.'

(C) Instruments shown in the general exhibit of the 'Deutsche Gesellschaft für Mechanik und Optik.'

(D) The private exhibits of particular instrument-makers.

(E) Exhibits made by single universities, i.e., those by the University of Pennsylvania and the University of Illinois.

I may consider these briefly in order.

(A) *The Laboratory for Experimental Psychology, gathered by the Department of Anthropology* (*Ethnology*).—This laboratory constitutes the first attempt ever made to exhibit at an international fair the state of progress of the world in this branch. When taken in connection with the other laboratories exhibited by this department, i.e., in Anthropology and Neurology, it may be accepted, in its main features, as an adequate historical index of the psychological progress of the nineteenth century. The general features of the working laboratory cannot be better described than in the words of the director, Professor Joseph Jastrow.*

The Psychological Laboratory.—"The object of this laboratory is to illustrate the methods of testing the range, accuracy, and nature of the more elementary mental powers, and to collect material for the further study of the factors that influence the development of these powers, their normal and abnormal distribution, and their correlation with one another. The laboratory is thus designed, not as are those connected with universities, for special research, or for demonstrations and instruction in psychology, but as a laboratory for the collection of tests. As in physical anthropometry the chief proportions of the human body are systematically measured, so in mental anthropometry the fundamental modes of action upon which mental life is conditioned are subjected to a careful examination. In both cases the first object is to ascertain the normal distribution of the quality measured. With this determined, each individual can find his place upon the chart or curve for each form of test and from a series of such comparisons obtain a significant estimate of his proficiencies and deficiencies. It should not be overlooked that mental tests of this kind are burdened with difficulties from which physical measurements are comparatively free. Our mental powers are subject to many variations and fluctuations. The novelty of the test often distracts from the best exercise of the faculty tested, so that a very brief period of practice might produce a more constant and significant result. Fatigue and one's physical condition are also important causes of variation. It is im-

* Official Catalogue of Exhibits, Department M, in which full descriptions may be found.

possible in the environment of the present laboratory to secure the necessary time and facilities for minimizing these objections. They detract more from the value of an individual record than from that of the combined statistical result. So much remains to be done in this line of investigation that at every step interesting problems are left unanswered. But what has been done emphasizes the importance and probable value of further research. The problems to be considered when once the normal capacity has been ascertained are such general ones as the growth and development with age of various powers; what types of faculty develop earlier and what later; how far their growth is conditioned upon age and how far upon education; again, the difference between the sexes at various ages, differences of race, environment, social status, are likewise to be determined. The relation of physical development to mental, the correlation of one form of mental faculty with others, the effect of a special training, —these, together with their many practical applications, form the more conspicuous problems to the elucidation of which such tests as are here taken will contribute. In addition to the interest in his or her own record, the individual has thus the satisfaction of contributing to a general statistical result."

(B), (C), (D), (E) *The Exhibits of* (B) *the German Educational Department*, (C) *the 'Deutsche Gesellschaft für Mechanik und Optik,'* (D) *Individual Private Instrument-makers, and* (E) *Separate Universities.*—The two German agencies mentioned as (B) and (C) send what may be considered as on the whole the best indication—when taken in connection with the special pieces of apparatus sent from German workshops to the collective exhibit of the department of Anthropology—of the application of modern mechanical skill to the construction of instruments of the delicacy required for psychological experiment. These instruments are mainly adaptations of well-known principles, and often of well-known apparatus, used in experimental physiology, physical optics and acoustics, electricity, etc. The instruments shown by the German Mechanical and Optical Society are almost entirely common to psychology and these sciences. The pieces in the German Educational Exhibit are largely the special arrangements found useful in the laboratory at Leipzig, and so show very inadequately the real progress of the science in Germany. Yet they are of great historical interest. The collection is much less complete than that made by the German instrument-makers in connection with the collective exhibit in the Department of Anthropology. In this connection it should be mentioned that the account given of Experimental Psychology in Germany by Professor Wundt in the official book, 'Die deutschen Universitäten' (ed. by W. Lexis, 1893), is not adequate if considered (and probably the author did not intend it to be so considered) as an exponent of the present condition of this science and the place it occupies in the German universities.

(D) The private exhibits of individual firms should be noted in the attempt to make one's conception of psychological activity complete. French exhibitors did not combine as the Germans did, and so lost both in effect and in local position. Yet much of the finest work is done in Paris, as is witnessed by the cases of surgical, physical, and psychological instruments grouped in the north end of the Anthropology building. An examination of the catalogues of the exhibitors (for example, that of Ch. Verdin of Paris) may serve for the details of this class of exhibits, as the united catalogues of the other collections mentioned serve in respect to them. The German makers have done their work more largely in connection with great university laboratories, and so have subserved better the needs of particular students in solving particular problems in physics and psychology: the French, on the other hand, have found the demand more marked from the side of clinical medicine and experimental physiology.

(E) The separate university exhibits of the Universities of Pennsylvania and Illinois were located respectively in the Liberal Arts and the Illinois State building. The aim of the former was to present a working laboratory restricted to a small number of topics. This original purpose was not subserved through the failure to provide attendants to collect experimental data; yet the arrangements for experiments in reaction-times and the visual æsthetics of form were instructive to visitors. Two pieces of new apparatus were exhibited by Dr. Lightner Witmer, the designer of them: a complex color-wheel which permits the alteration while in motion of the proportion of colors mixed, and a graphic movement apparatus involving new features.

The exhibit of the University of Illinois was mainly of instruments which were also included in the main collection of the Department of Ethnology. It was in charge of Professor W. O. Krohn of that university.

IV. Educational.

The educational aspects of the new work in psychology are of great importance. It is evident that education has two claims to make upon this study; one of these claims the old psychology aimed to meet, the other it was incapable of meeting. The first of these two duties of psychology to education is this: it should take its place as a factor in liberal collegiate culture in both of the functions which a great branch of learning serves in the university curriculum, i.e., undergraduate discipline and instruction, and post-graduate research discipline.

The older psychology, especially in America where it was hampered by the conditions pointed out in an earlier section, did, as I say, aim to instruct undergraduates. But even in this it was a means to another end: it was propædeutic to a philosophy and to a theology, both of which, as far as their demands upon 'mental science' were concerned, were dogmatic and intolerant. But the graduate disciplinary function was never served in any sense by the faculty psychology nor by the philosophy founded upon it in America.

The second great educative function of psychology is this: it should mould and inform educational theory by affording a view of mind and body in their united growth and mutual dependence. Education is a process of the development under most favorable conditions of full personality, and psychology is the science which aims to determine the nature of such personality in its varied stages of growth, and the conditions under which its full development may be most healthfully and sturdily nourished. One of the first duties of psychology, therefore, is to criticise systems of education, to point out 'the better way' in education everywhere, and to take no rest until the better way is everywhere adopted. This duty the old psychology did not realize: indeed, by its method and results it was cut off from the realization of it. It shall now be my aim to show how contemporary psychology is addressing herself to all these undertakings.

A. Psychology as Research Discipline.—I begin with this point because it is the most striking fact about the present state of psychology in all countries where the experimental idea has been given entertainment. Probably students and general readers hear more about 'research' in connection with psychology than with any other branch. And it is odd —indeed to workers in other departments amusing—that all this claim to research ability, and talk about 'original contributions to knowledge,' is by professors who are yet smooth-faced and generally quite inexperienced in university affairs. A physicist who makes contributions to knowledge is extremely rare, but the 'new psychology' has two men of research to every competent college instructor in its ranks.

This, I take it, is a hopeful and encouraging state of things, and has its origin in two influences: first, the new impulse has come from Germany, where the university-function corresponds very nearly to the graduate-discipline function in the few American institutions where graduate work is encouraged; and second, because the actual state of the subject is such that research is a matter of comparatively less difficulty than in the older scientific branches. Yet the actual value of this condition of things in the permanent development of the subject must be held to be disciplinary and educational; for the more serious and philosophical of the psychologists do not expect these first results of the new methods to be revolutionary in their value, nor have the researches so far published been much more than suggestions of what may be done when the method is held under better control and those who apply it have had adequate discipline and training in its use.

Accordingly, in my view, the very marked tendency to 'research' evident in the management of the new laboratory foundations of the colleges in this country is of main value as offering training to the future instructors in psychology throughout the land, rather than as offering contributions to knowledge. The students in these laboratories come largely from colleges where experimental psychology is unprovided for or held up for criticism by professors of philosophy. The utilization of their results, except in problems whose solution properly involves ignorance, crudity, and liability to individual variation, is manifestly impossible.

The research discipline offered by graduate work is indispensable, however, as discipline, since it is at present the only substitute for undergraduate discipline. This, indeed, is the function of graduate work in the other departments of science in the universities. It is emphasized, however, in psychology since, as I shall show below, undergraduate instruction in experimental psychology is still in an inchoate condition even in the few larger institutions in which it has been added to the B.A. course of study.

Chairs in Experimental Psychology occupied by men whose principal function is graduate discipline—although in some institutions the undergraduate function is being recognized—are now no longer novelties. Abroad the German universities take the lead in such instruction; yet the instructors are generally professors of philosophy or of psychology who offer experimental courses. Laboratory foundations began in Germany in 1878 with the Institute at Leipzig (Professor Wundt); they are now to be found as well at Berlin (Professor Ebbinghaus, now at Breslau), Göttingen (Professor Müller), Bonn (Professor Martius), Prague (Professor Hering), Munich (Professor Stumpf, now at Berlin), and Heidelberg (Professor Kräpelin). As for other European countries, a chair of Experimental Psychology was founded at Paris in the Collège de France in 1886 (Professor Ribot), and a 'Laboratoire de Psychologie physiologique' opened in the Sorbonne in connection with the École des Hautes-Études in 1891 (Professors Beaunis and Binet). Other such Continental foundations are to be found at Geneva (Professor Flournoy) and at Rome (Professor Sergi). At Florence a laboratory and museum of Psychology and Criminal Anthropology has recently been instituted (Professor Mantagazza). In Great Britain and her possessions the analytic method has not given way to the experimental. In Canada alone, at the University of Toronto (Professor Baldwin, now Dr. Kirschmann), a well-equipped laboratory was opened in 1891, although a little later a small sum was secured for the purpose of beginning work of this kind at the University of Cambridge, England. Lectures are given, however, both by physiologists (Professor Hill at University College, London, 1894) and by professed psychologists (Professor Alexander, Owens College, Manchester, 1893). Japan follows with one such laboratory—that at the University of Tokio (Professor Motora).

In the United States the extension of this method of treatment has been rapid, and the establishment of chairs and of laboratories extraordinary. The first laboratory was established in 1883 at Johns Hopkins University (Professor Hall), but it has since been closed. This was followed in 1888 by the establishment at the University of Pennsylvania of the first chair of Psychology alone with a laboratory (Professor Cattell). Here the first undergraduate laboratory instruction was given. Later, chairs for Experimental Psychology alone have been erected at Columbia College (Professor Cattell), Harvard University (Professor Münsterberg), where an additional Professorship in General Psychology exists side by side with it (Professor James), the College of New Jersey at Princeton (Professor Baldwin). Professorships either in Psychology as a whole, or as associated with Education, exist at Clark University (Professors Hall and Sanford), Wisconsin University (Professor Jastrow), Cornell (Professor Titchener), Chicago (Professor Strong), Indiana (Professor Bryan), Illinois (Professor Krohn), Stanford (Professor Angell), Catholic University at Washington (Professor Pace), Wellesley College (Miss Professor Calkins). At all these institutions laboratories with equipment have been provided ; and such provision has been made in others where no separate professorships have yet been erected, i.e., Yale (Professor Ladd), Brown (Professor Delabarre), Minnesota (Professor Hough), Nebraska (Professor Wolfe), Michigan (Professor Dewey, now of Chicago).

The nature of these laboratories is illustrated by the large exhibit already spoken of. That at Harvard University is the largest, best equipped, and most freely patronized by graduate students. A Harvard pamphlet-catalogue of the apparatus in the laboratory, containing also illustrations, bibliographies, and a list of topics under investigation (23 in number), was prepared by Professor Münsterberg for the collective university exhibit. The rooms given to this science, however, in the universities are usually inadequate and ill-adapted. The only such laboratory yet planned and constructed especially with regard to the requirements of this work is that at the university of Toronto, of which a description with plan is to be found in 'Science,' XIX, 1892, p. 143. The most extensive accommodation provided for this work in America is probably that at Yale, where a house with fifteen rooms is devoted to it. A description of the Yale laboratory is also to be seen in 'Science,' XIX, 1892, p. 324.

The following selected topics set recently for original investigation in two of the institutions may be taken as typical of the kind of themes through which the graduate discipline acquired in all these foundations is secured.

COLUMBIA (1893–4): "After-images—their duration and nature as a function of the time, intensity, and area of stimulation."

"The time of perception as a measure of differences in intensity, and the correlations of time, intensity, and area."

"The perception and attention of school-children."

PRINCETON (1893–4): "The progressive fading of memory for size of visual figures."

"Investigation of memory-types by means of reaction-times."

"Size and color contrast effects on the retina."

"Complex illusions of rotation." *

The treatment of general psychology is adequate as never before, also, in the graduate instruction of the country. The courses of lectures and the instruction by the Seminar method gather large numbers of students who have already graduated in less pretentious colleges. The publication in recent years of so many systematic treatises, especially in America, has

* Similar topics of research at Harvard are to be found (23 in number) in the Catalogue of the Harvard Laboratory already mentioned, and those at Yale in the "Studies from the Yale Laboratory," 1893.

contributed to this; a dominating influence in this matter being a work which has proved to be a *vade mecum* to psychological inquirers—the 'Principles of Psychology' of Professor Wm. James.

B. Pyschology as Undergraduate Discipline.—The position of psychology in the undergraduate curricula of the leading institutions also invites remark. Two important changes may be discerned in recent years, both indicating the permanent breaking away of this discipline from its earlier hampering connections: first, the recognition of the aim of the science as self-knowledge and self-control; and second, the introduction of the experimental method of instruction.

The first of these tendencies is shown in the remarkable change worked (and still working) in the qualifications and training of the occupants of chairs in Philosophy and Psychology. Even the smaller denominational institutions are following the lead of the great eastern foundations, and of the progressive state universities, in seeking men who are trained to the same rigorous interpretation of fact and search for it that are the first requisites of the genuine *Naturforscher* in other branches of science. The guardianship of this important realm, the mind, from outside, in the supposed but mistaken interests of religious and ethical truth, has had its day in many institutions—at least in any sense that denies to the investigator and teacher the full liberty of disputing hypotheses which facts do not support, and of stating those, however novel, which well-observed facts do support. Consequently Philosophy and Psychology are now self-controlling departments in the colleges; and so the courses of psychology are arranged with view both to the adequate instruction of the student in its history and results, and with view to that high discipline which the pursuit of the 'moral'—as opposed to 'physical' and 'natural'—sciences undoubtedly gives.

Second, the introduction of the experimental method of instruction has had its beginning. It consists in the actual demonstration of the leading facts of Experimental and Physiological Psychology in the class-room with added opportunities for students to perform them upon one another, and, under certain topics, upon the dissected nervous systems of animals. One of the results is the greater concreteness and interest given to the subject for younger students and the correspondingly increased election of all the branches of the tree of philosophy in the later years. The union of the two functions of introspection and experimental observation thus secured renders this branch, in my opinion, of unique and as yet undeveloped value in the total discipline of college life.

It is evident that this undergraduate service cannot be adequately realized until the science which aims to render it is itself well developed and sufficiently categorized. The actual condition of things suggests encouragement, therefore, but not enthusiasm. It is evident that such a method of instruction is at present impossible to any but the original workers in this field, and they indeed are each a law unto himself. There are very few experiments of a psycho-physical or psychological kind which are of such evident importance and value as to be recognized by all as available for class demonstration. And a more radical defect is that there are very few principles as yet formulated which can be adequately demonstrated by single or grouped experiments. Add to this the fact that the whole exhibit of apparatus at Chicago contained very few things which are suitable and convenient for untrained use or illustration, and the difficulties become in part apparent. It is a duty which experimental psychology owes to education to meet this need by bringing her results into line with the more elementary principles of general psychology, of providing simple apparatus which can be used by less expert instructors, and of preparing text-books for junior classes. While no text-book to-day exists for this purpose, it is yet gratifying that two such 'Courses in Experimental Psychology' have already been announced by competent writers, both Americans (Professor Cattell of Columbia College and Professor Sanford of Clark University).

Reference to the latest catalogues of Brown, Wisconsin, and Michigan Universities (not to mention many others) may serve to show the nature of the courses offered in institutions where the work is as yet mainly undergraduate.

C. Psychology in its Bearings on Pedagogy.—Finally, the relation of psychology to the science of education may be given a word after the discussion of its place in practical education. Pedagogy as a science treats of the application of psychological principles to the development of normal and cultured personality. The ground-work of such a science must be afforded therefore by psychology: and inasmuch as the teacher has to do with body as well as mind and with mind principally through the body, it is experimental or psycho-physical psychology to which this duty to theoretical education mainly comes home. It is needless to say that there is no such science of pedagogy in existence. Most of the books which have heretofore appeared in America on this topic—and their name is legion—are unworthy of serious attention. Further, the importation of the German *a priori* 'Systems of Pedagogics' finds its main service in keeping awake the expectation and the *amour penser* of teachers: not in affording them much empirical assistance in their task. Yet it is encouraging that the phrases 'child-study,' 'self-activity,' 'apperception,' 'scientific methodology,' are in the air, in this year of the Exposition, and every teachers' convention listens to hours of paper on such topics.

Contemporary psychology is becoming aware of this duty also, however far she may yet be from performing it. Children are being studied with some soberness and exactness of method. Statistical investigations of the growth of school-children, of the causes and remedies of fatigue in school periods, of the natural methods of writing, reading, and memorizing, are being carried out. The results of several such inquiries were plotted for exhibit in the department of Anthropology at Chicago. Questions of school hygiene are now for the first time intelligently discussed. The relative values of different study-disciplines are being weighed in view of the needs of pupils of varying temperaments and preferences. And it only remains for the psychologists — themselves teachers—to set the problems and establish the methods, and all the enthusiasm that is now undirected or misdirected will be turned to helpful account. Among those who have addressed themselves to this task in this country with information and influence two names may be mentioned, that of W. T. Harris, U. S. Commissioner of Education, Editor of the 'International Education Series,' which now includes 24 volumes, and President G. Stanley Hall of Clark University, Editor of the 'Pedagogical Seminary' (vol. I–III, 1891–4). Another journal which is doing good work for sound education is the 'Educational Review,' edited by Professor N. M. Butler of Columbia College (vols. I–VII, 1891–4).

V. Psychology and Other Disciplines.

It is necessary, in conclusion, in order that this report may adequately present the conditions under which psychology exhibits herself and her historical progress, to speak briefly of the relations which this topic sustains to the other 'moral' forces which make up largely the culture element in our present-day social environment. The traditional connection with philosophy is not severed by the new directions of our effort, but on the contrary they are made more close and reasonable. The change in psychological method was due in part, as I have said above, to changes in philosophical conception; and it is only part of the same fact that scientific psychology is reacting upon philosophy in the way of healthful stimulus. Both the critical idealistic and the critical realistic methods of philosophy are richer and more profound by reason of the lessons of the new psychology. It was only just that the science which owed one of its earliest impulses in this country to a book from an advanced thinker of the former school, the 'Psychology' of Professor John Dewey of the University of Michigan, should repay the debt by its reconstruction of the Kantian doctrine of apperception in terms acceptable to the later thinkers of that school. And it is no small gain to both schools that their issue should be joined, as it is to-day, on ground which stretches beyond their old battle-fields by all the reach of territory covered by the modern doctrines of Naturalistic Evolution, and the Association Psychology. Philosophy escapes the charge of Lewes that her discussions are logomachy, when the disputants on both sides are able to look back upon those even of the late period of Lewes and admit the essential truth of both of their hotly-contested formulas. As far as this is the case, I venture to say that it is due to the progress of psychology in giving content to the terms of the logomachy and in enabling the best men to reach more synthetic and more profound intuitions.

The relation of psychology to theology also is now as close as ever, and must remain so. And the obligation must become one of greater mutual advantage as psychology grows to adult stature and attains her social self-consciousness in the organization of knowledge. The benefits which theology might have gained from psychology have been denied in great measure through the unfortunate attempt to impose the theological method upon the treatment of the whole range of mental fact. The treatment of 'Anthropology' included in the text-books of systematic theology bears about the same relation to that of current Psychologies like Höffding's and James' as the physiology of the philosophers not long since bore to the work of the neurologists and morphologists. It is evident, however, that this condition of things is now happily mending; and it is to the credit of one man, ex-President James McCosh of Princeton College, that he first of the theologians who were teaching philosophy in this country welcomed and advocated the two new influences which I have taken occasion above to signalize as the causes of the better state of things: the influence of the German work in psychology (Preface to Ribot's 'German Psychology of To-day,' 1876) and that of the evolution theory in biology ('Religious Aspect of Evolution,' 1888).

Finally, I may note the growth of a new department of psychological study which aims to investigate the mental and moral life of man in its social and collective conditions. The evident need in such subjects as Sociology and Criminology is the knowledge of the laws of human feeling and action when man is found in crowds, orderly or disorderly, and in organizations, legitimate or criminal. This need is now beginning to be felt both by sociologists and by psychologists, and we may hope that the questions already started in Italy by Ferri, Sighele ('La foule criminelle,' 1893), in France by Tarde ('Les Lois de l'Imitation') and Guyau ('Education and Heredity,' Eng. trans. 1892), and in England by Spencer, may receive fruitful development in this country. It is an interesting sign of the times in education that the theological schools are beginning to realize the need of such knowledge of collective man, as part of the training of the ministry. Instruction in social questions is made a separate department in the Yale Divinity School and in the Chicago Theological Seminary, as well as in other such institutions.

En résumé, I have only to add that psychology is now the branch of knowledge which is developing in most varied and legitimate ways; and that the exhibition made at the Columbian Exposition, while not adequate in many respects, yet served, to those who studied it intelligently, to indicate the present gains and the future prospects of the science.

DISCUSSION.

IS PSYCHOLOGY A SCIENCE?

If the differences of view between Professor James and me as to the nature, aims, and present condition of psychology were a matter of merely private import, I should not think it worth while to discuss them. But they seem to me, on the contrary, of the highest importance for the future development of the science in this country. It will not be out of place, therefore, to speak of them briefly in the REVIEW; especially since I cannot take the statement of them in his, for the most part, appreciative notice of my book as representing me fairly.

Professor James says truthfully that "the word 'descriptive' on the title-page covers more of the contents [of my book] than the word 'explanatory.'" The same thing is true of every scientific treatise on mental phenomena that was ever written; or, indeed,—for that matter,—ever will be written. Moreover, it is also true for every form of 'natural science,' from astronomy, which can describe the aurora and the sun-spots, for example, but cannot at all adequately explain them, to biology, which, with all its wealth of descriptive details, is almost absolutely powerless as yet to state any of "the logical grounds or the efficient causes of the facts described." This discrepancy between description and explanation, between our knowledge of facts and our knowledge of causes, belongs to the very nature and progress of all human science. Has my critic never read through the detailed description of the embryology of the chick; and does not he see that it is just this careful, loving regard for facts as shrewdly observed and honestly reported, in which all the modern sciences of life are perpetually laying anew the basis for their advancement?

When, however, Professor James says that there is 'very little' explanation in my book, I quite decline to acknowledge the force of his criticism; unless I am allowed to add—very little, relatively, of the *kind* of explanation which my critic thinks is the only acceptable kind. And this brings me at once to one of the chief points of variance between us. For in a somewhat emotional statement toward the close of his review Professor James declares: "I desiderate 'conditions'; for Ladd 'analysis' and 'tracing of genesis and growth' are enough." But in another place I am given rather unusual credit for skill in 'unravelling the fibres of consciousness,' for realizing the intricacy of my facts with 'amazing thoroughness,' and for holding consistently to the view that 'each individual mind is an organic life that develops its own destiny.' Now has all this nothing to do with 'explaining' the phenomena of consciousness, with tracing the conditions, the grounds, the efficient causes, in the case of a study like that of the mind's life? Only the student who is predetermined to claim that cerebral psychology is the sole scientific psychology, and that the sole efficient causes are conjectural brain-states, could possibly answer 'No' to this inquiry. But such a negative answer throws out of the category of psychological science almost the entire body of the work now being done in our psychological laboratories, all the collection of statistics and all the experimentation in child-psychology, all the study of ethnic psychological peculiarities, and of the actual life of the mind, as undoubted experience brings it before us. And what does it actually substitute, or even promise, for this growing wealth of scientific material? I am quite willing that my critic's own voluminous essay in psychology, so far as it is distinctly one in cerebralistic theory, should be the answer.

Professor James distinctly declares that the 'crudest cerebralistic theories, or the wildest theosophic ones,' lead in a 'more healthy direction' than my resignation to the patient examination of the facts and to the working out carefully, in detail, of the various perplexing problems which our science proposes. This is exactly the opposite of my opinion. And if I thought that our somewhat like a score of psychological laboratories in this country were founded to serve such a method of advancing the so-called science, I would most willingly see them all perish in a single night.

How, nevertheless, Professor James can hold me up as *not* 'desiderating' the conditions of mental life, so far as they lie in cerebral and other nervous changes, I find it difficult to comprehend. Have I not written two books treating chiefly of this very subject—and one of them as big as, and even more 'tedious' than, the 'Psychology, Descriptive,' etc.? Even in this latter treatise are not the 'physiological conditions,' so far as they are known or can reasonably be conjectured, somewhat elaborately discussed in connection with almost every kind of mental phenomena? For 'the crudest cerebralistic theories,' as well as 'the wildest theosophic ones,' I admit I have no taste; nor will I ever indulge myself in such flights of mere fancy, even if I have to remain forever on the level ground of known facts, faithfully studied and made the basis of cautious generalizations. But that I should also be accused of a lack of 'craving for insight into causes,' and of desire to know about the mystery of 'the union of mind with brain,' is even yet more strange. This, in the face of the fact that the voices of the critics have scarcely died away who accused me of taking up altogether too much space with this very problem of brain and mind,—not to speak of Professor James's own correct conjecture that I have in preparation more on the same subject.

It would appear, then, that one point of difference between us is this. I regard psychology as the science which describes and explains the facts of consciousness. Unravelling the fibres of consciousness, tracing its genesis and growth, generalizing the laws that relate its states together, expounding the conditions of every sort on which the mental life unfolds itself—this *is* for me the science of psychology. Professor James, on the contrary, holds that there is only one kind of conditions which the student of scientific psychology cares to know about, and these are the brain-states ; and that until some Galileo or Lavoisier has arisen with a psycho-physic law that will sweep the board, as it were, we can have no *science* of psychology. I, on the contrary, assert that such a view perverts and misconceives the entire subject, substitutes 'wild' and unverifiable conjecture for genuinely scientific treatment of ascertainable facts, and surrenders to the charge of being 'unscientific' by actually becoming so.

But perhaps the differences of 'temper' between us—if I may venture to use the word—are quite as strongly marked as the differences of expressed views. As to these I certainly should not remark, did I not consider that the 'cause' is somewhat interested in these differences, too. Professor James finds my whole 'sort of treatment *tedious* as few things can be tedious.' If he deemed me the only or the chief sinner in this way, I should regard his criticism as merely personal and pass it by. But he has been kind enough to say that there are many eminent psychologists who are more tedious to him than I am. And elsewhere he has spoken freely enough of 'dear old' Fechner's 'patient whimsies,' and of the intolerable 'Herbartian jargon,' to say nothing of the 'Kantian machine-shop' and the 'strenuously feeble' prattle of Professor Green. Now different things are tedious to different people —even if they are all alike students of psychology, whether from the empirical or from the speculative point of view. But is this the spirit of a genuine student of 'natural science'? Fortunately we do not all get tired in the same way. And it is not the patient unravelling of the fibres of consciousness, the careful tracing of the genesis and development of the mind's life, but rather wild cerebralistic and theosophic theories which make the genuine student of psychological science tired.

All this is, however, for only two individuals of little or no account.

It makes no difference what seems 'tedious' to Professor James or what seems interesting to me; or whether it is the same things that interest or tire us both. Such differences may be matters of temperament or of temporary digestion. But it does make a vast difference with the welfare of psychology whether the 'virgin-minded youths and maidens' are given by their teachers to understand that the patient mastery of details is indeed 'tedious'; that they themselves already carry it all in their 'bosoms'; and that it is a 'violence done to human want' to ask them patiently to acquaint themselves with facts and proved principles. When they are 'hungry for spiritual food,' their hunger should, I suppose, be fed on cerebralistic or theosophic theories; or else it should be made to wait meal-time until the psychological Lavoisier has arisen to substitute his strong meat for my 'milk for babes.'

Now he who knows American youth—their needs, their faults characteristic or readily acquired, and their fine capacities—will be sure to judge, I think, that nothing can well be worse for them than to receive gladly such instruction as this. Doubtless they do carry much psychology in their bosoms; just as they carry much anatomy and physiology in their thoracic and abdominal cavities. But at the very time when America—and young America, too—is stepping rapidly to the very front rank of the world's students of psychological science; when the youths and the maidens are offering themselves for patient and enthusiastic work at our laboratories and lecture-halls in genuine scientific spirit and method, it seems to me immensely unfortunate that one whom they so justly admire should voice such reactionary and debilitating sentiments.

In closing I will only say that Professor James's 'learned Yale co-editor' is ready to join him in 'ringing in' the expected 'Lavoisier,' with his grand generalization that shall lift our knowledge of mental behavior to a level with that certainty which the behavior of the planets has for the mind that knows the law of gravitation; *if only* this great reformer of psychology ever arrive. But in this region he is not expected. And meantime we shall not spend our time longing and sighing for him. Meantime we shall go plodding on as best we may, even if we are thought tenfold more 'tedious' by our brilliant colleagues, in the future than in the past. Besides all this, we believe that the *science* itself, which is more than any Lavoisier, is with us now; and that this same patient study of facts, and this earnest but slow pushing forward of the realm of law on the basis of such study, is the very thing in behalf of which the bells should be rung, if they are to be rung at all.

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THE BEARING OF THE AFTER-IMAGE.

A few years ago Dr. Carl Hess examined carefully the change of tone produced in a given spectral color by the circumstance that the eye had already been fatigued by gazing at another color. The experiment was conducted in the following manner. The observer looked into the telescope of a suitably arranged spectroscope, and saw in one half of the field a given color, the fatiguing color ; by fixating a minute bright point in the centre of the field, the effect of this color was confined to one half of the retina. After a certain lapse of time (varying, in the different experiments, from 30 to 75 seconds) another color, the reacting color, was allowed to fall upon this same half of the retina, and upon the other, unfatigued, half such a spectral color as the observer found, by moving the collimator-tube, to match it exactly. The wave-lengths of the three color-tones, the fatigue-color, the reacting-color, and the comparison-color, were all taken ; hence the experiment would seem to be of a commendable degree of accuracy, and not to consist only of *schwankende Schätzungen*, to use Helmholtz' phrase in criticising it.

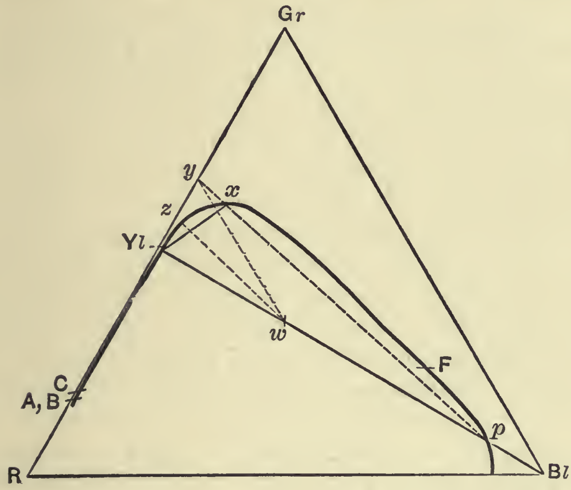
A large number of trials were made, of which the following may serve as examples :

After looking at red, violet becomes bluish green, green becomes greenish blue.

After looking at violet, red becomes reddish yellow.

After looking at blue, red becomes reddish yellow (more yellow than above).

It will be seen that in each of these cases (as happened in all the cases tried) the effect is the same as if the reacting color were mixed with a very considerable amount of the color complementary to that first gazed at. Now this *fact* is nothing new for Helmholtz ; he mentions it explicitly in the first edition of the *Physiological Optics* (p. 368). The question is whether the explanation given by Helmholtz of the after-image which causes the result is sufficient to account for the amount of change produced. The situation can be made plain with the aid of a diagram representing König's color-triangle. Suppose that fatigue has taken place for the fundamental blue, and that a slightly yellowish green, x, is then looked at. In accordance with the principles of the color-triangle, the effect upon x of a diminution of the blue contained in it is found by drawing a line from Bl to x, and producing it beyond x. But if this line be produced to the very border of the color-triangle,—that is, to the very limit of sensations ever experienced; in other words, if the power of the retina to produce the blue sensation is completely exhausted, the sensation which results cannot be beyond that represented by y, whose color-tone, found by joining y to w, differs very little from that of x. But the actual color-tone observed is found to be somewhere near the line zw,



and quite saturated; hence it can only be accounted for on the hypothesis that there has been a very considerable admixture of a saturated yellow. For Helmholtz, this can only be obtained as a residue of the self-light of the retina after it has been exhausted for blue. For the after-effect of blue upon red the case is still worse, since spectral red contains absolutely no blue. Helmholtz himself distinctly says, in the new edition of the *Physiological Optics*, where he discusses this work of Hess (p. 518), that for a theory in which any of the spectral colors corresponded to only one, or only two, fundamental colors, or for which the self-light of the retina was assumed to be small, the objection would be justified. In his present paper * Hess shows that, even with these conditions, the cause assigned is not sufficient to account for the effect; that even if the exhaustion had proceeded so far that the blue first looked at had turned to gray (what was far from being the case), and if the amount of the self-light was much greater

* *Ueber die Unvereinbarkeit gewisser Ermüdungserscheinungen des Sehorgans mit der Dreifasertheorie.* C. Hess. Archiv für Ophthalmologie, XXXIX. 2, 45–70. 1893.

than is usually attributed to it, it would still be impossible to account for the presence of a yellow of so much color-quenching power.

As already said, the phenomenon here exhibited has already been stated by Helmholtz to occur, and to its full extent. There is a simple experiment of Hering's, also, which ought to become classical, in which the amount of the after-effect is shown in a way which cannot be improved upon: one has only to look fixedly upon a patch of color illuminated by the light of a lamp, and then to partly *turn down the lamp*, when the patch, still looked at with open eyes, will appear to be spread over with the complementary color. This shows beyond question that the after-effect is something of very considerable intensity. While this paper of Hess, therefore, makes no distinct addition to our knowledge of facts, it is still very important in calling renewed attention to the necessity for admitting that when an after-image is produced, some very pronounced physiological process is taking place in the retina. But this does not establish an incompatibility with the Young-Helmholtz theory. Helmholtz at present assumes a photo-chemical process in the retina, the nature of which he does not farther particularize. There is no reason to suppose that, if he found it desirable, he might not add some such assumption as would enable him to assign a physiological basis to the after-image. It is not necessarily a fundamental part of his theory to give so much play as he does to illusions of judgment. They must evidently play some rôle, for everywhere else the principle of the relativity of sensation is extremely important; if Helmholtz proves to have been mistaken in assigning to them too important a rôle as an intensifier of after-images, it is, beyond question, a mistake that might easily be corrected. It is an error of reasoning far too commonly made to suppose that all explanations of phenomena given by the individuals who uphold a theory are an essential part of the theory, and that to discredit them is to upset the theory itself. And in particular there is nothing in the facts adduced by Hess which is incompatible with a 'three-fibre' theory, but only with the explanation which Helmholtz has hitherto given of the after-image.

We have pointed out in the May number of this Review (page 324) that the case is the same for the phenomena of simultaneous contrast; that they are of a physiological nature is not incompatible with the Young-Helmholtz theory (and still less with every three-color theory), but only with the attribution of an unnecessarily exaggerated importance to an admitted effect of illusion of judgment. It is essential to set this forth with explicitness in the interests of sound reasoning, but it must not be taken as giving any appreciable amount of aid and comfort to the upholders of the Young-Helmholtz theory. The single fact that that theory requires us to interpret white (not the physical cause of the sensation, but the sensation itself) as a mixture of red, blue, and green is enough to make it an absolutely impossible theory to the psychologist. So the fact that Hering's theory requires us to attribute equally useful functions to decay and to growth makes that an impossible theory to the physiologist, except as the last resource of an exhausted scientific imagination. But every one who is interested in these theories is either a physiologist or a psychologist. Hence the necessity of substituting for them both an hypothesis free, upon the threshold, from objection of so critical a nature.

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PSYCHOLOGICAL LITERATURE.

SOCIAL PSYCHOLOGY.

The Psychic Factors of Civilization. LESTER F. WARD. Boston, Ginn & Co., 1893. Pp. xxi + 369.

Social Evolution. BENJAMIN KIDD. New York and London, Macmillan & Co., 1894. Pp. vi + 348.

Civilization during the Middle Ages. GEORGE B. ADAMS. New York, Charles Scribner's Sons, 1894. Pp. vi + 463.

History of the Philosophy of History. ROBERT FLINT. New York, Charles Scribner's Sons, 1894. Pp. xxvii + 706.

An attempt to state the foundations of a sociology definitely based upon psychological methods and data has an interest for psychologists quite independent of its worth for students in the social field. This interest is a double one: it is worth while to see what sort of psychological ideas are used to lay the basis of another science, and it is worth while to note the reaction of their social application upon the ideas themselves—to note, that is, how psychological ideas look when handled by one whose chief interest is in their efficiency to explain the development of social life. Accordingly I shall consider Mr. Ward's work on both sides: how in his essay psychology contributes to sociology, and how sociology in his hands supplies valuable data to the psychologist. And if I am led to the conclusion that Mr. Ward gives back considerably more to the psychologist than he succeeds in borrowing from him, the conclusion only adds to the psychologist's interest in the work, however it may square with Mr. Ward's intention.

There are two questions of paramount interest in sociology: one, the question of the nature of the social forces; the other, the question of their control. As it happens, both of these questions are psychical questions. The force which keeps society moving is a psychical one, the 'soul,' using the term *soul* not in a theological or even technical philosophical sense, but in its popular meaning—the feelings taken collectively. The power which gives direction to these forces is also psychical—the intellect. Now, on one hand, according to Mr. Ward, these considerations suffice to overthrow the reigning *biological* method in sociology, as represented by Spencer, in its theory, and still more as to the practical conclusions (*laissez-faire*) drawn from it. As to this—save to suggest that possibly Mr. Ward takes Spencer somewhat more seriously than a psychologist would take him, and to regret that the somewhat irritating self-consciousness of Spencer's style should occasionally have infected Mr. Ward's way of putting things—we have nothing to do. We are concerned with his subjective psychology, or account of feeling as psychical motor, and his objective psychology, or intellect as psychical director.

Mr. Ward's psychology of feeling and action is a compound (not a happy one, as I shall try to show) of the old-fashioned psychology of sensation, dating from Locke, and Schopenhauer's theory of will. The crudeness of his account of sensation and idea may best be gathered from his own words : "When the end of the finger is placed against any material object two results follow. There is produced a *sensation* depending upon the nature of the object, and there is conveyed to the mind a *notion* of the nature of the object" (p. 16). If the sensation is indifferent as to pleasure and pain, attention will be fixed upon the notion conveyed, and abstracted from the sensation. In this case perception occurs. What sort of thing the percept of an object will be independent of the qualitative character of the sensation, Mr. Ward does not try to say : he only tells us that "the sensation and the notion are not one and the same, but two distinct things." This complete dualism (he tells us of the 'dual' nature of mind, p. 12) lies at the basis of his conception of feeling as psychical and social force, and intellect as directing power.

While indifferent sensations are neglected for the notion which they convey, the intensive sensations meet a different fate. Pleasure and pain are connected with them, and this fact occasions movement : movement which is definite and purposeful *—away from object when there is pain, towards it when pleasure. These acts are the simple impulsive movements. But besides this sensori-motor apparatus, there is an ideo-motor apparatus, which gives rise to rational acts. These acts, Mr. Ward asserts (p. 33), come as clearly as the sensori-motor within the generic definition of being the result of sensations and away from pain and towards pleasure. This may be true ; but how it can be true with-

* If this be true in the unqualified way in which Mr. Ward states it, it is difficult to see why the intellect should ever be needed to 'direct.'

out a complete reconstruction of his original dualism between the 'subjective' sensation and the 'objective' idea, I fail to see, the 'idea' having been defined as wholly without pleasurable or painful quality.

Desire is the next stage of development, and is 'the recorded and remembered pain and pleasure.' Since the representative states are much more important in our life than presentative sensations, our whole being becomes a theatre of desires seeking satisfaction, but checked in many ways, so that there results a perpetual striving to obtain the objects of satisfaction. From this time on the psychology of will completely supersedes that of sensation: the appetites of hunger and thirst, love, æsthetic and moral cravings, all springs to action, are included under desire, and language is strained to exhibit "all animated nature burning and seething with intensified desires" (pp. 52–3). We are next told that all desire is a form of pain, while effort aroused by desire is simply to satisfy it, that is, allay the pain. "All the enormous exertions of life are made for the sole purpose of getting rid of the swarm of desires that goad and pursue every living being from birth to death" (p. 55). That a remembered pleasure as well as a remembered pain should be of itself desire, that it should be pain (not simply painful), and that of itself it should know how to terminate itself, and that this termination should be pleasure—all this will probably strike the psychologist as curious enough; but the end is not yet. The satisfaction of desire terminates it, and the subject returns, psychologically, to its previous condition. But this of itself leads to the pessimistic conclusion: the sole spring to action is desire, desire is pain, and the satisfaction of desire is simply the cessation of pain. Yes, replies Mr. Ward, all this would be true if the act of gratifying a desire were absolutely instantaneous (p. 65). But the sensation of gratification is continuous; it takes time; in the higher form of love, indefinite time. "So long as the object is present the pleasure abides" (p. 68). Now I do not intend to question this as a fact; but, again, I do not see how the statement can be true if Mr. Ward's previous psychology be true. All gratification of desire implies the presence of desire; a non-existent desire can hardly be gratified, and all desire is pain. *Ergo*, as long as there is gratification there is pain—at most a mixed state of pleasure-pain. This is Mr. Ward's only logical conclusion. The *object* whose permanence gives permanent satisfaction is a visitor from another sphere than that of sheer feeling which forms, with memory, the whole of Mr. Ward's data. The contradiction becomes oppressive when we are further told (p. 74) "that, provided the means of supplying wants can be secured, the greater the number and the higher the rank of such wants, the higher the state of happiness attainable."

While feeling (pleasure) is the result of desire *psychologically* (or for the individual), *function* is the result so far as nature is concerned. The satisfaction of the desire to eat builds up the whole system's further structure, and that develops organic function. There is still another result, *totally* (p. 79) different from either feeling or function. In satisfying desire the individual puts forth *action*, and this is a condition of building up structure. It is the connecting link between pleasure and function—the consequence of the former, the condition of the latter. The transformations thus wrought constitute material utilities, material civilization. Of these neither the individual nor nature is the beneficiary, but society. Thus there are three distinct ends—function for nature, pleasure for the individual, and action, with its products, for society.

I mention these points for their negative rather than their positive value. All these separations, with the contradictions previously indicated, result logically from the original premises. Let the fundamental thing be conceived as impression resulting from contact with an object, and thought, perception, must be another sort of thing; desire and action can be brought in from passive feeling only by a virtual contradiction, while nature, the individual, and society have independent ends.

For, to begin with the last point, it is simply the insertion of a passive impression between the 'object' and the feeling and idea that makes such a break in the respective ends of nature, individual, and society as Mr. Ward introduces. Let once the standpoint of *action* be taken and there is a continuous process: the sensory ending is a place, not for receiving sensations and starting notions on their road to the mind, but a place (viewed from the standpoint of nature) for transforming the character of motion; the brain represents simply a further development and modification of action, and the final motor discharge (the act proper) the completion of this transformation of action. Whether the discharge is sensori-motor or ideo-motor depends simply upon the intermediate transformation which the original motion undergoes. Now while the psychological description of the process may employ different terms, it cannot involve a different principle. To suppose that feeling starts off action attributes a causal power to a bare state of consciousness at which many of the 'metaphysicians,' before whom Mr. Ward so shudders, would long hesitate. What feeling adds is consciousness of value of action in terms of the individual acting. While this appreciation of value marks a tremendous factor in the development of life, it is altogether too much to suppose that its introduction means the introduction of a new agency: the abdication of 'natural' energy (motion) and the substitution for it of a new power-feeling.*

Furthermore, there is no reason to make function the 'end' of nature: its 'end' (like its beginning) is activity, or motion; the structural organization (and the corresponding functioning which goes along with it) being simply the objective manifestation of the transformation of motion. Even from the standpoint of 'nature,' function (or rather structure, which I take Mr. Ward to mean, since function always *is* action) is instrumental, not final. Only because Mr. Ward tries to get action out of passive states of feeling (pleasures and pains) does he have to reverse this natural order, and make action the intermediate term between feeling (the individual's end) and function (nature's end). Once adopt the united and continuous standpoint of action, and our three different ends resolve themselves into one—an end which may be termed valuable (felt) functional activity.

It probably is hardly necessary to deal at length with the weakness of Mr. Ward's treatment of original and representative action. The ignoring of impulse, save as representative, or the memory of previous pain and pleasure; the reduction of both ideo-motor and sensori-motor action to response to feelings of pain and pleasure, leaving out of account both the qualitative side of sensation and ideas, and also the connection of sensation (directly) and ideas (indirectly) with impulse; the account of desire as representative pleasures, which are suddenly asserted to be a state of pain; the abrupt appearance of permanent objects of satisfaction—all this is its own sufficient commentary.

When, however, we remember that Mr. Ward's original text is the need of relatively less attention to the intellect and more to the motive side of mind, and that his object is to get a basis for social dynamics on the side of its motor powers, we have an instructive object-lesson. All this unsatisfactory and self-contradictory analysis results from the fact that Mr. Ward is so under the spell of an old psychology of sensation that he fails to recognize the radical psychical fact, although just the fact needed to give firm support to his main contentions—I mean *impulse*, the primary fact, back of which, psychically, we cannot go.

* It may avoid misapprehension here if I remark that I am not arguing that the 'external' motion is the cause of the 'internal' state of consciousness. To treat one as *cause* of the other is to suppose one independent of the other, and thus to break the continuity. My point is, that if one chooses to take the standpoint of physical science and describe as far as possible the psychical occurrence, this occurrence is one of the transformation and complication of motion. The fact of feeling and of the existence of ideas must be recognized, but they must be treated from the standpoint of the development of action.

Starting with impulsive action, Mr. Ward would have, I think, no difficulty in showing the secondary or mediate position occupied by intellect. In order to secure this, his main purpose, he could well afford to sacrifice both the theory of feelings of pleasure-pain as stimulus to all action, and the old myth of sensation somehow walking from the object over into the mind. He would secure both a consistent psychology and a unification of the ends now attributed to three different existences by a psychology which states the mental life in active terms, those of impulse and its development, instead of in passive terms, mere feelings of pleasure and pain.

It is a pleasure to turn from these somewhat negative results to the other field—the light which Mr. Ward throws upon psychology from the standpoint of sociological evolution. I must omit more than bare reference to Mr. Ward's account of the reaction upon environment resulting from the introduction of specialized psychical phenomena. The points he makes (pp. 84–89) regarding the effects upon vegetable life in the way of the evolution of flowers and fruit, of the appearance of mind (in insect and bird organisms), and concerning the effect upon physical characters, including the brain, of the male animal of the development of sexual appetite in the female, are well worth attention.

But Mr. Ward's main contribution in this direction is in the theory which he propounds regarding the growth of intelligence, and the differentiation of the male and female types. It would perhaps hardly be safe to say that there is anything absolutely original in the points urged by Mr. Ward, but I do not know any writer who has made them in so striking and effective a way.

The key-word to the whole evolution of mind is *advantage*. Gain consists in increased ability to satisfy desire ; hence the arousing of direct effort, of that striving which we call brute force. But many desires cannot possibly be satisfied by the primary method of direct effort. When a desire having a certain amount of active vigor at command meets obstacles, the result is that the animal is no longer simply checked ; while external motion is arrested internal motility is increased. In this way the animal may continually change its position or point of attack, and thus by an indirect or flank movement finally reach its goal. This advantageous method would be selected and perpetuated until, finally, the power of mental exploration is developed. This incipient power leads up to 'intuition,' defined as the "power of looking into a complicated set of circumstances, and perceiving that movements which are not in obedience to the primary psychic force are those which promise success."

Intelligence is thus indirection—checking the natural, direct action, and taking a circuitous course. This accounts for the touch of moral obliquity attaching to all words naming primitive intellectual traits—shrewdness, cunning, crafty, designing, etc. It also accounts for the large part played by deception in historic social life—military strategy, political diplomacy, and, at present, business shrewdness. It is the legitimate consequence of this stage of mental development. So far as nascent intelligence is directed towards other sentient organisms (as it is where the getting of food or avoiding of enemies is concerned), intelligence is egoistic, living at the expense of other organisms. But a further development takes place when it is directed to inanimate objects. Ingenuity is substituted for cunning, and in so far intelligence becomes objective, impersonal, disinterested. When the savage makes a bow and arrow, his ultimate aim, indeed, is still gratification of appetite ; but for the time being his attention must be taken up with a purely objective adjustment—with perception of relations of general utility, not of simple personal profit. In this way intelligence gradually, through the mediation of invention, works free from subjection to the demands of personal desire. It sets up its own interest, its own desire, which is comprehension of relations as they are. Scientific discovery and speculative genius are simply farther steps on this same road.

The ordinary biological theory of society does not see beyond the egoistic, exclusive development of intelligence. Its practical conclusions are, therefore, all in the direction of *laissez-faire*. But a psychological theory must recognize the change in the conditions of evolution wrought by the development of the non-personal, objective power of intelligence. True legislation is simply the application in the sphere of social forces of the principle of invention—of objective co-ordination with a view to increase of efficiency, and preventing needless waste and friction. Given a social science and a psychology as far advanced as present physical science, and *laissez-faire* in society becomes as absurd as would be the refusal to use knowledge of mechanical energy in the direction of steam and electricity. Mr. Ward, however, does not hold that psychology justifies the extreme socialistic conclusion, but rather leaves action a matter of specific conclusion : Let society do as the individual does—do what seems best after detailed study of the relevant facts. This seems good sense, but I doubt if Mr. Ward has duly considered the possibility of this outcome if, as he has previously urged, society has one end, viz., action, and the individual has another, feeling. If this opposition of ends exists, any possible development of intelligence can, it seems to me, only bring the conflict into clearer relief, and bring out definitely the necessity of choosing whichever is considered more important and sacrificing the other. In other words, what is needed is not the substitution of a psychological theory (in terms of individual feeling) for the biological theory (in terms of function), but rather an interpretation of the latter into its psychological equivalents—a theory of consciously organic activity.*

At an early period a differentiation into two main types of intuition occurs : male, whose course we have already followed, and female. Male intuition develops with reference to reaching remote ends ; it works out means ; it is essentially planning or contriving. It develops new schemes, etc. Female intuition develops with reference to the immediate present ; it is a question not of getting food at a distance, against obstacles, but of protecting herself and young against present danger. Female intuition develops, therefore, in the line of ability to 'size up' the existing situation ; it reads signs : it is essentially interpreting, not projective or contriving. This seems to me the nearest approach yet made to putting the psychology of the sexes on something approaching a scientific basis. When Mr. Ward goes on to argue that the male intelligence is radical, the female conservative, I cannot follow him so unreservedly. It seems to me that both the facts and a legitimate deduction from his own theory justify the conclusion that the male intelligence is radical as to ends, but cautious as to immediate methods to be followed—that is, while entertaining new projects easily, is slow in coming to a conclusion as regards their execution. The peculiar abstractness of the male intelligence results from this combination. The female intelligence, while hesitating in the consideration of radically new ends, is decidedly radical in its adoption of means with reference to ends—its tendency is to take the shortest course, irrespective of precedent. The prevalent theory of the essentially conservative nature of woman's intelligence seems to me a fiction of the male intelligence, maintained in order to keep this inconvenient radicalism of woman in check.

I cannot conclude without adding that Mr. Ward's book is extremely suggestive—as well for what it does not accomplish as for

* Before passing on to the next topic, I wish to remark that Mr. Ward's general theory of the evolution of intelligence seems to me to promise a much more hopeful reconciliation of the *a posteriori* and the *a priori* than Spencer's method. The 'raining in' of an external environment upon the organism until its main features are reproduced in the organization of the latter offers more difficulties than it solves. From Mr. Ward's standpoint, the development is always controlled by the organism itself—it occurs in the process by which the latter reaches its own end, and in that sense (probably the only tolerable one) is *a priori*, while the whole process is itself an experimental one.

what it does. Its moral (to my mind) is pointing to a step which the book does not itself take. The current theory of mind undoubtedly needs reconstruction from the sociological standpoint ; it needs to be interpreted as a fact developing with reference to its social utilities. The biological theory of society needs reconstruction from the standpoint of the recognition of the significance of intellect, emotion, and impulse. Mr. Ward seems to me, when all is said and done, to give a compromise and mixture of the two older standpoints, rather than a re-reading of either of them.

Three ideas run through and through Mr. Kidd's book, repeated and intertwined without much regard to the logic of formal presentation, and yet so put each time as not to convey the effect of wearisome reiteration. These ideas are : I. Progress is always effected through competition and struggle. There is infinite narrow variation, some variations tending slightly below, others slightly above, the existing average standard. There is in these variations no essential tendency to progress. Progress comes only through selection of favorable differentiations, and there is no selection save where there is rivalry and struggle. This biological law (with regard to which Dr. Kidd follows Weismannism in its extreme form) holds of human as of animal history. Its scene of operation is simply transferred to the rivalry of nations and of industrial life.

On this point Mr. Ward and Mr. Kidd seem to me to provide necessary correctives of each other. The positive evolutionary significance of conflict seems hardly to be recognized by Mr. Ward ; he seems to think that intellectual progress can now cut loose from the conditions under which it originated, namely, preferential advantage in the struggle for existence. To me it appears as sure a psychological as biological principle that men go on thinking only because of practical friction or strain somewhere, that thinking is essentially the solution of tension. But Mr. Ward is strong where Mr. Kidd appears defective : in the recognition of the part which coherent, organized science can play in minimizing the struggle, and in rendering effective that residuum necessary to maintain progress. The elimination of conflict is, I believe, a hopeless and self-contradictory ideal. Not so the directing of the struggle to reduce waste and to secure its maximum contribution. It is not the sheer amount of conflict, but the conditions under which it occurs that determine its value. Mr. Kidd seems practically to ignore this possibility of increasing control of conflict, and to leave the individual at its mercy ; the individual, according to him, is a tool of the conflict in evolving progress, not the conflict a tool of man.

This brings us to the second point. II. Progress implies the sacrifice of the individual to the race; the individual has to suffer from the conflict in order that the race may enjoy the benefits of progress. This position of itself offers nothing new; the problem has been felt ever since man became conscious of progress. The contention between Herder and Kant in Germany, between Malthus and the 'perfectionists' in England, represent it. But the use to which Mr. Kidd puts the idea is, so far as I know, original, and marks a mind of scope and daring. As man becomes conscious of the extent to which he is sacrificed to a progress in whose benefits he does not share, and as he gains in rational power, he will squarely propound to himself this problem: Why should I continue to suffer simply for the sake of progress? Go to; let us make the best of the present and eliminate struggle and conflict. And from the standpoint of reason this position is logically justified; there is no *rational* sanction for progress. This is the psychological basis of socialism, for socialism is simply extreme rationalism applied to the existing conditions of life. It proposes to put a stop to the suffering which struggle inflicts on individuals; though this implies a brake on progress.

III. Where then is the sanction for progress, science, or rational method utterly failing to justify it? In feeling subjectively, or religion objectively. The sociological function of religion is to cultivate in the individual passive resignation to or even active co-operation in his sacrifice to the good of future generations. Only in this way can the universality, historical and psychological, of the religious consciousness be explained. The scientific man in his ignoring of, or attack upon, religion fails to notice this sociological, evolutionary meaning, and indirectly plays into the hands of the socialist.

I have given, I think, a fair account of Mr. Kidd's main intentions; what I have not given is his force of statement and his wealth of illustrative material. Any detailed criticism upon such radical and far-reaching propositions is out of the question, but I cannot refrain from two suggestions. If the individual is *continually* sacrificed to the conditions of progress, where is the progress? Mr. Kidd speaks as if sacrifice to progress and sacrifice to welfare of future humanity were the same (see p. 291). But this cannot be; the benefit which will accrue to the future generations must, when their turn comes, be incidental to the sufferings attendant upon conflict as a condition of further progress. The process never amounts to anything, never has any value, *unless it has it both now and then*, i.e., all the time. Mr. Kidd seems to me to have fallen into the old pit of a continual progress *towards* something. This indicates my second suggestion. The antithesis which Mr. Kidd makes between what constitutes the happiness of the individual and the conditions of progress appears to be overdrawn and out of perspective. Overlooking the fact that the sense of contributing to progress is an important, and to many an indispensable, rational ingredient of happiness, what ground is there for the assumption that the individual's rational conception of happiness excludes all suffering arising from struggle? I do not see that the case stands otherwise for the conditions of happiness (individual welfare) than for the conditions of progress (general welfare). A certain intensity and, so to speak, tautness of activity appear requisite to happiness; and rivalry or struggle, for anything we know, is as constantly necessary to keep us strung up to the proper pitch for happiness as it is to afford the conditions which enable preferential selection (progress) to act.

All this is upon the supposition that Mr. Kidd is justified in his extreme Weismannism of premise. If we suppose that consciously acquired activity, and habits formed under the direction of intelligence, are conserved, the case against his point is much strengthened. While struggle and consequent pain are not eliminated, the vibration is so loaded by established habits as to lessen its range. There is even no need to suppose that the conservation of rationalized activity is direct or through the organism; if the environment is so changed as to set up conditions which stimulate and facilitate the formation of like habits on the part of each individual, the same end is reached.

I hope it will not seem an injustice to Professor Adams's lucid and substantial piece of work if, after having called attention to its helpfulness to students of intellectual as well as of political development, I use it to point a moral for psychologists. As giving an adequate and coherent account of the general conditions and movement through the middle ages, the book is highly valuable to any one who is trying to understand the philosophy of that period. But from a narrower psychological standpoint the value is, in the main, negative. I mean that it indicates the slight extent to which psychology has as yet penetrated into the sciences which lie nearest to it, the historical. Psychology has not as yet made of itself a generally useful tool; it has not impressed the worker in other fields so that he feels the necessity of keeping his eyes open for the psychical development, the growth in consciousness; nor does it give him much help when he does attempt this. To take one point: Professor Adams recognizes clearly the great significance of the middle ages in discovering the individual and bringing him to the light of day (pp. 91, 92). But this is treated mainly as an objective change—a change in political status. The extent to which this depended upon a changed psychical attitude, and the part played by the implicit and explicit psychological theory of mediæval thought

—all this does not meet recognition. And yet this seems the key to understanding the outer transformation. Now this, of course, is no reflection upon the historian ; he cannot be expected to stop historical investigation in order to make for himself adequate psychological instruments ; it is, once more, a warning and a stimulus to the psychologist.

It is hardly necessary to do more than to call attention to Professor Flint's noble beginning of a monumental work. The present volume (of 700 pp.), after an Introduction dealing with Greek and Roman speculation upon history, is devoted to the philosophy of history in France, and we are led to anticipate further volumes upon England, Italy, and Germany. I cannot pretend to have the knowledge required to speak critically of this book ; indeed, so wide is its range and so thorough its treatment that I do not see that anybody but Professor Flint himself is competent to speak of it as a whole. It seems, however, to have all, and more, of the solidity, accuracy, and restraint of judgment which marked the older volume upon Germany and France. To risk one more *obiter dictum*, judging from the accounts of authors with whom I have some acquaintance, as Rousseau and Comte, the book is more likely to be serviceable as a statement of the facts of the case than as an account of the underlying *motif* and trend—as interpretation, in short. But this simply means, I suppose, that I should interpret it otherwise myself.

J. D.

Pain, Pleasure, and Æsthetics. An Essay concerning the Psychology of Pain and Pleasure with Special Reference to Æsthetics. By HENRY RUTGERS MARSHALL. Macmillan & Co., London and New York, 1894. Pp. xxi + 364.

This book, parts of which are already known to readers of *Mind*, contains at least two notable contributions to psychology. One is a new formulation of the differentia of the æsthetic judgment, a new definition of beauty. The other is a physiological hypothesis about the basis of pleasure and pain, together with the deduction from it, by way of verification, of various æsthetic laws. The reader stands in some need of emphasizing these chief points and of sustaining his interest by keeping them in mind, because the book is not compactly arranged and there are some arid stretches to traverse. One cannot help regretting that there is so little pruning and illustration, and so many phrases like 'the indifferent state of sleep-unconsciousness,' * 'joy-pleasure,' † 'non - expectation painfulness,' ‡ and 'pleasure - pain toned.' § But it must be confessed that in this scorn of the graces

* p. 327. † *Id.* ‡ p. 235. § p. 237.

Mr. Marshall follows good precedents. Philosophers writing about beauty have seldom made their books an illustration of their subject.

The attempt to define beauty objectively, we are told, has failed. No characteristic can be found common to all beautiful things except the power of pleasing us. In this relation to our pleasure, then, we are to see the source of æsthetic values. Æsthetics thus appears as a subdivision of hedonics, and the question is now only to find the differentia which distinguishes æsthetic pleasures from those of other kinds. And here Mr. Marshall makes a distinction which he advances with his accustomed modesty, but which we are inclined to regard as one of the most valuable points in the book. It marks a rare freedom from the convention and routine of abstract thinking and a fresh perception of psychological fact. The distinction is one between the æsthetic impression—the beauty which we see and feel in the moment of perception—and the æsthetic judgment—the beauty which we attribute to the object upon subsequent reflection. Into the first enter all the pleasures we may be having at the time; there is none which does not merge with our enthusiastic recognition of a present beauty and which does not add warmth and sincerity to our delight in the object before us. But the parts of this psychosis are not equally re-presentable. Some will never be recalled, and the pleasure in particular which forms a part of our consciousness may be detached entirely from the object when this object recurs in the memory. The memory of things pleasant is often painful, and the memory of things painful pleasant. When, therefore, we are called upon to pronounce a deliberate judgment on the æsthetic value of a remembered object, we shall no longer find the same pleasures in our mind to form the source and substance of our approval; some of these pleasures will surely have disappeared, and only some, if any, will remain. Of these pleasures which recur with the revival of the object our permanent judgment of beauty must be made up. Those pleasures are therefore æsthetic which are relatively permanent in revival.

Striking as the distinction is between æsthetic experience and æsthetic opinion, the definition of beauty founded upon it seems open to a double objection. In the first place, granting that all concomitant pleasures add to the beauty of present objects, there remains a clear distinction between the sense of pleasure and the sense of beauty. If, for example, I have no definite object before the mind, but am lost in a torpid reverie, I might still have pleasure; but this pleasure would not be æsthetic, because I could not perceive any beauty, seeing that no object is present to me in which that beauty may reside. Our author seems to make the distinction between pleasure and beauty impossible for direct perception, where it is nevertheless clearly found. He thinks that a pleasure is always æsthetic if it is attached to an image in the memory. But here again the facts stand in the way. For few pleasures are so vivid in revival as those of satisfied vanity, affection, revenge, and other personal passions; it is easier to summon them up than to recall the feelings produced by a work of art. In fact, many of our strongest passions are ideal, and feed and grow upon imagination, while taste can establish itself only by constant renewals of observation, shifting tests of sense, and attentive return to the reality of experience.

If pleasure is the source of beauty and æsthetics a department of hedonics, the nature of pleasure is evidently a matter of fundamental importance for the æsthetician. Mr. Marshall accordingly devotes the greater part of his book to the discussion of the nature and physical basis of pain and pleasure. He examines and rejects the classification of pleasure and pain as sensations and as emotions, and defines them himself as qualities either of which may, and one of which must, belong to every perception of the mind. That is to say, there is nothing in the nature or character of any idea which makes it essentially agreeable or disagreeable : the object may remain the same while its value changes. But some value, negative or positive, it must always have, and must affect the mind with some tinge of pain or pleasure. This part of the discussion, which occupies the first chapter, turns, it seems to me, upon a matter of words. Introspection cannot distinguish perceptions from the qualities of perceptions. The same fact, for instance, is described equally well by saying that color is a quality of extension, or that they are two simultaneous perceptions, or that they are both qualities of a present substance. The question becomes real and not verbal only in the field of physiology. If one organ gives the idea of extension alone and another organ that of color alone, then the perceptions are separable ; we have but to inhibit the action of one organ to prove it. And so it seems to be also with pleasure and pain. If there are specific nerves that yield these feelings and nothing else, they are independent sensations ; if every nerve, as Mr. Marshall believes, yields a further sensation which may be tinged now with pleasure and now with pain, according to the circumstances of its excitement, then pleasure and pain are qualities of sensation, incapable of appearing by themselves. Apart from physiological evidence, which is not here presented, the discussion of such a point seems to be futile.

We pass at once, then, to the author's physiological hypothesis. It is that when an organ responds to a stimulus by the discharge of surplus stored force the activity gives pleasure ; when the stored force is inadequate to meet the demands of the stimulus we have pain. The significance of this formula evidently depends upon the fixing of the idea of the *normal* reaction ; we must know how much force is necessary, and just where the storage of surplus force begins. To say that the stored force was inadequate when we feel pain, and that it was superabundant when we feel pleasure, would be to fall into a vicious circle. Mr. Marshall is perfectly aware of this danger, and seeks to avoid it by an appeal to habit. The processes of nutrition go on at a given rate ; the stimulus may cause a discharge that will practically balance the nutrition, and in this case we have practical indifference in consciousness. If the stimulus falls short of this strength, the discharge will not counterbalance the nutrition, and there will be a storage of energy in the organ ; this is the surplus force. If the stimulation is now increased to more than what it was in the first and normal case, we shall have pleasure, because the organ will be ready to meet this unusual stimulus with its stored energy. Had this stronger stimulus, however, been applied before the storage of the surplus energy, the result would have been pain ; for the organ would have had no surplus on which to draw, and would have found the stimulus excessive and ultimately destructive. An unusual demand need not always produce destructive action, because it may attract increased nutriment to the organ affected ; and by this increase of the rate of nutrition the standard of normal action will be raised, and the given stimulus will cease to be excessive. In this way we understand how pain by its very nature tends to disappear.

Mr. Marshall's formula thus proves to have a good deal of elasticity and suggestiveness. If it does not wholly emerge, even at the end, from its inevitable vagueness, it is at least developed with great ingenuity, and many facts, at first sight rebellious to it, are explained as its corollaries. The pleasures of relief, for instance, are attributed to the return of activity in organs other than that which caused the pain, organs in which action had been inhibited and surplus energy consequently stored. Introspection surely accords with this explanation, and our author adds one to the strong attacks upon the theory of contrast and total relativity between pleasure and pain, attacks begun long ago by Burke, and which the hasty theory in question never could have withstood had it not been so useful for purposes of apologetics and edification. The pain of constraint is similarly explained by the escape of energy from the overfed organ into the surrounding tissues, which are incapable of responding to this unusual attack. An impressive corroboration is gleaned in this manner from various fields, and further evidence is found by the deduction of certain æsthetic rules from the formula. Of these rules the one which there is, perhaps, the greatest present need of inculcating is that which demands the exclusion of pain. "No one who desired to produce an æsthetic work would think of giving it such a form that its appreciation would be dependent upon the holding of one's breath or upon . . . hunger or thirst."* A laborious age that feels the need rather than the presence of æsthetic values is apt to make its art too difficult, and to add so much spice to beauties that they lose their savor. Mr. Marshall, however, is not insensible to the value of discords. He tells us at the end that we should not wish pain out of our lives, because it is a condition of growth and of "elemental partnership in the higher progress of the wider entity."† These last pages are the only place (and none could surely be more fitting) where he gives vent to metaphysical yearnings; indeed, many a reader of philosophical æsthetics will thank him for nothing so much as for the tone and temper of his book, for his simplicity and candor, and for his introduction of a patient and scientific spirit into a realm so largely the prey of whim and dogma.

G. SANTAYANA.

HARVARD UNIVERSITY.

Basal Concepts in Philosophy. An Inquiry into Being, Non-being, and Becoming. ALEXANDER T. ORMOND. New York, Charles Scribner's Sons, 1894. pp. viii + 308.

This book is much more an essay at a system of metaphysical philosophy than a psychological treatise. It treats, however, of 'concepts,' and the genesis and nature of concepts are psychological inquiries; while the author admits (p. 300) that all the 'philosophical categories' so called are originally, as it were, psychological. The 'psychological categories' must be 'translated from subjective to objective universals' by being passed through the historic medium and embodied in the experience of humanity.

Professor Ormond starts out with what he calls 'the Norm,' or category which holds the supreme content of man's reflective thinking; and which he finds to be the concept of the Logos understood as conscious personal self-activity. In successive chapters he traces the development of the 'moments' of this concept, beginning with 'Being and Non-being,' and then passing on to 'Becoming,' 'Space and Time,' 'Nature' (Cosmic, Organic, and Psychic), 'Consciousness,' and 'Morality,' in a manner not without plain suggestions of the Hegelian dialectic. Chapters on History, Religion, Art, Knowledge, Logos, God, and Spiritual Activity follow, showing how comprehensive is the

* p. 307.

† p. 359.

range of philosophical subjects which the author aims at least briefly to cover.

The distinctive feature of the entire volume is the treatment given to the concept for which the term 'Non-being' is employed. This attempt to establish a better theodicy by a readjustment of this category may be best described in Professor Ormond's own words (p. 302). "It may be maintained with Hegel that the highest category is an absolute idea which comprehends the dual moments, being and non-being, within itself. To this we may yield a qualified assent, provided this idea be translated into spirit and its dialectic be conceived as on its affirmative side, self-affirmation, but on its negative side the denial of its opposite. The reform in Hegelism which has been urged throughout this inquiry may be expressed in the following statement: Being must be identified with spirit. The inner movement of spirit is a dual dialectic in which spirit asserts itself and denies its opposite. The dual movement is thus immanent in being. But the negative which spirit denies is not in being. It is an oppositive excluded conception, which spirit ever wars against and suppresses, but which never passes into its opposite."

We have the greatest respect for all serious attempts to sound the abstruser problems of philosophy to their lowest depths. We disclaim all fear and dislike of metaphysics, but rather wish to see it more diligently cultivated in the future as the only cure for its own worst ailments, the only means of supplying its obvious defects in the past. And we give Professor Ormond respectful credit for his serious and patient essay in metaphysical thinking. At the same time we are more than ever before persuaded that concepts are nothing and can do nothing; can neither, of themselves, count nor account for anything. They are all only mental representations that result from the combined activity of imagination and intellect, and must correspond to actual and concrete existences either immediately known or inferred. Now 'non-being' is no such concept; it is simply a word standing for the refusal to think, or the limit of thinking. It can therefore not be 'warred against,' or 'suppressed,' or constitute one 'moment' in a dual dialectic. Much less can it account for the existence of any form of evil, or constitute either a background, or a barrier, or a reactionary opposite to the will of God. G. T. L.

Inductive Psychology. E. A. KIRKPATRICK, Instructor in Psychology in the State Normal School, Winona, Minn. Jones and Kroeger. Pp. 104.

This little work is an introduction to the study of the intellectual powers by the inductive method. It consists of descriptions, examples, and questions primarily used by the author in his own instruction, but designed also to enable teachers and others "to pursue the subject understandingly either in books or in daily life, and in the school-room." In the belief that the 'thoroughly-dried specimens' of the ordinary text-book obscure rather than elucidate psychological problems, the attempt is made to lead the student through his own inquiries to a clear comprehension of mental phenomena. Mr. Kirkpatrick writes from the standpoint of recent psychological theory, and is very successful in his introduction of the new results in connection with elementary instruction. It is inevitable, however, that so brief a sketch should include not a few conclusions which more detailed discussion would find open to question; and it is to be regretted that some phenomena deemed fundamental by most writers have received so meagre consideration. A discussion of attention with almost no reference to the influence of feeling, of perception with but a bare sentence or two on localization, of the representative faculties with time-perception unexplained, seems hardly sufficient for even an elementary work. The examples or 'specimens' for study show a similar blending of success and failure. Many are very happy; others will hardly replace those now current.

A. C. ARMSTRONG, JR.

WESLEYAN UNIVERSITY.

La fatigue intellectuelle et physique. A. MOSSO. French translation by P. LANGLOIS. Paris, Alcan, 1894. Pp. 192.

In this popular book Mosso gives a lively and interesting description of a number of experiments and observations which he has made on fatigue. Part of his book is purely anecdotal, and need not be dwelt upon. He has studied the fatigue of birds (quails which have come into Italy from Africa across the Mediterranean, and carrier-pigeons), of sailors, of soldiers, politicians, professors, etc., by means of the ergograph, an instrument invented by him and described in this book. The results given are a popular statement of earlier papers published in various journals.

Les caractères. F. PAULHAN. Paris, Alcan, 1894. Pp. 240.

It is singular that all the studies of character which have been published heretofore, at least to my knowledge, have been *a priori* studies, mainly devoted to classification. To study character has been to classify the types of character ordinarily distinguished or classed by preconceived ideas. This work of Paulhan is no exception to this rule. He applies to character the idea already developed in his work on the systematization of states of consciousness. At the end of his book, it is true, he seeks a sort of experimental verification of his conceptions by a study of the character of Flaubert; but this is so vague, so poorly documented, that it would serve any other theory as well. But, criticism aside, this new book of Paulhan contains the keen analysis and breadth of view which we expect from him. We can only give a general idea of his classification. In the first part he passes in review the types produced by the predominance of a particular form of mental activity, i.e., the different forms of psychological associations (eleven types, from the 'balanced' to the 'completely unbalanced'); then the types which arise from the kind of tendencies involved (again eleven types).

In the second part the classification proceeds upon the nature of the object of the tendencies, and the types fall into two groups: those resting upon 'vital' tendencies and those upon 'social' tendencies, the former including the gluttonous, the sexual, and all such temperaments in which visual, auditory, gustative, etc., sensations are prominent. Upon the social tendencies are built up the egoistic and altruistic types, all exaggerations of love, family affection, social and professional tastes, etc. The pages devoted to descriptive work are curious and pleasant reading; but we are forced to confess that we prefer more exact observations.

L'éducation de la volonté. J. PAYOT. Paris, Alcan, 1894. Pp. 276.

This book has a beautiful title—one which an aged physician, an aged educator, an aged priest, who had seen and confessed many people, might write entertainingly upon. But M. Payot is a young man, though he has meditated much and mainly upon himself. As a piece of self-examination, however, the book has value. Indeed, it is much to be desired that many such accounts might be written by persons who are capable of recording the manner of their own bringing up, provided they be not simply moral tales, but precise and practical *psychological sources* from which we may draw lessons in personal conduct.

M. Payot busies himself especially with the growth of the voluntary faculty. He holds that we have a certain power of arrest over our ideas—by making appeal to actual sensations, by going through a regimen of voluntary movement, or by using our organs of speech voluntarily for such a purpose. Against our life of feeling, however, we are more poorly armed. He suggests the possible use of simple medicines as a resource against the onset of emotion, but thinks the source of our weakness in this respect is our loss of time by temporizing. We ought, on one hand, to cultivate associations inimical to the sentiment we wish to dispel, and, on the other hand, to suppress it by inhibiting its expression, by avoiding occasions of its excitement, and by abstaining from its recall in memory. A. BINET.

SORBONNE, PARIS.

Brain Surgery. M. ALLEN STARR. New York, William Wood & Co., 1893. Pp. xii + 295.

To the general reader, Dr. Starr's book is of interest as a presentation of the practical results in medicine and surgery of the vast amount of research of the past thirty years in the field of cerebral localization, and it must be confessed that the impression left after a review of the facts given is rather disappointing. The promises of physiology have certainly not borne fruit to the extent that might have been expected, nor is this lack apparently to be laid entirely at the door of shortcomings in operative technique, for it would appear that in the last few years cranial surgery has been brought to a very considerable degree of perfection.

The opening chapter of the work in question is a general discussion of the diagnosis and localization of cerebral disease, and to the uncritical reader might prove misleading, for from the delightfully brief, lucid, and positive statement regarding the localization of function one would get no hint of the actually very uncertain state of our knowledge on this important point. Such discussion is perhaps only incidental to the real scope of the book, and the fault is a small one, but surely here, if anywhere, dogmatism should be avoided.

To each of the pathological conditions which in his opinion may indicate operation Dr. Starr devotes a chapter. These states are: epilepsy, imbecility due to microcephalus, cerebral hemorrhage, abscess, tumor of the brain, intracranial pressure, and insanity. Of these the results are perhaps most encouraging in the case of tumors of the brain and epilepsy. Of the former, without going into details, the author estimates from a comparison of reports that seven per cent are removable by operation, and from his table of results one finds that of ninety-seven operations recorded, the tumor was successfully removed and recovery ensued in forty-two cases. Regarding operative success in epilepsy, of the forty-two cases collected by Dr. Starr the results were—thirteen cured, eleven improved, fifteen not improved, three died. It must be remarked here that only those cases are regarded as indicating operation where there are distinct symptoms of a localized lesion, which cases would, of course, represent but a very small proportion of the total number of epileptics. On the other hand, the average mortality is but seven per cent for all cases of trephining for epilepsy, which, considering the unfortunate condition of the patients before operation, would seem to justify surgical interference, even with the above results in view.

Of the other pathological states discussed, space does not permit description, but it may be said that the results are generally less successful than those mentioned above.

Notwithstanding the discouraging state of the question at the present time, it is quite possible that with increased knowledge of cerebral physiology and consequent certainty of diagnosis and localization the results will improve; but for this one can only wait. Of matters at present, 'Brain Surgery' may be recommended as a very fair and judicious review. LIVINGSTON FARRAND.

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THE NERVOUS SYSTEM.

The Effect of Stimulation and of Changes in Temperature upon the Irritability and Conductivity of Nerve-fibres. WM. H. HOWELL. (With the assistance of Mr. S. P. BUDGETT and Mr. ED. LEONARD.) Journ. of Physiology, vol. XVI. Nos. 3 and 4.

In view of the uncertain character of our information regarding the nervous impulse, the papers which deal with its modifications are of great interest. In many cases we are able to separate irritability from conductivity in the nerve-fibres, but find, as might be expected, that in the main they vary together.

This paper deals mainly with the question of conductivity. In general, low temperatures diminish conductivity. High temperatures increase it, first rapidly, then slowly, and somewhere about 50° C. this increase gives place to a rapid decrease which leads to its complete abolition. As a rule, return to the normal temperature brings about a rapid recovery, provided that in the case of high temperature it has not been maintained for too long a time.

In the experiments under discussion, a tube carrying fluid, cold or heated, almost encircled the nerve, the temperature of which was controlled by that of the fluid in the tube. The classes of fibres examined, were the motor, the inhibitory fibres of the heart, the vaso-motor fibres, the sweat-fibres, the afferent (respiratory) fibres, of the vagus, and the afferent fibres of the vaso-motor centres. It was observed that cooling the vagus nerve in the rabbit to 15° C. or below very often prevented from passing the impulses which would normally stop the heart. In dogs the vagus might be cooled much more before yielding this reaction. In a dog the vagus nerve at 3° C. conducted impulses which weakened the force of the heart-beat, without affecting the rhythm.

In working with the vaso-motor nerves, it appeared that with changes of temperature the vaso-constrictor fibres lost their irritability sooner than the vaso-dilators.

The interesting feature that these observations have in common is the following: that the nerve-impulse passing a point which is modified by temperature takes on a new character, dependent upon the modification; that is, if it passes a part of the nerve which has been cooled, it is decreased in its efficiency despite the comparatively short part of the nerve which has been modified. In the same way, if it passes a heated portion of the nerve it may become increased in efficiency despite the fact that the remainder of the nerve is in its usual condition. Remarkable, therefore, is the influence of this small modified portion of the pathway on the final effect of the impulse. It is also of interest to note that groups of fibres having different physiological functions react dissimilarly under these conditions.

The susceptibility of nerve-fibres to loss of irritability at the point of stimulation was also investigated. In general, nerves are not susceptible to 'stimulation-fatigue.' Stimulation of the sweat and vaso-constrictor fibres in the sciatic nerves of the cat shows, on the contrary, that the reaction disappeared when the stimulation was continued at one point for even a moderate length of time. That this is due to fatigue at the point of stimulation, is suggested by the fact that moving the electrodes even a millimetre towards the periphery produces the initial reaction in full force. Curious reactions were found upon stimulation of the fibres, causing dilation of the pupil. If the cervical sympathetic, including the fibres controlling the pupil, is stimulated on the central side of a 'block,' the stimulation may continue for as long as 20 minutes; yet upon then removing the 'block' the pupil reacts. This is taken to indicate that a 'stimulation-fatigue' does not occur. When, however, no 'block' is introduced, and the nerve is then stimulated, the pupil first enlarges, but soon begins to diminish, gradually returning towards its normal size despite the continuance of the stimulus, showing therefore complete fatigue. When, however, the electrodes are now shifted to a new point on the nerve, a new dilation occurs, a result which is interesting and difficult to explain, in view of the fact that the first experiment indicates an absence of 'stimulation-fatigue' of the nerve.

The Sensory Motor Functions of the Central Convolutions of the Cerebral Cortex. F. W. MOTT. The Journ. of Physiology, vol. xv. Jan. 1894.

To those who have considered the anatomical bases on which the physiological reactions of the cerebral cortex rest, nothing could have been plainer than the fact that all parts of the cortical mass must be both sensory and motor, if these terms are used with sufficient looseness. We have clearly marked in the cortex a locality in which impulses pass from one nerve-cell to another, and in which the direction of the impulse is changed, for incoming impulses arriving at the cortex travel from the cord towards the brain, whereas those impulses on which the motor reactions depend necessarily pass from the brain towards the cord. This being the case, it was anticipated that injury to the cerebral cortex would give rise to disturbances both of motion and sensation in the opposite half of the body. The disturbance of motion has been comparatively easy to detect; but disturbance of sensation was both more difficult to determine and less constant. Special localities for the dermal sensations have been proposed; and on the basis of experiment, Schäfer and Horsley suggested that the gyrus fornicatus was their centre; but there are good reasons for questioning this result.

So far as the work of Mott is concerned, he inclines to place the termination of the afferent fibres in very nearly the same location as that occupied by the cell-bodies which give rise to the efferent fibres. From the nature of the case the efferent fibres by means of their collaterals may have different terminations, and even in a given locality terminate in a diffuse manner; and, by consequence, very limited lesions which may impair motality do not necessarily impair sensibility to the same degree. Mott's contribution to the subject is briefly this: Monkeys very carefully selected, on account of their sensibility, were operated upon in such a way that the connection of a motor area was severed at some distance below the surface, the severed portion being allowed to remain in place. Loss of motion and loss of sensation were found in the same regions, at the periphery; and the conclusion is drawn that the so-called motor regions are nearly coincident with the localities in which the fibres for the dermal sensations from these parts terminate in the cortex.

The Derivation of the Pineal Eye. Preliminary Announcement. WM. A. LACY. Anatom. Anz., IX. B. 5, 6. 1894.

Within the past seven years a large number of studies have been made on the organ now known as the pineal eye. This small structure is situated in the middle of the head just above the mid-brain, and in some forms of amphibia and reptiles can be clearly seen from the outside. Through the work of de Graaf (1886) it came to be generally accepted as an eye, constructed on the plan found among the invertebrates. Since that time its interpretation as a visual sense-organ, its origin, its connections with the brain, and its functional condition, have been widely discussed.

The observations of Lacy deal with its origin. In the Elasmobranch Fish (*Squalus acanthias*) he has observed in the neural plate, behind the pair of depressions which later form the lateral eyes, two other pairs of depressions similar in character. These two pairs become associated with the mid-brain when the neural tube is formed. The posterior pair disappears, but the anterior persists. The latter forms parts of the roof of the mid-brain, and marks the locality at which the epiphysis grows out. In Squalus the pineal eye is not distinctly differentiated, but the enlarged end of the epiphysis may be homologized with it.

The argument is therefore made as follows : The epiphysis originates from paired structures which in the first instance are comparable with the optic depressions giving rise to the lateral eyes, and so the final modification of the tip of the epiphysis into a visual sense-organ would be in harmony with its early embryonic history. The paired origin appears also to explain the fact that the pineal eye and epiphysis appear in some forms to be distinct structures.

These observations are full of suggestion, and the next step, as the author points out, is to determine whether in other animals the pineal outgrowths have a similar embryonic history. H. H. D.

ANTHROPOLOGICAL.

The New Life: A Study of Regeneration. ARTHUR H. DANIELLS. Am. Jour. of Psychol., VI. 61–103. Oct. 1893.

Mr. Daniells, with many other thoughtful theologians, believes that theology has "lapsed from its high functions," and has "become so conventionalized and rigid that religion itself has often fallen into disrepute and is losing its hold both upon the masses and the cultured classes." But he believes that anthropology, "which is the pedagogical root and very lifespring of theology," has itself entered upon a new stage of development and is now ready "to offer a few ripe insights for the rehabilitation of theology, point by point." "As an illustration of what is thus promised to theology and religion," Mr. Daniells has made a very careful study of the phenomena of puberty, viewed from the anthropological, physiological, and psychological points of view, and endeavors thus to throw light upon the theological doctrine of regeneration. He tries to show "that . . . regeneration is one of the deepest needs of the human body and soul felt among savage and civilized men of all races and times." He attempts "to rebase the doctrine on sound anthropological and psychological principles," hoping thus to "strengthen the pulpit and . . . widen and deepen the sympathy between theology and other sciences."

In pages 63–79 we have a very interesting account of the initiation ceremonies at puberty as practised by sundry peoples, and an attempt is made to point out the significance of some of these ceremonies to those that use them. This shows wide acquaintance with the literature of the subject and is to my mind by far the most valuable part of Mr. Daniells' work. Yet if it is upon the evidence here presented that Mr. Daniells relies to show that regeneration of body and soul is felt as a need by savage and civilized races alike, I cannot see that he has proved his point. It shows indeed that the physical and mental changes of puberty, which might perhaps be termed a regeneration of body and mind, are recognized as a fact by nearly all races. It is possible also to speak of the transition from childhood to manhood as an entrance into a new life. But this is not what the theologian means by regeneration and the new birth, and the recognition of the one most certainly does not prove the recognition of the other.

The physical and psychical phenomena of puberty and adolescence are outlined pp. 79–93. Especial emphasis is laid upon the fact that the obvious development of the sexual impulses is only one aspect of a profound and rapid modification of the whole psychical nature. The intellectual life quickens, the ethical instincts become stronger, the emotional life becomes deeper and more varied.

In this connection Mr. Daniells brings in the thesis which he wishes to prove : "that during the adolescent years there is a necessity grounded in man's nature as a human being, of a new consciousness of man's relation to God, of the conscious choice of Christ as the chief object of his love and service, with an overmastering sense of the duties and responsibilities involved in this ideal life, in order that he may not only be saved from selfishness and the dangers which threaten youth, but also that his newly-awakened capacities and powers may be controlled and used for the development of the noblest character."

The burden of this thesis turns upon the value which we ascribe to the word 'necessity.' If Mr. Daniells merely wishes to say that the acceptance of the Christian scheme is *essential* to the development of the highest type of character, he will find many to agree with him.

But he has not, as far as I can see, adduced any proof of the statement, nor has he shown that such acceptance is based upon anthropological or psychological principles. The period of doubt incident to the intellectual growth of adolescence may indeed end in Christian faith, but it may also end in rejection of that faith, and it does not lie within the scope of either anthropology or psychology to show which is the normal and right end, but of philosophy.

But this is not, I think, what Mr. Daniells means. He wishes to show that the psychical change described by theologians as regeneration, conversion, the new birth, etc., is simply a stage in the normal development of the human soul, a profound psychical modification which usually supervenes at puberty or shortly after and is therefore to be co-ordinated with the numerous other psychical and other changes of the same period. If this be true, he has succeeded in putting the doctrine of regeneration upon a psychological foundation. Mr. Daniells shows us that one is more susceptible to religious impressions in adolescence than at any other period of life, and that the spiritual life, if it is to awaken at all, will probably awaken then. But he does not even attempt to show what is essential to his theory as I conceive it, that this awakening to religious things is natural, i.e. universal, in the same way in which the other changes of puberty are natural. Until that is established the doctrine of regeneration is as far from having a sound psychological foundation as it ever was.

WM. ROMAINE NEWBOLD.

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CHILD-PSYCHOLOGY.

Mental Defect and Disorder from the Teacher's Point of View. JOSIAH ROYCE. Educational Review, vol. VI, 1893, pp. 209, 322, 449.

The author, writing not as physician, but as 'student of human nature,' considers "in a practical way . . . the attitude that a teacher ought to be ready to take toward . . . cases which lie . . . in that wide and ill-defined border-land region which separates the world of the sane from the wilderness of insanity." "What a studious layman can do in dealing with nervously and mentally defective pupils is to supplement his ordinary knowledge of human nature by such psychological insight and reading as will make him keen in watching for the symptoms when he meets them and apt to judge a burdened pupil in a psychological rather than in a merely censorious or moralizing spirit." The teacher may, moreover, learn much that is interesting and important from a study of defectives. "It is in fact the mission of the mentally defective to give us . . . object lessons in psychology."

A condensed prefatory statement of the chief forms of normal conscious activity from the standpoint of 'the empirical psychologist as such,' is further condensed in the statement: "What is all the while going on is the adjustment of this organism to this environment under the influence of these stimuli, and by virtue of these central dispositions or habits,—the adjustment always expressing itself by these motor responses and inhibitions which constitute our more or less highly intelligent conduct." Further: "Our perceptions, imaginations, general ideas, impulses, trains of thought, and lines of conduct are modified and determined, not only by our past habits, but also by the current state of nutrition or of exhaustion, or in general by the transient psychological susceptibility of our centres." "The actual states of nutrition or exhaustion of our nervous centres," as well as various specific and organic sensations, "are more or less immediately represented in our current emotional condition."

The test of the normality of a mental process is "the success of the adjustment of the organism to the environment in so far as this success depends upon the processes that embody the habitual functions of the highest or mental grade of nervous centres themselves" ("slightly modified from Dr. Charles Mercier, *Sanity and Insanity.*") Confining himself to 'border-land cases,' the author considers first those arising in childhood. The hallucinations of childhood may be unimportant, but if they become systematized and recurrent, should be carefully watched, especially if accompanied by other signs of abnormality, as tendency to grow easily delirious, pessimistic moods, very active and precocious wits, symptoms of general physical inability. As to bad nervous heredity: "Respect heredity, but never despair on account of it, since its caprices are endless." In youth 'definable insanities become more common.' The most frequent types of the 'border-land cases' are: (1) Emotional depressions and exaltations, characterized by the author at some length; (2) Emotional dullness; (3) Mental confusion and failing of memory; (4) Lassitude of will; (5) Painful insistent ideas.

A difficult and important problem is to distinguish between 'the normally constituted person making bad blunders' and 'the sick man in need of treatment.' The teacher should study these cases as a 'true naturalist,' as a biologist studies cell-growth, etc.; must love the study; must be 'tender with the sanctities of youthful feeling,' treating the burdened pupil as a 'beautiful wounded bird.' The teacher should analyze 'the successive grades of mental facts,' as sensations, elementary feelings, perceptions, etc., and seek to place the defect in this series. In estimating the importance of defects one must determine how far they affect one's actual habits of conduct and how stubbornly they refuse to yield to 'gentle social suggestion.' An intellectual or moral abnormality unaccompanied with feelings of suffering is more grave on that account.

The author promises another article on anomalies and abnormalities of temperament. In view of the fact that these articles are addressed to those who are almost never medical experts and rarely psychological experts, it is to be wished that the forthcoming article should contain more such clear and explicit characterizations and advices as those given by the author in treating of Emotional Disorders. The fact that 'there is no magic about the teachers' art of treating mental defects' except 'humanity, experience, watchfulness, intelligent comprehension of the actual situation—and tact' does not make such explicit characterizations and advices superfluous.

Die geistige Entwickelung in der ersten Kindheit, nebst Anweisungen für Eltern. W. PREYER. Berlin, 1893. Pp. 201.

Mental Development in the Child. W. PREYER. Tr. by H. W. BROWN. International Educational Series. New York, Appleton, 1893. Pp. 170.

Professor Preyer's object is to introduce mothers to methods of observing children. The author's *Seele des Kindes* (3d Ed., Leipzig, 1890) is so well known (even to the general educational public, through Mr. Brown's translation of the second edition) that a review of its contents is superfluous in this journal. The book will doubtless receive the wide reading it deserves, and will contribute materially to the host of influences that are making toward a scientific psychogenesis. It is to be regretted that the translator has not given to American readers the sixty-page "Anleitung zur Führung eines Tagebuches über die geistige Entwickelung kleiner Kinder, etc.," which concludes Preyer's volume.

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Notes on the Development of a Child. MILLICENT W. SHINN. University of California Studies. Berkeley, published by the University, 1893. pp. 88.

This is the first instalment of a work which will prove exceedingly helpful to psychology. Miss Shinn has carried on a careful, personal, daily study of *one child* (a healthy, bright California girl) during the first three years of her life, and recorded her observations on the spot. The present volume is confined to the study of Sight, and the observations are arranged topically, in a manner similar to that of Preyer, though with some additional topics. The sections on Color Perception and Appreciation of Form are of special interest. Some of the child's first attempts at drawing are reproduced in full-page cuts. A very welcome section is that devoted to the development of the child's Interest in seeing. Careful study of this and kindred topics is much needed. The author is very diffident about putting forward theories, but she is certainly doing her part towards furnishing child-psychology with that which it needs most at present, viz., 'a body of carefully observed facts' (to quote the words of Prof. Le Conte in the Introduction). We look forward to the appearance of the remaining portions of the work. FREDERICK TRACY.

UNIVERSITY OF TORONTO.

L'imagination et ses variétés chez l'enfant. F. QUEYRAT. Paris, Alcan, 1893. Pp. 162.

In this book M. Queyrat presents in a popular way, as he says, for teachers, the evidence recently brought out of 'types' of memory and imagination—visual, auditive, motor, etc. He makes no effort to explain the facts or to add to our knowledge, and his information is drawn largely from French sources. Some of the newer analyses, also, have escaped him altogether. But for the audience which he addresses the book is well suited, and may be commended to teachers and students as a clear statement of a class of psychological results likely to prove of very great pedagogical importance. J. M. B.

VISION.

(1) *Experimentelle Untersuchungen über die Helligkeit der Farben.* EDUARD GRUBER. Philosophische Studien, IX. 429–446. 1893.

(2) *Untersuchungen über den Lichtsinn.* KARL PÉTRÉN. Skand. Archiv für Physiologie, IV. 420–447. 1893.

(3) *Zur Lehre von den Gesichtsempfindungen welche aus successiven Reizen resultiren.* KARL MARBE. Phil. Studien, IX. 384–400. 1893.

(4) *The Use of the Rotating Sectored Disk in Photometry.* ERVIN S. FERRY. The Physical Review, I. 338–345. 1894.

(5) *Ueber den kleinsten Gesichtswinkel.* E. A. WÜLFING. Ztschr. f. Biologie, XXIX. N. F. XI. 199.

(1) Gruber finds that he is able to determine the relative brightness of two different colors, or of a color and a given gray, with a mean error which is hardly perceptibly greater than when the comparison is

made between two sensations of the same color-tone. This is contrary to the experience of Helmholtz and other writers on the subject. He goes on to discuss the belief of Hering and of Hillebrand that the effect of the addition of the specific color-process to the accompanying white-process of a mixed sensation is, in the case of blue and green, to diminish the brightness, and in the case of red and yellow to increase it. Hillebrand's argument is based upon the fact that, if a gray, a yellow, and a blue are chosen, which are equally bright when the illumination is so faint that no color is perceptible, then in the illumination of ordinary daylight the blue will be darker than the gray and the yellow will be brighter than the gray. (That the yellow is much brighter than the blue is the well-known Purkinje phenomenon, but that the gray is intermediate between them was not mentioned by Purkinje.) That the specific blue-process exercises an actual darkening effect upon what would be the brightness of the gray-process if it existed by itself, Gruber considers is not proved by this fact, and for this reason : the fact can be perfectly well explained by the assumption that the relative increase of the color-component and the gray-component is different for different wave-lengths ; and this assumption is in fact the more probable, because, first, there is no reason to suppose that they would be the same, and secondly, increase of illumination beyond a certain point increases the gray component more than the color-component for all wave-lengths. It is difficult to see that either of these considerations adds anything to the fact that, in the absence of information, one supposition is as reasonable as another. But if they do, they have a bearing only upon the fact that blue becomes less bright than yellow with increased illumination, and no bearing whatever upon the fact that blue becomes less bright than gray. Hillebrand's argument is indeed fallacious, but not at all more so than Gruber's argument against it. Gruber also found that to a red-green blind person a given green pigment looked no brighter (that is, was matched with no brighter gray) than to the normal-eyed. But it does not appear that any pains was taken to find a green that was really neutral to the individual tested ; most greens are either yellow or blue to those who see blue and yellow only, and their neutral band is very narrow (for this individual near *E*). The experiment is therefore of no value, but it suggests an interesting field for farther investigation.

(2) Petrèn finds that, for most experiments on the sensation of light, it is extremely desirable that the background should be neither black nor white, but an even gray ; the difficulty of securing correct adaptation is then avoided, for this is the illumination to which the eye is adapted in ordinary use. With this background, and with ob-

jects, both black and white, which subtended a visual angle of 50′, he found that the period of excitation necessary to produce a sensation was $\frac{1}{1000}$ of a second. The sensitiveness to a change in the period of excitation is slight for less periods than this; for greater periods, it first rises, and then remains constant. One twelfth was the smallest difference perceptible. The time necessary for the production of the maximum effect of a light-sensation cannot be exactly determined, but it is not less than $\frac{3}{10}$ of a second. The most favorable visual angle for the rapid perception of form is 35′, or between 23′ and 53′.

(3) The first experiments upon this subject were made by Plateau and by Emsmann. They found that different conditions were necessary for the fusing of different colors, but as it does not appear that they were careful to obtain colored papers of equal saturation and equal brightness, their experiments on this head are not conclusive. But it is certain, on the other hand, that the less the intensity of the illumination, the greater may be the duration of the separate impressions,—that is, with sectors of a given size, the less is the requisite velocity of rotation. Marbe has confirmed this result, using a large number of exactly determined intensities and periods. He finds, however, that the two quantities do not vary in a simple proportion. When the two sectors were of unequal size (the experiments were made only in black and white) he found that with increasing intensity the differences of the two sensations could be increased, and so with diminished velocity of rotation, other things, of course, being equal. Fusion takes place more readily when the white sector is larger than the black one, than when the reverse is the case. The view of Plateau and v. Helmholtz that, provided the number of the sectors is the same, the relative size of the black and white sectors is of no consequence, is thus found to be not correct.

(4) It is commonly assumed that when a rapidly rotating disk from which radial sectors have been removed is used to diminish the intensity of light, the proportion of light cut off is equal to the ratio of the aperture of the disk to the entire disk. While this is of course true physically, it has been found to be not the case for sensation; the error which arises is negligible only when the total aperture is more than one half of the entire disk. The error depends only upon the total aperture, and not upon the number of the vacant sectors which go to make it up. With an aperture of 24°, white light was diminished from ten to fifteen per cent more than theory would require. The error is not a function of the total amount of illumination. The writer considers that it can be explained in the following way: The luminosity of a given white light is the sum of the luminosities of the different wave-lengths which

compose it. Both the intensity of the retinal sensation and the time light must act upon the retina in order that it may be seen are direct functions of the luminosity. When light acts upon the retina for a very short time the elements of low intensity—i.e., at the ends of the spectrum—will not produce their maximum impression, and hence the total luminosity will be diminished. But it was noticed that when the disk rotated so slowly as to produce flickering, *more* light went through the screen than should go through theoretically, and this the author attributes to the building up of separate impressions upon the retina. Why the separate impressions should be built up in the one case and not in the other he does not explain. When the error was very marked (with rapid rotation) the light at the disk end of the photometer was of a bluish tint. How this shows that blue light—that is, the non-luminous end of the spectrum—was disproportionately cut off does not appear. Other experiments were made in confirmation of the fact that in the rotation of black and white disks the luminosity is the same as it would be if the light emitted were distributed evenly over the whole disk; monochromatic light was used throughout a wide range of luminosity and wave-length.

(5) The author points out that the separation of luminous points is a process not well adapted to the detection of the *minimum visibile*, on account of the necessary errors caused by irradiation. He made use of a vertical line whose lower half could be moved by a measurable amount to the right or to the left. He found that a deviation of .1 mm at a distance of two metres could still be detected, which would correspond to a least visual angle of ten seconds. This can be brought into harmony with the known size of the cones in the fovea only upon the supposition that the cones are not separated from each other by a distance greater than their diameter.

C. L. FRANKLIN.

BALTIMORE.

Flatternde Herzen. ADOLF SZILI. Zeitsch. f. Psychol., III. 359–387. 1891.

Zur Theorie der 'flatternden Herzen.' A. SCHAPRINGER. Zeitsch. f. Psychol., V. 385–396. 1893.

When patterns or figures of certain colors occur on grounds of certain other colors, the red or yellow parts seem to stand out in front of the plane of the blue or green parts, and to form an independent lattice-work. If now the eyes of the observer move about, an apparent movement or flickering of this lattice-work over its ground takes place because of the absence of the parallactic excursion which would be made by an actual lattice-work in front of the observed plane. The same is true if the observed object itself be moved back and forth with some rapidity.

The first of the above papers gives the results of a careful investigation of these phenomena and the circumstances which favor their occurrence. The second paper furnishes an explanation for them. Helmholtz' explanation is, that the light-impression in the eye does not arise and disappear with the same rapidity for the different colors, and therefore the blue remains apparently somewhat behind the red (Phys. Op. [1]383 ; [2]533). Schapringer accepts chromatic aberration as a partial explanation. Differently-colored rays of light are not equally refracted by the lens ; if blue rays are brought to a focus on the retina, red rays from an equally distant object have their focus behind the retina. To focus the latter on the retina requires a change of accommodation which arouses the impression that the red is nearer than the blue.

But this cannot be a complete explanation, for to some people the blue appears nearer than the red, or at the same distance, or sometimes nearer sometimes farther. These differences are accounted for by the introduction of a second factor already noticed by Einthoven, but not applied to these phenomena; namely, the non-coincidence of the point where the line of regard passes through the plane of the pupil with the central point of the pupil. Fixate and sharply accommodate for a blue point, and a red point which lies close to and directly above it will produce on the retina a dispersed image whose central point will lie not directly under the image of the blue point, but to right or left of it. This difference does not exist at the retinal pole of the eye's axis, but grows continually greater the farther we go peripherally from this point. Now whereas on a moving surface the red and the blue have the same rapidity, in the retinal image the red moves faster than the blue. When an image moves peripherally from the fovea centralis, in one eye it approaches, in the other recedes from, the retinal pole of the axis of the eye ; so that the increasing mutual distance of the colors in the one eye is balanced by the decreasing distance in the other, and the flickering does not occur. But having passed beyond this pole the distance increases in both eyes, and more rapidly as the image progresses farther towards the periphery ; and this fact together with the greater sensitiveness of the peripheral parts for the perception of slight movements (Exner) accounts for the greater distinctness of these phenomena in indirect vision. That they are more apparent in dim light is due to the widening of the pupil, which causes its central point to deviate still more from the line of regard. E. B. Delabarre.

Brown University.

L'audition colorée chez les aveugles. JEAN PHILIPPE. (Details to be printed in France.)

Pseudo-chromæsthesia in its different forms is more common in the blind than in others. Thus the phenomena were discovered in 30 cases of the 150 examined. This greater frequency results from the general tendency of the blind to associate their lost visual memories with the auditory and other sensations which have taken the place of sight. The phenomena vary greatly according to the occupation of the blind. They are more frequent in those who are versed in music and literature, especially in the former. No instance of pseudo-chromæsthesia was found in those born blind. In all other cases it may be present, and even seems to depend on the blindness, for it is usually subsequent and sometimes follows its variations.

AUTHOR'S ABSTRACT.

SORBONNE, PARIS.

HEARING.

Researches in Acoustics. ALFRED M. MAYER. Am. Journ. of Science, vol. XLVII. 1–28. Jan. 1894.

The first investigation in this paper is on "the law connecting the pitch of a sound with the duration of its residual sensation," and is in continuation of researches published in the above journal in 1874 and 1875. More especially the object of this research was to measure the "duration in which the after-sensation of a sound does not perceptibly diminish in intensity." Three forms of apparatus were used, in all of which the continuous sound from a tuning-fork reinforced by a resonator was cut off into periods of a given length by a perforated revolving disk. Most trustworthy results were given where the ratio of the diameter of the hole in the emitting end of the resonator to that of each hole in the disk was 1 to 2. Taking the period between maximum and minimum intensities of succeeding sound-pulses at the point where the pulses became continuous, the author found that the residual sensation diminished in duration as the number of vibrations increased: being, for instance, 0.0361 sec. for 64 v. d. and 0.0049 sec. for 1024 v. d. Expressed in vibrations, however, the corresponding values are 1.38 and 3.01. The computed values show a very close agreement with the observed values.

In saying that no one has made similar researches since his first papers published in 1874–75, the author has overlooked Urbantschisch's work 'Ueber das An- und Abklingen akustischer Empfindungen' in Pflüger's Archiv, XXV, 1881, p. 328. Urbantschisch found with our author a decrease of the residual sensation with increasing number of vibrations.

We infer from the account of the experiments that Prof. Mayer took the point where the interruptions just blend into a continuous tone as the limit for determining the residual effect: this gives a lower value than Urbantschisch found by descending from the blend to the point where the interruptions became just audible. If the ear could stand it, combining the two ways according to the method of minimal changes would be better than either way separately. Whether we may assume that the periods of the rise and decadence of a tone-sensation are equal or not, at any rate it is interesting to observe that the duration of the residual tone-sensation agrees closely with the time found by several observers (Pfaundler, Kohlrausch) for the rise of a tone-excitation into consciousness, i.e., the time of two to three vibrations. (Cf. G. Martius, 'Ueber d. Reactionszeit und Perceptionsdauer d. Klänge,' Phil. Stud. VI. 394.)

The second part of the paper is 'On the Smallest Consonant Intervals among Simple Tones,' as derived from the blending of beats. Dr. Koenig of Paris assisted in this investigation with forks not at the author's command. For tones above 192 v. d. the computed and observed values of the consonant intervals agree closely, but for 48 and 64 v. d. Dr. Koenig found no consonant interval short of the octave. This Prof. Mayer refers to the interference of inferior and superior beats. On the other hand, there was considerable difference between the values found for beats and those calculated from interrupted tones. The factor of the relative intensities of the interrupted and the beating tones may affect the ratio of the results. Besides, as Wundt has pointed out (Phys. Psy., I[4] S. 474), the blending of interrupted tones is dependent on the size of the spaces between the holes of the rotating disk, as well as on the number of tone-pulses.

In the third part of the paper, which is on 'The Duration of the Residual Sonorous Sensations as deduced from the Smallest Consonant Intervals among Simple Tones,' the author does not find that the discrepancy between the results from interrupted tones and from beats is harmonized by taking the beats during the time between the halves of the maximal amplitude of vibration instead of between the maximal amplitudes themselves. Here, of course, the question of the just observable difference comes up. This value is by no means so simple a function as has been commonly supposed, for it depends not alone on the intensity of the stimulus, *but on its rate of change.* We do not quite understand Prof. Mayer when he says that *a priori* considerations lead Professors Cattell and Fullerton in their work on 'The Perception of Small Differences' to 'the opinion that it is probable that the sensation is directly as the stimulus.' These investigators, or more especially Prof. Cattell, propose on the basis of their experiments to substitute for Weber's law the following: "The error of observation tends to increase as the square root of the magnitude, the increase being subject to variations whose amount and cause must be determined for each special case" ('Perception of Small Differences,' p. 153).

The suggestions made by the author on a method of determining just-observable differences of intensities of sound by means of the micrometric observation of amplitudes of vibration are valuable. It is a pleasure to find that Prof. Mayer's ability and technical resources are again to be put into use for the benefit of psychological acoustics.

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MEMORY.

Experimentelle Beiträge zur Untersuchung des Gedächtnisses. G. E. MÜLLER and F. SCHUMANN. Zeitsch. für Psychol., Bd. VI. 81–190, 257–339. Nov. and Dec. 1893.

Stimulated by the work of Ebbinghaus, the authors set out in 1887 with the general purpose of investigating the extent to which his methods might be employed for further research. Their labors have extended intermittently over a period of five years.

Although containing some rather radical alterations, their general procedure is highly similar to that of Ebbinghaus. The one really fundamental difference is found in the fact that in the present instance the subject in no case knew either the purpose or the result of the experiment, whereas Ebbinghaus, being his own subject, was kept constantly aware of both these things. As in his work, a large number of meaningless syllables were constructed, consisting of a vowel, or diphthong, between two consonants. It was decided, owing to certain preparatory experiments, to prevent the occurrence of alliteration, rhyme, and assonance, together with the too frequent repetition of diphthongs and difficult consonants. These, with a few other less important modifications, were, however, made in accordance with a fixed rule, which allowed chance to be always the deciding factor in the actual arrangement, and never the subjective preference of the experimenter. The syllables thus obtained were then arranged on a drum, which revolved at a medium and uniform rate, exposing the syllables successively through a slit in a screen. [In the experiments of Ebbinghaus the whole series was exposed at once.] The subject then pronounced the syllables aloud in trochaic rhythm, save in one set where iambic was employed, and the process was continued until the whole series could be correctly anticipated.

The nerve of the whole method lies in the various highly ingenious modifications of these memorized lists, which the subject is then required to learn again, his rapidity in the last instance compared with his rapidity in the first showing at once the effect of some one constant factor. Suppose, for example, that on one day six series of twelve syllables each had been learned. On the following day are learned two series formed exclusively of syllables used the preceding day, but so arranged that no two syllables stand side by side, which previously were in immediate juxtaposition. Two more sets are learned, in each of which six syllables occupy the same positions they did the day before, both with reference to each other and to the trochaic measure, i.e., they bear the same accent. Still a third set of two series is memorized, in each of which six syllables again stand side by side as on the first day ; but the position in the foot is now changed, and consequently the accent altered. It will be seen at a glance that each of these sets presents different constant factors. In the first, the syllables themselves alone remain the same. In the second, both the order and the accent are in part identical, while in the last only a portion of the order is retained. The varying ease and rapidity with which these different series are committed to memory will serve as an index of the influence these various factors exert. This illustration must suffice to convey, however inadequately, a general notion of the mode of procedure.

One may summarize the more important results as follows : (1) The suppression of rhythm in the attempt to memorize such lists as these renders the task much more difficult, consuming about twice as much time as otherwise is necessary. For Germans trochaic measure seems to permit more rapid memorizing than iambic, probably because the majority of German dissyllables have the accent on the first syllable. In the pronouncing of such lists there occurs an involuntary chief ictus on certain syllables,—conspicuously on 1, 5, 7,—coupled with a slight pause and lowering of the voice after 6. The result is to split the series ordinarily into two. (2) By means of a pneumograph records were made showing the peculiarities of breathing during the experiments. Each series of twelve syllables shows a double apex in the breathing cuvre, corresponding to the above-mentioned rhythmical division of the series. (3) The syllables first memorized are by no means always those which stand earliest in the series. Often the last half of a list is committed first. That syllable which first attracts the attention strongly is likely to be first learned, and precisely which that shall be depends upon circumstances purely accidental. An equal distribution of attention over the several members of a series does not occur. (4) As regards the sensorial character of lists thus memorized, the results are tentative and suggestive rather than conclusive. Visual, auditory, and kinæsthetic elements all manifest themselves, and under the influence of practice the form used seems to vary; e.g., from visual to auditory. (5) The first of any two successively—as well as simultaneously—experienced syllables tends to call up the second, and, when associated through intermediate syllables, the association is stronger if both are accented. Furthermore, the second syllable in a trochaic measure tends to call up the first, and apparently this tendency is stronger than the tendency to call up the first syllable of the next succeeding foot. This result is not to be interpreted as proving that every presentation tends to reproduce by association its immediate predecessor, but is rather to be viewed as belonging under the general law, according to which each element of a presentation-complex tends to the reproduction of the whole. Associations of syllables with their numerical positions ('absolute positions') in the series were apparent. The results of this nature seem to be connected with the previously-mentioned division of the syllables by accentuation. Ebbinghaus seems to have overlooked this element. It may properly be added here that his adoption of the decreased effort demanded for re-learning a list, as the criterion of the strength of association between the separate members of such a series, is quite unwarranted, for it disregards at least one important consideration—i.e., the complex nature of the associative tendencies represented by any one syllable. (6) Under some conditions, syllables with already-established associations prove more difficult to memorize in combination with new syllables, than where such associations are lacking. The previous associations press in on consciousness and disturb attention in various ways. (7) The success of any particular attempt to memorize seems to depend in part upon its time-relations to the other attempts of the same day, e.g., whether it comes first or last. We need only say that attention and fatigue are the principal conflicting elements. A gradual improvement in the rapidity of learning by heart was constantly manifested. (8) The varying ease with which lists of this kind are learned depends not wholly upon subjective differences, but in part upon objective conditions valid in some degree for all persons.

The mathematical problems encountered in interpreting the results are discussed with great care and much fulness. The calculation affecting the widest range of considerations is that by which is obtained a so-called 'central worth' for the number of repetitions necessary to memorize a list. This is simply a quantity attained under conditions intended to eliminate the disturbing effects on the arithmetical average of large individual variations.

Upon one and another point of detail careful readers are sure to find room for critical queries. With a piece of work covering so much ground this could not be otherwise. Professor Ebbinghaus is quite certain to find occasion for reply to some of the respectful strictures upon his work. But taken all in all, the masterly appreciation of the significance of the problems involved, the untiring energy and ingenuity with which they have been attacked, and the admirable discrimination with which the results are interpreted combine to make this one of the most hopeful pieces of recent experimental work. The Old Benchers will proclaim that the definite results are out of all proportion to the time and labor expended. But unless the whole method can be proved fallacious, one must admit a genuine enlargement of our accurate psychological knowledge.

JAMES R. ANGELL.

UNIVERSITY OF MINNESOTA.

Le problème de la conscience du moi. Dr. PAUL CARUS. Paris, Alcan, 1893. Pp. 144.

Dr. Carus discusses the problems of consciousness and self from the monistic standpoint. Starting with the incommensurability of consciousness and physical phenomena, he shows the inadequacy of materialistic and spiritualistic solutions of the difficulty, and urges the superiority of monism over a dualistic interpretation. Objectivity is the outer side of being, of which subjectivity is the inside or kernel.

The author examines at considerable length (chap. VI) the neuroses of reflex and voluntary movement, and their relation to thought. "The mechanism of thought consists in combinations, separations, and recombinations of representative images or symbols ;" "the object of thought is adaptation to environment." Passing to the genetic problem, he attributes to the lowest forms of animal life *isolated feelings*, which become states of consciousness "only in organisms where sense-impressions revive memory-traces, and an interaction can take place between different feelings." He distinguishes (chap. IX) *consciousness* from *intelligence*, including under the latter not only conscious states, but also many sense-impressions, movements, and intellectual functions which are not accompanied by consciousness ; all these are 'mental operations.' "The unity of consciousness is not an intrinsic property of mind" : it is the result of concentration (attention). The continuity of man's psychic life is due not to any permanence of substance, but to the 'permanence of form' which characterizes the memory-traces.

The author takes decided issue with the utilitarian theory. He minimizes the value of pleasure as a motive and claims that "pain is an important and even beneficent factor in evolution." Mr. Spencer's state of perfect adjustment between organism and environment is an unrealizable dream. The final chapters treat of immortality and the theistic problem.

H. C. WARREN.

PRINCETON.

NEW BOOKS.

Introduction à la psychologie expérimentale. ALF. BINET. Paris, Alcan, 1894. Pp. 146.

Man and Woman. HAVELOCK ELLIS. New York, imported by Charles Scribner's Sons, 1894. Pp. 409. $1.25.

Éléments de psychologie physiologique et rationnelle. Dr. SURBLED. Paris, Masson, 1894.

Zur Lehre vom Inhalt und Gegenstand der Vorstellungen. K. TWARDOWSKI. Vienna, Hölder, Pp. 111. M. 2.40.

Materials for the Study of Variation treated with Especial Regard to Discontinuity in the Origin of Species. W. BATESON. London and New York, Macmillan & Co., 1894. Pp. xv, 598.

L'eredità e l'origine delle specie. PIETRO MANTIA. Palermo, 1894. Pp. 43.

Psychiatrie. TH. ZIEHEN. Berlin, Friedrich Wreden, 1894.

La famille neuropathique. CH. FÉRÉ. Paris, Alcan, 1894.

La contagion du meurtre. DR. P. AUBRY. Paris, Alcan, 1894.

Classification objective et subjective des arts, de la littérature, et des sciences. R. DE LA GRASSERIE. Paris, Alcan, 1894.

Hegel's Philosophy of Mind. Translated from the Encyclopædia of the Philosophical Sciences. Oxford, at the Clarendon Press, 1894. Pp. cciv, 202.

A Study in the Origin of German Realism. NORMAN WILDE. Contributions to 'Philosophy, Psychology, and Education,' Vol. I. No. I. Columbia College, New York. Pp. 77. 60 cents.

Das Ganze der Philosophie und ihr Ende. Ihre Vermächtnisse an die Theologie, Physiologie, Æsthetik und Staatspädagogik. RICH. WAHLE. Vienna, Braumüller, 1894. Pp. xvi, 558. M. 10.

History of Trade Unionism. SIDNEY and BEATRICE WEBB. London and New York, Longmans, Green & Co., 1894. Pp. xvi, 558.

NOTES.

Professor DEWEY has accepted a call to the Head Professorship of Philosophy in the University of Chicago.

Professor FULLERTON has been elected Vice-Provost of the University of Pennsylvania.

Professor EBBINGHAUS, one of the editors of the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, has left Berlin to occupy a chair in the University of Breslau.

Mr. HENRY RUTGERS MARSHALL has been appointed Honorary Lecturer on Æsthetics and Dr. NORMAN WILDE Assistant in Philosophy in Columbia College.

MR. JAMES R. ANGELL has been appointed Instructor in Psychology in the University of Chicago.

The death of Professor GEORGE JOHN ROMANES, which occurred at Oxford on May 23, is a serious loss to psychology as well as to biology. Romanes was born in Canada on May 20, 1848, and was consequently just forty-six years old. Had his life been longer other equally important works from his hand would undoubtedly have followed *Animal Intelligence*, *Mental Evolution in Animals*, *Mental Evolution in Man*, and his other writings. These, however, suffice to give him a high place among the great men who have developed the doctrine of evolution.

The *Philosophical Remains* of G. CROOM ROBERTSON, edited with a memoir by Professor BAIN, is announced by Messrs. Putnam.

Messrs. Ginn & Co. announce the publication of an experimental study by Dr. HERBERT NICHOLS entitled *Our Notions of Number and Space.*

We are asked to print the following note :

"The editors of *Mind* request that all MSS. from America intended for publication in that Journal be sent to Professor TITCHENER, Cornell University, Ithaca, N. Y., instead of to Mr. STOUT as has hitherto been the case."

VOL. I. No. 5. SEPTEMBER, 1894.

THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (II.)

COMMUNICATED BY PROFESSOR HUGO MÜNSTERBERG.

A. THE MOTOR POWER OF IDEAS.

BY HUGO MÜNSTERBERG AND W. W. CAMPBELL.

The direct dependence of bodily movements upon ideas is not only one of the most important, but also one of the most neglected, chapters of psychology. Its importance grows with the modern development of psychological theory. On the one hand, the tendency increases to consider the conscious phenomenon of will as a combination of elements which are not essentially different from the elements of ideas. Voluntary movements must also be understood as the results of sensations and feelings. On the other hand, the tendency is apparently increasing to emphasize the motor elements of ideas and to consider the motor effects of ideas as essential factors of their *rôle* in consciousness—especially to look at the phenomena of attention from this standpoint. Exact study of the direct relations between ideas and involuntary movements has therefore its bearing both on the intellectual and on the volitional side of the psychological life.

The most careful experiments made in this direction are on subjects in abnormal conditions, either hysterical or in the hypnotic state. In both cases the abnormal inhibitions make it possible that the movements under the influence of ideas take place without the interference of a conscious act of will. But it is obvious that these cases are extremely complicated and difficult to understand; above all, that the results obtained from them cannot be identified directly with normal functions. With normal subjects the possibility of such experiments seems extremely limited. The resulting movement is here conscious, and the perception of it becomes therefore the motive for a voluntary impulse which changes or inhibits the movement, even when there is not a volition controlling the movement from the start. An action of the muscles under the influence of ideas without any interference of the will seemed open to quantitative experimental study only in the case of those movements which are too small to be perceived, as in the experiments on muscle-reading. Movements of the hand, which correspond to the ideas of space-direction, without a subjective knowledge of these movements themselves, can in such cases be registered graphically. Experiments on imitation, on time-measurements of muscular reaction, on coördination of movements, etc., present similar conditions. But all these methods do not offer more than general hints about the relations of ideas and movements. A quantitative study of the motor power of ideas, therefore, and the variations and conditions of this motor power, seemed impossible. Nevertheless, there is one group of muscles which allows just such experiments—the eye-muscles.

The facts on which our new method is based offered themselves almost by chance. A physicist informed one of the writers, Münsterberg, some years ago that he had discovered a proof that after-images are of central and not of peripheral origin. The proof was as follows: If he looked at a bright flame for twenty seconds, closed the eyes, and turned the head perhaps 45°, he then saw an after-image of the flame in the direction toward which the head was turned; but if he looked at the flame one second only, and closed the eyes and turned the head, he saw the after-image, not in this direction, but in the direction of the actual flame. He was of the opinion that this localization of the after-image, independent of the position of the head, could be the result of a central projection only. Münsterberg repeated the experiments and confirmed the result fully; but he saw immediately that the result is dependent entirely upon eye-movements. When he opened the eyelids after turning the head he found that the eyes were in the first case turned with the head, while in the second case they were directed towards the flame. The after-images appeared in the direction toward which the eyes were turned; the result had therefore no bearing on the question of the seat of after-images. But Münsterberg recognized that the experiment offers an instructive case of measurable muscle-action, under the influence of impressions, without conscious influence of the will, as the subject in both cases did not move the eyes voluntarily, but gave his whole attention to the head-movement only, while the eyes went their own way, either with the head or towards the flame. The meaning of the experiment, seen from this standpoint, is obvious. When I open my eyes and see a flame, this optical impression brings out the motor effect of fixating my eyes upon it—an effect which is the essential element in attention. When I turn my head with closed eyes this head-movement is naturally coördinated with the eye-movements, and the head-movement acts as stimulus to a motor reaction of the eyes. This second stimulus is, of course, the same whether I turn my head after twenty seconds or after one second. Now if this same stimulus brings out two so very different effects, it must be because the stimulus from the optical impression is different after twenty seconds and after one second. After one second the optical stimulus is stronger than the head-stimulus, and the eyes turn therefore to the flame; after twenty seconds the motor power of the optical stimulus is fully discharged, or at least so exhausted that it is overpowered by the head-stimulus, and the eyes follow the head. In this primitive form the experiment would tell us, of course, only that the motor power of the optical impression was after one second stronger than after twenty seconds. On the one hand the question arises: What was its strength during this interval? What after two, three, or ten seconds? Did it decrease continually, or is there a fluctuation or a sudden decrease? Are there periods in which neither stimulus is stronger, so that the eyes take a middle position, which perhaps by its changes may give a measurement of the phases of this motor power. On the other hand the question arises, What is the effect if we take other optical objects instead of the flame? Is the motor power changed if I change the color of the flame, or if I take a picture or a figure or words, and what influence has the character of those objects? Of course a picture gives us no satisfactory after-image; but the after-image has nothing to do with the results which interest us. We can dispense with it, as it only serves as an index of the position of the eyes. If we open the eyes after turning the head, and state in what direction our eyes are looking while at the same time measuring the position of the head, we shall then be able to measure the motor power of the optical impressions by its dependence both upon the time we look on the objects and upon the character of the objects themselves. In this way we developed the systematic method upon which the following experiments are worked out.

One thing may be said at the outset: Not every one is able to repeat the experiment described above; and of those who do, not every one is a good subject for the more complicated experiments we are about to describe. The reasons for this are obvious. There are many people who are unable to turn the head voluntarily with closed eyes without first imagining in visual terms the direction in which the movement is made. This optical idea of course has the strongest influence upon the eyes, and the head only follows after the eyes. The idea to turn the head with closed eyes has on them the same effect as if they saw an object in indirect vision and in order to look at it directly make a head movement. It seems that most of the strong visualizers are of this kind, while the persons of motor type succeed easily, and after a little training get to be in a state in which the head is moved after an acoustical signal by a simple reflex which is without relation to visual presentations. We found that it was successful with almost half of the laboratory students. The fact that some are not skilful enough in fulfilling the conditions of the experiments, as they are unable to move the head with closed eyes without thinking of the optical space relations, does not of course signify anything against the method. Such persons often need only a short training to learn it. The following experiments were made with six subjects who all succeeded very well, but as a complete series was made only by three men, our report will be confined to the results of these three. One of them (Münsterberg) had already had much practice in this kind of experiment and moved the head after a sound-signal as an acoustical reflex. The two others, Mr. Starbuck and Mr. W. W. Campbell, succeeded also from the beginning.

Our special question was, how the motor impulse to fixate the eyes upon an optical object varies with the quality of the impression and with the time of fixation, especially with regard to the intervals of 1, 2, 3, 4 seconds. Our point of view was in the first instance a methodological one: is the new method really fit to bring out characteristic and suggestive results? The apparatus used was as follows: A plane black surface was placed in a horizontal position, about the height of the chin of a person standing. Fixed perpendicularly upon this surface was a semicircle of black cloth 50 cm high, having a radius of 60 cm; the centre of the head of the standing subject was the centre of this semicircle, and at the height of his eyes were numbers marking the 180 degrees, by fives, on the lower part of this semicircle. A black easel with an opening 10 cm square was so constructed that cards or pictures could be fixed behind the easel so that they just filled the little window in the black frame; this easel stood in the middle of the semicircle just opposite the face of the subject, 60 cm from his eyes.

We had ten groups of optical stimuli, in each group ten different pieces; so that every one made 100 experiments without repetition of the stimulus. As a full series included 400 experiments, which were extended over a whole year, no stimulus was repeated during a period of several weeks; of course with the exception of special experiments on the influence of repetition, of which we shall speak later, they are not contained in these 400. The 10 groups were the following, the order here given being arbitrary, as they had to come quite irregularly during the experiments.

(1) Black single letter or number, 2 to 4 cm high, pasted upon white cardboard which filled the aperture. (2) Single word of five to eight printed letters, always a substantive, on white cardboard. (3) Card with nine such printed words, three lines of three words; each a substantive; the height of letters 1 cm. (4) Single pictures; these were black printed simple outline drawings of single objects, as a top, or bird, or

horse; the picture about 2 cm square on white cardboard. (5) Card with nine such pictures, three lines of three, arranged in a solid square. (6) Single color; it consisted of a piece of saturated colored paper, filling the full size of the aperture in the black frame. (7) Two or three color-strips, of different colors, each taking a half or a third of the space in regular form. (8) Irregular arrangement of different pieces of four or more different colors. (9) Photographs, cabinet size, heads of men and women. (10) Columns of several numbers of three places which were to be added by the subject.

The second part of the apparatus was an arrangement of two sound-signals, two electric hammers with different sounds, connected with Schumann's instrument for the study of the time-sense. This instrument consists of five wheels on one axle, which runs in the kymograph. Each wheel has movable platinum points, which can close a mercury contact on a special board. The rapidity of the kymograph and the distance of the five platinum points were so arranged during all our experiments that the contact of the second wheel was one second, of the next two, of the following three, and of the fifth wheel four seconds after the contact of the first wheel. The contact of the first wheel produced the sound of the first electric hammer, which was the signal for opening the eyes; the contact of one of the other four wheels produced the noise of the second hammer, which was the signal for closing the eyes and moving the head to the side. A pressure of the buttons decided which of the four wheels was to give the contact. In this way we had four exact intervals at our disposal.

The method of the experiment was as follows: The subject stood upright with closed eyes, his chin over the centre of the semicircle and his face in the direction of the middle of the semicircle before him. In order that this position might be found when the eyes were closed two symmetrical supports were provided for the hands. Some one object from the hundred stimuli was then placed in the black frame, directly in front of the subject. The noise of the distant kymograph gives the signal that the attention must be given to the experiment; four seconds later comes the first signal. The subject opens the eyes and looks at the object before him; after an interval comes the second sound—the subject closes the eyes and turns the head to the right or left side and opens the eyes immediately. We both found it easier to turn always to the right, Starbuck turned alternately to right or left. The degree of the semicircle is then noted which the eyes of the subject fixate, and the direction of the head, which can be easily found by a wire pointer fixed on the forehead. Both the points for the eyes and for the head were noted to five degrees. The head movement and the opening of the eyes are therefore the same for all experiments; the difference lies only in the 100 stimuli and in the four intervals of time. A full series contained 10 experiments (corresponding to the 10 objects) in each of the 10 groups with each of the four intervals, that is, 400 experiments, made in entirely irregular order. When the subject opened the eyes to look at the window he never knew whether there would be color, word, picture, or numbers, etc., and never knew whether he had one, two, three, or four seconds to look. The following results give for each of the 40 groups the averages from 10 experiments. The 10 experiments of one group were always made on 10 different days, and all experiments under the same conditions. The degrees are counted from the point in front, 0°, toward the right, 90°; the average head position is in parenthesis.

MÜNSTERBERG.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
1. Letters	12 (50)	25 (48)	48 (52)	48 (50)	33 (50)
2. One word	13 (46)	25 (46)	47 (47)	47 (52)	33 (48)
3. Nine words	0 (48)	8 (49)	17 (48)	28 (50)	13 (49)
4. Picture	14 (46)	32 (50)	45 (52)	47 (47)	35 (49)
5. Nine pictures	0 (45)	0 (47)	2 (50)	16 (49)	5 (48)
6. One color	4 (48)	28 (47)	31 (50)	47 (52)	28 (49)
7. Two colors	4 (47)	22 (47)	23 (46)	46 (48)	24 (47)
8. Irregular colors	0 (46)	16 (48)	25 (50)	39 (51)	20 (49)
9. Photograph	5 (52)	15 (51)	22 (52)	28 (50)	18 (51)
10. Numbers for adding	42 (52)	47 (47)	42 (52)	44 (49)	44 (50)
Average	9.4 (48.0)	21.8 (48.0)	30.2 (49.9)	39.0 (49.8)	25.1 (48.9)

It seems to us that no one who looks over these figures can regard them as resulting from chance. The head movement varied only between 46° and 52°, and nevertheless we see that the eyes turned unintentionally to any position between 0 and

50°, obviously dependent upon the quality of stimulus and the time-interval. The results prove that this method yields results which allow the finest discriminations of differences which can be studied in no other way. The eye-muscles work here with the exactitude of a physiological nerve-muscle preparation, the contractions of which correspond to the quality and intensity of the electric stimulus; so our eye-muscles show in the most perfect way the effect of the central motor impulse which the optical stimulus and its associations produce.

With regard to the details there is no result so constant as the increase of the eye-angles with the increasing time. With practically the same head-movement of 48° to 50°, the eyes remain on 9° after 1 sec., 22° after 2 sec., 30° after 3 sec., and 39° after 4 sec. The longer the time the weaker the motor stimulus which tends to turn the closed eyes in the direction of the optical object. And the average result of all the 10 groups holds for each of them with the exception of the tenth, which has a different character, as we shall see. This result, that the motor power of every impression is strongest after 1 sec. and decreases steadily, is characteristic for this subject only; the other tables show a very different result. The decrease is quickest where the impression is simple; for one color after 1 sec. 4°, after 2 sec. 28°; and it therefore reaches its maximum for one letter, one word, or one picture after 3 sec. The decrease is slowest where the act of reading or looking fills nearly the whole four seconds; thus the nine pictures have after 4 sec. the power to turn the eyes to an average of 16°. As stated, the only exception is the 10th group—the adding of figures: here the eyes turn even after 1 sec. almost as far as the head, and all four intervals give the same result as if the figures had no motor power at all. The notes on the results of self-observation, which were taken regularly, show that the opposite is true. The motor power of these figures, together with the associational ideas of adding, was evidently too great to allow the reflex head-movement. The subject felt himself unable to make the head-movement in the usual reflex way so long as he really was adding; the adding inhibited the movement. He was therefore in almost every case obliged to stop the adding intentionally after hearing the signal and to give his whole interest to the movement, so that the idea of the calculation was quite swept out of consciousness. The tenth group can therefore not be compared for this subject with the nine others. If we take the average of the four intervals for those nine groups only, we get the exhaustion of the motor power still more distinctly: 5.8, 19.0, 27.8, 38.4.

If we compare those nine groups without regard to the influence of time, we find the following order: The weakest motor power results from one picture (35), one word (33), one letter (33), one saturated color a little stronger (28), two colors still stronger (24), then the irregular colors (20), the photograph (18); and strongest from the nine words (13) and the nine pictures (5). It is interesting to see that one letter and one word show exactly the same type of motor power, corresponding to the well-known fact that the time to apperceive them is the same. Very characteristic is the increase of motor energy produced by the variety of color; one color and two colors have the same motor intensity after one second, but even after two seconds the energy of one color is more discharged than that of two colors, and an irregular combination of many colors is stronger from the beginning. But the note-books show a fact which disappears in these general averages—that there are marked differences for the different colors; the red and yellow colors have more power than blue, etc.

The following tables give the results with the two other subjects:

STARBUCK.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
Letter	22 (36)	20 (33)	21 (35)	24 (32)	22 (34)
Word	14 (38)	15 (35)	25 (31)	23 (37)	20 (35)
Nine words	18 (37)	10 (41)	13 (39)	17 (33)	14 (37)
Picture	15 (38)	16 (37)	18 (37)	23 (34)	18 (36)
Nine pictures	12 (34)	9 (34)	16 (39)	13 (39)	12 (36)
Color	25 (37)	27 (35)	26 (35)	32 (36)	27 (36)
Two colors	22 (34)	21 (37)	24 (34)	16 (36)	21 (35)
Irregular colors	17 (36)	14 (36)	2 (35)	8 (34)	10 (35)
Photograph	11 (37)	4 (41)	15 (38)	13 (38)	11 (38)
Figures	18 (33)	12 (37)	19 (37)	10 (38)	15 (36)
Average	17.4 (36.0)	14.8 (36.6)	17.9 (36.0)	17.9 (35.7)	17.0 (36.1)

CAMPBELL.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
1. Letter..........	41 (41)	42 (42)	40 (40)	37 (44)	40 (42)
2. Word..........	34 (40)	37 (43)	37 (43)	37 (43)	36 (42)
3. Nine words.....	19 (45)	9 (46)	8 (43)	3 (43)	10 (44)
4. Picture.........	36 (41)	28 (40)	28 (43)	29 (40)	30 (41)
5. Nine pictures...	7 (42)	15 (43)	6 (44)	3 (42)	8 (43)
6. Color....	33 (41)	37 (46)	22 (43)	35 (44)	32 (43)
7. Two colors.....	21 (42)	18 (40)	27 (41)	27 (44)	23 (42)
8. Irregular colors.	16 (42)	14 (42)	20 (44)	9 (41)	15 (42)
9. Photograph.....	30 (42)	9 (46)	8 (43)	3 (43)	12 (43)
10. Figures.........	30 (40)	7 (42)	8 (40)	10 (44)	14 (41)
	26.7 (41.6)	21.6 (43.0)	20.4 (42.4)	19.3 (42.8)	22.25 (42.5)

It is evident that not only the absolute figures, but also the whole type of these two tables is different from the first one; but it is at the same time clear that here also the results are not due to chance. The difference of the results does not speak against the method; on the contrary, these tables prove that individual differences, which could not be stated in any other way, can be easily found by this method. It is of small importance, to be sure, that the average angle of head-movements is different for the three persons; the average for all experiments being, for M. 48.9°, for St. 36.1°, for C. 42.5°; and, somewhat proportionately, the average eye-movement, for M. 25.1°, for St. 17.0°, for C. 22.25°. These absolute values are of course unimportant, as they depend upon the voluntary impulse to turn the head; the tables show that they were fairly constant for each subject. More characteristic by far is the difference with regard to the influence of time. With M. there was a continual decrease of the motor energy of the optical stimulus, the averages being, after 1 sec. 9.4, after 2 sec. 21.8, after 3 sec. 30.2, after 4 sec. 39.0. With St. it is quite different. His average is after 1 sec. 17.4, after 2 sec. 14.8, after 3 sec. 17.9, after 4 sec. 17.9. This means that the motor power of the optical impression has not reached its maximum in the first second, but is increasing, coming to its height after 2 sec., and even then not discharging itself, but going back only to the intensity of the first second and remaining there till the end of the fourth. But this general average of the ten groups loses just the characteristic details which the full table shows. We see with St., firstly, that the motor energy may be also exhausted after the first second if the object is extremely simple—for instance, only one word or one picture or one color; secondly, that for more interesting objects the motor power fluctuates, getting, for instance, for added figures or nine pictures a second increase after four seconds; and thirdly, that it does not get the maximum for objects which are difficult to apperceive, like the irregular color combinations, before the third second. With Campbell the general average shows a continuous increase of motor energy; the increase is a slight one for the average of all ten groups, but looks very different when we consider the relations of the special groups. The table shows that the simple objects, as one word, one letter, one picture, one color, had from the beginning almost no motor influence at all, and did not get it later; only for the one color does the motor energy seem to increase in the third second. The nine pictures alone worked as a strong motor impulse from the start, decreasing a little in the second, but strongly increasing again in the third and fourth seconds. The nine words are steadily increasing; but it is especially characteristic that here, and much stronger still for the photographs and the added figures, the whole energy awakes during the second second, from 30 to 9, 8, 3 for the photograph, from 30 to 7, 8, 10 for the figures. All these complicated stimuli work in the first moment with Campbell like a meaningless object, and only when the associations awake does the motor energy increase rapidly. Even with the single picture there is the slight increase from 36 to 28. The type of the decrease and increase of motor energy is then extremely different for the three subjects: a steady decrease almost without exception for M., an increase till the beginning of the third second and a fluctuation during the following period for St., and a steady increase for C.

Much greater and almost complete agreement exists, however, for the three subjects if we abstract from the influence of time and look on the relative differences of the ten groups of impressions. All three subjects agree that a simple letter, word, color, or picture has the weakest motor influence; all agree that two colors have more motor power than one, and the irregular colors still more (M., one color 28, two colors 24, irregul. col. 20; St., 27, 21, 10; C., 32, 23, 15); that nine pictures have by far stronger motor energy than one (M. 35–5, St. 18–12, C. 30–8); nine words stronger than one (M. 33–13, St. 20–14, C. 36–10); that the photograph of a person has far stronger motor function than the simply sketched picture of an object of daily life (M., picture 35, photograph 18; St., 18, 11; C., 30, 12), etc.

We have not yet mentioned one group, the eleventh of our experiments, which was made with the other ten—the group of repetitions. Every subject had sometimes for each of the four intervals, not a new optical stimulus, but that of the foregoing experiment with the regular pause of a few minutes only. The subject, of course, did not know that the stimulus would be repeated—he expected something new; and as these repetitions were seldom tried, there was in every case a decided feeling of unexpected acquaintance. We repeated especially those stimuli of which the first impression showed rather strong motor energy. The average of the first column represents, therefore, a greater motor influence than the average of all ten groups together. The average on both sides is for each subject from exactly the same forty stimuli.

	M.		St.		C.	
	First Time.	Second Time.	First Time.	Second Time.	First Time.	Second Time.
One sec........	5 (45)	27 (41)	15 (39)	28 (40)	20 (46)	37 (45)
Two sec........	8 (50)	40 (50)	9 (38)	26 (33)	21 (43)	34 (41)
Three sec.......	18 (50)	40 (48)	13 (40)	25 (38)	13 (46)	40 (40)
Four sec........	22 (50)	47 (51)	14 (38)	23 (34)	17 (43)	37 (43)

It is obvious that, without exception, the same stimulus after the same interval has much weaker motor energy in the case of repetition; and this is true not only for these averages, but for every single trial.

The results of all the experiments show that the new method gives an answer to the three questions; the influence of the quality of the stimulus upon the intensity of the motor discharge, the influence of the duration of the stimulus, and the influence of the repetition of the stimulus. In all these cases the facts gave the most delicate record of individual differences. If we consider that this motor energy produced by a stimulus is the essential factor of that complicated emotional state which we call attention, it becomes evident that the whole question of the psychophysics of attention—its intensity, its fluctuations, etc.—is here opened to a method of study which frees us from the doubtful and narrow study of just perceivable sensations, and which allows an endless variation from the simplest optical sensations to the highest functions suggested by any optical impressions. Further, the mechanism of automatic impulses gets a method of exact study, which allows us to analyze those individual differences which even in our tables come out so decidedly, and which seem extremely important for the understanding of differences in central mental processes.

B. Memory. (II.)

By John Bigham, Ph.D.

The following experiments are an immediate continuation of those published in this volume of this Review, pp. 34–38. Their purpose is again to get empirical material for understanding the mechanism and the conditions of reproduction and memory. The special question is the influence of the time-interval between learning and recollecting with regard to its length and its filling. The experiments were made with six subjects during the winter 1893–94; average age of the subjects, 25.5 years.

The apparatus for all these experiments consisted of ten different classes of series. each series being composed of ten single presentations. Five of the classes were of visible presentations and five of audible. The visible series, placed horizontally on a white field, were exposed simultaneously by lifting a screen, twenty seconds for each series, and filled a space 40 cm long. The audible series were of course given in succession, 20 sec. being allowed for the ten presentations. The ten classes of series were:

1. Visible numbers: Zero and the nine digits, black, mounted upon white cardboard 3 cm square. 2. Audible numbers: The name of the ten numbers spoken by the experimenter. 3. Visible colors: Small squares of colored paper,

3 cm; white, gray, black, red, orange, yellow, green, blue, violet, brown. 4. Audible colors: The names of these ten colors spoken by the experimenter. 5. Visible forms: Ten geometrical figures drawn with red ink upon white cardboard squares, 3 cm. Star, cross, square, line, circle, etc. 6. Audible forms: The ten corresponding words spoken by the experimenter. 7. Visible words: Several hundred different monosyllabic words—nouns, verbs, and adjectives—each composed of two consonants and an intervening vowel; small black letters mounted upon white cardboard. These were arranged into series of ten words, the same word occurring only once for each subject during a period of at least a month, and then only in new combinations. Care was taken to secure proper variety in the sequence of the vowels and consonants of the words in each series. In the first six classes only the location of the single presentation in the series need be remembered; here, as every word occurred practically only once in all the experiments, the content of the presentation itself had to be remembered. The same is true for the three following contents. 8. Audible words: The same words, but arranged in series differing entirely from the visible series, and practically new words, as care was taken that every word appeared only once in a period of four or five weeks. The words were spoken by the experimenter. 9. Visible nonsense-syllables: Several hundred syllables, each consisting of two consonants with an intervening vowel; small black letters mounted like the words. The same method of arrangement as for the words. 10. Audible nonsense-syllables: The same method as with the audible words.

All the audible presentations were pronounced in a monotone without rhythm. For the visible numbers, colors, and forms, the observers had duplicates of the given presentations and arranged them as recollected. For the audible numbers, colors, and forms the observers were supplied with small white cardboard squares on which the names of the colors, forms, and numbers were written. For the words and syllables the subjects wrote the recollected letters upon strips of paper.

The errors were recorded by the conductor of the work and the subjects were not informed of their extent or character. They were considered as misplacements or as omissions of the single presentations, and separate records were made of the two kinds of error. For the words and syllables there was of course a third kind of error possible—the introduction of words or syllables which did not exist in the objective series at all. The character of each series was fully described before it was presented for learning. The time required for recollection was recorded by a stop-watch. Fatigue was minimized by giving the observers a rest after each series. They were advised not to use any mnemonic devices and to give equal attention to every presentation in the series; but their methods of learning, remembering, and arranging the series were free from any special control. The subject was alone with the experimenter in the room; after experiments lasting some minutes he left the room and another subject came in his place.

The first question studied was the influence of the length of vacant time-intervals between the hearing and recollecting. The intervals examined were 2, 10, and 30 seconds; each of the ten classes was submitted to these three intervals. The subjects' eyes were closed during all the intervals and during the learning of the audible series. The series were never voluntarily remembered during the intervals. For each of the ten contents two series were used with each of the three intervals, with each of the six men, before proceeding to the next content, and these 360 experiments were repeated after an interval of some weeks spent in other researches, etc. The averages represent, therefore, a very similar degree of training.

Viewing the data of all ten classes in a general way, the following errors appear for the three unfilled intervals:

	M.	N.	P.	R.	S.	W.	Av.
2 sec............	14.3%	34.8%	16.3%	30.5%	27.5%	27.5%	25.2%
10 sec............	14.8	38.5	17.0	31.8	30.3	40.5	28.8
30 sec............	18.3	44.5	20.5	34.3	31.3	38.1	31.1

The longer the unfilled interval between learning and recollecting, the weaker is the memory, with the one exception that W. has the greatest percentage of errors after 10 sec. We were unable to study the effect of longer intervals, as even 60 sec.—an interval which passes quite comfortably when filled—appears extremely tedious without filling, and the resulting emotions interfere with the memory processes. If we disregard the visible or audible modes of presentations, and so reduce the ten classes to five, for which the conditions are exactly the same, we get the following averages for the six subjects:

	Numbers.	Colors.	Forms.	Words.	Syllables.
2 sec................	10.0%	13.5%	18.8%	34.4%	49.2%
10 sec................	7.3	20.2	23.5	36.3	56.7
30 sec................	8.6	22.1	23.1	41.7	39.4
Average........	8.6	18.5	21.4	30.5	48.3

In all the intervals the memory is increasingly weaker for numbers, colors, forms, words, syllables—the only exception being the syllables at 30 sec. The large increase in error for the two last groups is due to the greater difficulty with presentations, which do not recur in the various series. The lack of associations explains the fact that the nonsense-syllables are much harder to remember than words. It is remarkable that the numbers are remembered best after 10, worst after 2 sec., and that the syllables are remembered very much better after 30 sec. than after 2 or 10 sec.

If we separate the different kinds of error we find:

	Misplacing.	Forgetting.
2 sec.............	8.1%	14.9%
10 sec.............	10.8	16.2
30 sec.............	11.5	17.5

Both are correspondingly increased with the interval. The third kind of error, to which the words and syllables only were liable, the intrusion of 'alien' presentations, is:

	Words.	Syllables.
2 sec.............	0.4%	1.0%
10 sec.............	0.3	1.5
30 sec.............	0.4	1 7

A further analysis shows that misplacements are much more and increasingly common for numbers, colors, and forms, but less for words and least for syllables:

Numbers.	Colors.	Forms.	Words.	Syllables.
7.0	16.5	18.3	5.7	4.3

Forgetting, on the other hand, is much commoner with the words and is most frequent with the syllables:

Numbers.	Colors.	Forms.	Words.	Syllables.
1.6	2.1	3.5	29.9	43.2

As the syllables were in structure exactly like the words,—two consonants and an intervening vowel,—the difference in error indicates the relative superiority of association bonds for the verbal series. The same factor explains why the introduction of 'alien' syllables is more than three times as frequent as with the words.

The following data give the location of the three kinds of error. The location of the misplacements in the ten places is:

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	2.5	7.5	11.7	12.5	15.7	12.1	10.8	11.3	6.3	2.5
10 sec...	4.2	8.8	12.1	12.9	15.0	16.7	12.9	11.7	10.0	3.3
30 sec...	6.3	6.7	12.1	15.9	15.4	17.9	13.8	12.1	10.0	5.0
Average.	4.3	7.6	12.0	13.6	15.4	15.6	12.5	11.6	8.9	6.2

The location of forgetting:

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	5.8	11.3	15.9	23.8	20.4	14.6	23.8	14.6	12.1	6.7
10 sec...	9.2	12.5	19.6	23.3	24.6	19.6	20.4	14.6	12.5	6.3
30 sec...	7.9	19.6	21.3	24.2	21.3	19.6	26.3	15.0	10.4	10.0
Average.	7.6	14.3	18.8	23.6	22.1	17.7	23.7	14.7	11.6	7.6

For all intervals the misplacements are greatest at the fifth and sixth and least at the first and tenth places. Forgetting follows similar variations, but is greatest at the fourth and seventh places. The location of the wrong words or syllables is very different:

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	3.3	1.3	1.7	2.1	1.3	0.4	0.8	1.3	0.8	0.8
10 sec...	2.9	3.3	2.1	0.8	2.1	0.0	1.3	2.5	2.1	1.7
30 sec...	2.5	2.5	2.9	2.9	0.8	1.7	2.1	1.7	2.1	2.5
Average.	2.9	2.4	2.2	1.9	1.4	0.7	1.4	1.8	1.7	1.7

The errors are here most numerous at the beginning and are least in the middle.

We recorded, as mentioned, the time from the moment when the signal was given for reproducing the series learned till the moment when the subjective content of the memory was discharged. The average time for all observers is for one series of any ten presentations:

After 2 sec.	45.4 sec.
After 10 sec.	47.2 sec.
After 30 sec.	48.8 sec.

The longer the interim, the longer the time for recollection. That means also; the longer the time for recollection, the larger the number of errors. This result is brought out still more distinctly by the individual records. If we place in order the six subjects with regard to the length of time necessary for recollection and with regard to the number of errors for all their experiments, we find:

2 sec.	Time:	M	S	P	W	R	N
	Error:	M	P	S	W	R	N
10 sec.	Time:	M	P	S	W	R	N
	Error:	M	P	S	R	W	N
30 sec.	Time:	M	P	S	W	R	N
	Error:	M	P	S	R	W	N

The foregoing correspondence proves, and later tables will show again, *that the memory which acts quicker acts better;* the number of errors increases regularly with the time used for recollection—a result which seems to be surprising and has no doubt an interesting bearing on pedagogical applications to memory. In the same way the time for recollecting corresponds to the number of errors with regard to the different contents. The order we found for the errors was: numbers, colors, forms, words, syllables. The time for them was:

Numbers.	Colors.	Forms.	Words.	Syllables.
26.6 sec.	32.5 sec.	43.2 sec.	74.7 sec.	81.9 sec.

Unfilled intervals represent a rare and artificial condition for our memory; nearly all our recollecting is done when optical or acoustical impressions fill the interval between learning and reproducing. The following experiments endeavor to submit this question to an experimental test. The time-intervals were 2, 10, 30, 60 seconds. The intervals were filled with either optical or acoustical impressions, rich with associations. The optical filling was secured by exposing printed matter, usually newspapers, vertically on the screen which covered the series, so that the page was read from the moment when the series disappeared. For acoustical disturbance the conductor read aloud newspapers, etc., with sufficient expression to interest the subject, who was sitting with closed eyes. The investigation was made in connection with the preceding one. In order to secure an accurate comparison, the three vacant intervals were immediately followed by the four filled ones, optical and acoustical filling alternating regularly.

The filling of the intervals hinders the memory. The general average of errors for the six subjects is:

	Empty interval.	Optical filling.	Acoustical filling.
2 sec.........	25.2	29.4	34.7
10 sec.........	28.8	31.0	36.0
30 sec.........	31.1	33.0	37.1

It is manifest that the acoustical disturbance weakens the memory more than the optical.

As one half of the series is visible and one half audible, the question arises: What relations exist between the kind of material and the kind of filling? Taking the total results for the four filled intervals, we have:

	Optical filling.	Acoustical filling.
Visible contents......... ...	34.5	33.3
Audible contents	31.4	38.3

The eye-memory is therefore more sensitive to optical disturbances, the ear-memory much more to acoustical. The most effective disturbance to recollection is homogeneous to the sense employed in perception—a fact which will show itself important for the understanding of the psychophysical mechanism of memory. For both fillings the memory shows substantially the same variations with the five contents as it does for the empty intervals. For both fillings and in each of the intervals the hinderance is least for the numbers, and increases for colors, forms, words, and syllables; but while for all the other contents the acoustical filling hinders more than the optical, the opposite is true for the words, as the following table shows:

	Numbers.	Colors.	Forms.	Words.	Syllables.
Optical filling ...	8.0	21.6	27.1	46.6	63.3
Acoustical filling.	13.9	24.9	31.9	42.9	66.6

The special analysis of the two kinds of error shows that the relative amount of complete forgetting compared with mere misplacements increases with the filling of the intervals. Still greater is the increase of 'alien' words and syllables; these occur twice as frequently with the acoustical as with the optical fillings. The location of the errors for both fillings agrees substantially with that for empty intervals.

It is interesting that the recollection-time is largely increased by the filling of the interval, and that also here the time and the number of errors closely correspond:

	Empty.	Optical Filling.	Acoustical Filling.
2 sec....	45.4	48.6	53.9
10 sec....	47.2	54.0	57.1
30 sec....	48.8	56.9	58.0
60 sec....	—	60.2	57.7

And for the different contents:

	Numbers.	Colors.	Forms.	Words.	Syllables.
Empty	26.6	32.5	43.2	74.7	81.9
Optical filling	30.5	43.4	51.1	81.4	110.9
Acoustical filling ...	36.6	46.0	52.9	75.2	117.0

The time for words with acoustical filling is shorter than with optical filling. Even this corresponds exactly to the relations of the errors; and in the same way we find again the parallelism of the order of subjects arranged according to time or to errors. *The quicker the memory is discharged the better is the result, even when the subjective feeling of certainty is the opposite.*

Finally, we made during the whole year the same experiments with intervals of 2 and 24 hours. The experiments were done in the same stage of training as the others, but were less numerous. Each subject made only 80 experiments. A filling of the intervals with purely optical or acoustical impressions was here of course impossible. The subjects were engaged in the ordinary duties of university students, but the 2 or 24 hours never included any other memory or association experiments. The result shows that the number of errors steadily increases with these large intervals:

	Numbers.	Colors.	Forms.	Words.	Syllables.
2 hours....	11.3	27.6	20.6	64.1	50.8
24 hours....	22.3	49.4	37.9	72.5	76.6

The character of the errors is very much changed, as forgetting is now much more common than misplacement. Forgetting surpasses misplacement by 20% for two hours (9.4% *vs.* 29.4%) and by 25.3% for twenty-four hours (13.3% *vs.* 38.8%). The location of error shows no marked deviation from the general law.

I mention, finally, that only one of the six subjects, P., was a strong visualiser. He visualized all contents. M. and N. visualized forms and numbers, R. forms only, S. forms and colors. W. did not visualize at all.

My next communication will give the results of memory experiments on the combination of form and content.

C. The Localization of Sound.

By Hugo Münsterberg and Arthur H. Pierce.

Experiments in late years have conclusively shown that our localization of sound, in respect both to direction and distance, is much more accurate than has usually been admitted by the theories of space-sense which confined themselves mostly to sensations of touch and sight. The explanation of the phenomena, however, especially in connection with the direction of sounds, is still a matter for consideration; and this special consideration stands, of course, in the closest relation to that of the general problem of space. For the same contrasted theories are present here, and it seems not impossible that this very discussion of the phenomena peculiar to auditory space may contribute to the understanding of the general problem.

The essential factors of the mutually opposing theories of sound-localization are the following: First, the auditory sensations that come to the right and left ears are in some way different; and this original difference is the foundation upon which, by means of association, the whole localization is built up (Stumpf). Second, the sound-stimuli arouse special space-sensations in the semicircular canals. The nerves of the canals act like a sense-organ which is stimulated in various portions when the stimulus enters from different directions (Preyer). Third, the localization of the sound depends upon a judgment of the difference of the intensities received by the two ears (v. Kries, Bloch). To these possibilities a fourth is usually added, viz., that the localization is assisted by sensations of touch in the shell and drum of the ear. In opposition to these theories Münsterberg endeavored (Beiträge, H. II. S. 182) to develop the view that the assigning of direction to sounds rests upon the union of sensations of sound and sensations of movement, the latter originating from actual or intended movements of the head in the direction of the sounding body—a theory that carries the Lotzian notion of local signs over into the auditory field. That such movements are called forth reflexly is easily seen in children and animals. With adults, to be sure, the sound is associated so immediately with the presentation of sight that the movement itself, by which the sounding body is brought into the middle of the visual field, is from the very outset inhibited, and only the memory of previously associated sensations of movement is present.

To the theory just mentioned v. Kries, Stumpf, and others have brought forward an objection which touches at the same time the theory of accompanying sensations of touch. V. Kries says: If we localize a tone by uniting it with a sensation of movement, how then is it explicable that we are able to localize two different tones that are strictly simultaneous? If each of the two tones arouses a special sensation of movement, how can we distinguish which sensation of movement corresponds to its particular tone, since the two are not connected with each other? The result to be expected, therefore, with the ear as well as with the eye is that the two directions must be as often confused as rightly recognized. This objection does indeed offer apparent difficulties to the theory of movement- or touch-sensations; but it must not be forgotten that the difficulty is one that belongs not merely to this individual case, but to every single phenomenon of association. When I see two colors and two names of colors arise by association in my consciousness, these two names are not coupled with the localized sensations of color, and yet no confusion occurs. This objection, therefore, points simply to a universal defect in our usual psychophysical theories. Such a theory ought, indeed, to make more allowance for the combining of groups of sensations—this, of course, is not the place to discuss it—but no one denies that such combining really happens, and its existence therefore must be presupposed for special questions. Accordingly, our lack of ability to explain this process of combination on the basis of the usual psychophysical theories ought in no way to be brought up as an argument against this individual case of association.

On the other hand, really serious objections confront the other theories. Preyer's theory, in the first place, has nowhere found approval, and rightly so; for, disregarding the purely physical objections, it is opposed, above all, by the physiological facts which show the almost certain connection between the semicircular canals and the phenomena both of dizziness and of head-movements. Psychologically also the conception of a system of auditory space-sensations is without question untenable. Still more unpsychological is the theory of localization by means of a judgment of the intensities in the two ears. This theory throws us back into the time of the doctrine of unconscious judgments, for there can be no question about the fact that a conscious comparison of the two intensities and a judgment therefrom do not exist. If we hear a sound upon the right side the coöperating of both ears may have some physiological relation to that process which accompanies the conscious localization, since the closing of the left ear would render more difficult the exact localization upon the right. But it is by no means true that two sensations enter into consciousness, one of which placed on the right appears stronger than the one placed on the left. The fact is that the sensation as a whole appears to come from the right.

Stumpf's theory, which makes the sensations originally different in the two ears, manifestly fits the facts much better.

But disregarding the fact that upon this view all the details of a more exact localization must be left to a secondary association, the phenomena occurring when several simultaneous and equal sounds are given are particularly opposed to the theory. Stumpf himself remarks, to be sure (Tonpsychologie, Bd. II. S. 54), that the value of the local relations between the two sides may in such cases coincide. By that, however, he has in mind only the fact that two equal tones coming from both sides are not located at all in the surrounding space, but within the head. As a matter of fact, in the majority of these cases, which have been too little noted heretofore, the sound is located in the surrounding space. Instead of two sounds coming from right and left only one is heard, with a perfectly definite localization; and this localization, which is the product of both tones, appears to consciousness exactly the same as localization which results from only one tone. It is hard to see how such a coöperation of these two original spatial relations is to be conceived. At any rate there is a sharp contrast here to the facts of sight, for there two separate but qualitatively equal lights are never taken for a single light coming from a third definitely located point.

It was for the investigation of these peculiarities that our experiments were arranged, and from this starting-point they were in a position to advance to the most various allied problems, in order to contribute new material for the resolution of these theoretical questions. Of course it cannot yet be said that the question has been brought to a final conclusion; for the investigation showed rather that the conditions are extremely complicated, and every one of the interpretations mentioned offers certain difficulties, or at least never corroborates all the possible consequences that can be deduced from any one of the various theories. First of all, therefore, emphasis should be laid upon the careful examination of the facts themselves.

The apparatus used consisted of a graduated circular metal rim 1 m in diameter, which rested horizontally upon supports, and could be adjusted to any desired height, and of arcs of the same curvature as the rim, which could be adjusted in the median, transverse, or any other desired plane. The subject sat in the centre of the horizontal circle with the ears in its plane. The head was supported from the back by a metal ring. The sounds were given by means of telephones of equal intensity and quality. The telephones could be fixed by hooks on any place on the large rims in the direction of the radius, so that they were directed exactly towards the middle of the line which connects the two drum-membranes of the subject. The wires of all the telephones went to the same commutator and received the secondary current of a small induction-coil, which was in one of Gilman's impermeable boxes. The rustling sound of the coil could not be heard at all therefore, while the telephones reproduced it so that the two or three sounds blended perfectly into a single one. The primary current of the coil worked continuously; the secondary current, which was connected by the commutator with the telephones, had to pass an electric key controlled by the experimenter, usually A. H. Pierce. The telephones gave the sound only so long as this key was closed; for very short sounds, as in Series M., a mercury contact of a swinging pendulum was substituted for the key. The loudness of the sound could be changed by a resistance-box in the electric circuit. The experiments were done during the past two years with twelve subjects—in all many thousand experiments. For all experiments 0° means in front of the subject, 90° r at the right, 180° behind, and 90° l at the left. The subject's ears were carefully tested to discover the possible presence of abnormalities. The answer was always given with closed eyes in numbers of degrees by the subject after hearing the sound.

SERIES A. Two sounds, one on each side, symmetrically placed. Duration one second.—All localizations were at 0° or 180°. The only question is in regard to the conditions which determine the one position or the other. First, individuals differ greatly, some having a constant tendency to locate at 180°, and others having a preference for 0°. R. located all symmetrical sounds at 180°, but remarked that 5° r–5° l was nearer the head than the rest. The same individual differs at different times, and will often place at 0° the same pair of sounds that the day before he had placed at 180°, and that with equal certainty. Secondly, there is usually a certain point at which the sound seems to shift from front to back, or *vice versa.*

Thus B. usually located at 0° all pairs of sound from 10° r–10° l to 80° r–80° l; hence to 150° r–150° l the sound was located now in front and now behind, while all symmetricals back of this were placed at 180°. S. wavered continually from 10° r–10° l to 70° r–70° l, while back of the latter point all sounds were placed at 180°. Two other subjects located at 0° up to 80° r–80° l, back of which they wavered, up to 160° r–160° l. Two others wavered in the region between 60° r–60 l and 110° r–110° l, while all sounds in front of this region were placed at 0° and all behind it at 180°. The general deduction is that all symmetrical sounds in front of 60° r–60° l are most likely to be placed at 0° and those back of 110° r–110° l at 180°. The sound appears always to be at one special point—never in the head, never extended over the whole field, never on the two objective points right and left, never at 0° and 180° at the same time. The result is distinctly against the pre-supposition that the sound-sensations of the two ears are originally different.

B. One sound on each side of the horizontal arc, in all possible combinations of position from 5 to 5 degrees. Duration one second.—The limitations of the localizing power and the entire lack of constancy in the localization of the different individuals are shown everywhere throughout this series. It is highly probable that any table of usual localizations for a given pair of sounds would be valid for the individual alone from whose observations the table was compiled. The differences in the shape of ear and head, as well as the differences in the hair, beard, etc., are so marked and are such important factors in determining any particular localization of sounds within a few feet of the head that no one individual's special localization can be considered a standard for those of another. Several general statements, however, can be made.

For any given point in either of the two quadrants upon one side of the median plane a point can be found in *each* of the two quadrants on the opposite side which in combination with the first will give a localization in the median plane at 0° or 180°. For example, a sound at 45° r will give 0° or 180° not only with its symmetrical 45° l, but also with a sound in the second left quadrant. Thus, for B. 45° r gave 0° with 105° l, for M. with 115° l, for W. with 130° l, for P. with 140°; for N. 45° r gave 0° only with 45° l, but 180° with 130° l, and for R. with 125° l.

But we can state a more general principle, of which the foregoing is only a special case. For any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give the same subjective localization. Thus B. locates 10° r–110° l and 10° r–70° l at 20° l; 50° r–10° l and 50° r–130° l at 20° r; 100° r–50° l and 100° r–150° l at 25° r; 120° r–40° l and 120° r–100° l at 40° r. Similarly with other combinations, and so without exception for all nine observers.

Very similar to this principle is the fact that different individuals, or the same individual at different times, may locate a given combination in two different quadrants. Thus B. locates 0°–110° l at 60° l and again at 130° l; 30° r–110° l at 40° l and 160°l, etc. Or as illustration for the individual differences: sounds at 0°–135° r by B. 25° r, by M. 65° r, by P. 160° r; at 0°–160° r by B. 170° r, by M. 75° r, by P. 10° r.

It is characteristic that every subject feels, at the moment of his answer, perfectly sure that the sound comes from that one point only. It is obvious that the basis of these differences lies in the fact that not only 0 and 180, but also other points before and behind, are confused when they are sounding in a combination. To the example above, for instance, 0°–135° r, the judgment 65° r represents rather the middle; 25° r represents the middle, if 135° r is confused with the corresponding sound from the front at 45°; and 160° r represents the middle, if 0° is confused with 180°. Just so with 0°–160°, 170° r results if 0° stands for 180°; and 10° r if 20° stands for 160°.

All these experiments seem to show that the tactual sensations in any case cannot have any important influence, as they would be entirely dependent upon the objective stimulations, and neither so different under the same stimulations nor so similar under so very different stimuli. On the other hand, just this might be expected for such a subjective function as the impulse to movements, as they of course must be dependent upon a combination of all stimuli and associations.

C. Sounds symmetrical, the intensity of one gradually increased by moving the telephone along a graduated radius nearer the head of the subject.—The subjective sound started at 0° or 180°, and moved to the point of greater intensity. For instance, B. located 90° r–90° l at 0°, and when the intensity of the sound at 90° l was increased, the subjective sound moved from 0° to 90° l, *through the first left quadrant.* On the next day B. located the same combination at 180°, and the subjective sound moved from 180° to 90° l, *through the second left quadrant.* Again, B. located 45° r–45° l at 0°, and when the intensity of the sound at 45° r was increased the subjective sound moved towards 90° r through the *first right quadrant,* while on a later date the same combination was placed at 180°, and moved from that point to 90° r through *the second right quadrant.* On still another date B. located this combination at 0°, and when the intensity of the sound at 45° l was increased the subjective sound was now in the *first* and now in the *second left* quadrant, until finally it settled in the former near 70° l and moved from there to 90° l.

D. Horizontal circle, both sounds on the same side of the median plane.—When the two sounds are in a forward quadrant the resultant may be placed in a rear quadrant on the same side, and *vice versa.* In many cases when the sounds came one from each quadrant different subjects located differently, but always so that it resulted from confusing the sound in the first with a corresponding sound in the second quadrant—10 with 170, 20 with 160, 30 with 150, etc. When both sounds were objectively in such corresponding places it became quite obvious that three localizations were in conflict: the sound appeared either in the middle or at the place of the one or of the other; for instance, 45° r–135° r were located by H. at 45° r, by P. at 100° r, and by M. at 135°.

E. Sounds at 0° and 180°.—The well-known confusion between 0° and 180° is only a special case of the confusion that we have found to exist between the front and rear quadrants. The favorite localization of this combination is at 0°; only two subjects preferred 180°. A change of intensity does not alter the judgment; it remains at 0°, even if the 180° sound becomes stronger and stronger, or comes nearer to the head; and S. placed it nearly always at 180°, even when the 0° sound came nearer to a fifth of the distance. No such simple formula as that weak sounds are preferably placed behind and stronger in front seemed to be supported by the results in this series. All these facts speak clearly against the influence of touch-sensations.

F. Two sounds symmetrically placed on the graduated radius at short distances from the head. Several very different intensities by means of the resistance-box in the electric circuit.—The sound is localized within the head when both sounds are very near the ears, but in front of or behind the head when a few inches distant. Both results are independent of the intensity of the sounds for all subjects. For instance, B. localized within the head the sounds from 60° r and 60° l, 4 cm from the face, even with very faint sounds, but localized 6 cm in front of the eyes when both sounds were 8 cm distant from the face even with very strong sounds. The strong sound 8 cm distant was a much stronger stimulus for the ears than the faint sound 4 cm distant, and the objective intensity was in both cases unknown to the subject. This shows how independent of the absolute intensity the localization is.

G. Three sounds.—The telephones at 0°, 90° r, and 90° l, at equal distances from the centre of the head, are heard by all observers almost without exception at 180°, while 0° alone is more often heard at 0° than at 180°. The three sounds remain at 180° even when the 0° sound comes half-way nearer to the head. Sounds at 0°–90°–180° are mostly located at 90°, sometimes between 90° and 180°.

H. Two sounds on a vertical arc. On the median arc more sounds were subjectively perceived in front of the transverse plane than behind.—Confusions were found similar to those noted by Preyer and v. Kries, one locating in the front quadrant what another would place in the rear quadrant.

In the transverse arc it was most characteristic that all sounds symmetrically placed one on each side of the median plane were almost always located at 180° on the *horizontal* circle, while one sound alone was usually localized in the transverse arc.

J. Only one sound given, but the attention fixed upon a point at the side, without movement of the head.—The experiments were made with very well-trained observers, who made no mistake of 5° for one sound under normal conditions, with the exception, of course, of that between 0° and 180°. B. localized the sounds from 45° r, 90° r, and 135° r correctly without exception under normal conditions; but when the attention was fixed upon 90° r he localized 45° r occasionally at 60° r, 90° several times at 110°, and 135° at 160°. The points on the left side showed quite irregular misplacements when the attention was fixed upon a point at the right. Similarly P., under the same conditions as above, localized 45° once at 60° and once at 65°, 135° at 160°, etc. Were the misplacements a purely psychical illusion resulting from prefixed attention, the sound from 135° r ought to have been heard nearer to 90°, not farther away from it; and 90° itself ought never to have been misplaced. As we find that on the right side all the misplacements are in the direction of 180°, while those on the other side are irregular, it shows that the directing of the attention to 90° has the influence not of bringing the sound nearer to 90°, but of increasing the angle on the whole right side. This is easily explicable on the ground of an additional motor impulse, but not on that of a comparison of intensities.

K. One sound. Eyes blindfolded and turned to 45° r or 45° l.—The results differ for different observers. Some show only an increased uncertainty and irregular misplacements, while three observers show a regular tendency to place a sound on the same side 10° or 15° more to the rear. There is nowhere a marked tendency to bring a sound between 45° and 180° nearer to 45°. The tendency is rather towards 180°. In the case of three persons the eye-movements seem to bring in an additional motor impulse, while the other observers are disturbed only by the unusual and prolonged fixation with closed eyes.

L. The muscles of one side of the body strained by voluntary effort.—The result is a marked tendency to locate the sounds on that side farther away to the rear. P., with a strain to the right, localized four fifths of the sounds on the right side 10°–20° more to the rear. B. did the same in more than half of the cases, while a misplacement in the opposite direction happened only once.

M. The sound very short, the electric current to the telephone being closed by a swinging pendulum.—With regard to the direction the results for two or three sounds are just the same as with long sounds. It was sometimes necessary to repeat the sound before a judgment was possible. The judgments for one sound show more misplacements of 5°–10° in both directions. There is no tendency to localize very short sounds before or behind.

N. One ear fatigued by continual stimulation, the telephone with its strongest sound being pressed to the ear-shell for several minutes.—The stimulation becomes finally extremely disagreeable, even painful. Immediately after stopping the same sound moves from the right side or from the left side along the rim, and the observer gives a signal when he hears the sound at 0°. The result is for five subjects that the fatigue has no influence at all; 0° is exactly recognized. The same result is reached when, immediately after fatiguing, one sound on each side is given. The combination is localized as under normal conditions, and when the two sounds are symmetrical is always heard at 0° or 180°, and not on the side of the unfatigued ear. This localization towards the unfatigued side occurred sometimes with the subject Mü.

In close connection with the above a further question concerned us—one indeed of an essentially physiological nature, but, like so many questions of nervous physiology, one that must be answered from psychological resources. Our experiments and the psychological analysis of them show that a conscious relation of the tones to either of the ears does not exist; that sensations of touch play no essential rôle; and that one cannot speak of a judgment of the difference of intensities in the two ears; while many things go to show that the accompanying sensations of movement are to be regarded as the psychological basis of the auditory spatial relations. The sensations of movement point to motor impulses, and it is a physiological question where these impulses are reflexly called forth. Several hypotheses are conceivable. Münsterberg's theory suggested as a side issue the possible relation between the movements and the semicircular canals. This was supported by the spatial arrangement of the canals, by their relation to the acousticus, and above all by the physiological fact that stimulation of the canals causes movements of the head. The chief objections to this hypothesis lie in physical considerations. Another hypothetical possibility would be that the impulses to movement are set free in the brain through the action of the two-sided auditory stimulus. As erroneous as it is on the one hand to believe that a comparison of sound-intensities takes place, just so possible is it on the other hand to suppose that the difference in the physiological excitation of the two ears conditions the motor impulses with which the head reacts physiologically to the sound. At the same time it can be conceived that the stimuli of touch and pressure as well as associations influence the direction and intensity of these central impulses. Both hypotheses are compatible with Münsterberg's theory of sound-localization by means of sensations of movement. With a view to the possible settlement of these questions, experiments were arranged to investigate the localization of sound during and after movements of rotation. Since there seems now to be no doubt, especially after the investigations of James, Kreidl, and others, that the semicircular canals are the organ whose stimulation produces dizziness, it must be true that if the canals are indirectly connected with the localization of sound the latter will be changed and disturbed during dizziness. For these experiments all the apparatus formerly used was fastened to a large, horizontally rotating disk, which by means of a belt could be brought into very smooth rotation of any desired rapidity. The chair was so fastened that the subject revolved exactly about the axis of the head and trunk, while the telephones retained their position 50 cm distant from the head and could be sounded during or after the rotation as desired. The observations upon dizziness in general made incidentally during these experiments will be given in another connection. Here we confine ourselves to dizziness in relation to the localization of sound.

The revolutions were relatively very rapid in order to produce strong effects. On the average ten rotations took place in from fifteen to twenty seconds, the rapidity increasing at first and then diminishing. The rapidity of the rotation could be registered upon the drum of the kymograph. The experiments were extremely disagreeable to the subjects, and often left after-effects that lasted for hours. Accordingly we were obliged to confine the investigation to the more simple questions. Rotation in the direction of the hands of a watch is designated positive, that in the opposite direction negative. The fundamental phenomena of dizziness itself corresponded, of course, to what is already known from the excellent experiments of Mach, Delage, Aubert, and others. If the eyes were kept closed during the rotation and then opened when it ceased, the body itself appeared to be at rest, while the visual field moved in the direction opposite to that of the rotation. If, on the other hand, the eyes were kept closed after the rotation stopped, the after-effect was the feeling of a lively rotation of the body itself, likewise in the direction opposite to that just given. With many of the subjects this took place in a vague way; with others the number of the reverse rotations could be counted up to eight and more. It is clear that there is an apparent contradiction between the illusory movement of the body itself when the eyes are closed and of the visual field when the eyes are open, since they both take place in the same direction. The feeling that the body is rotating stops at the moment when the eyes are opened. These facts are already known from the works of Delage and others. It is also well known that there is a tendency to turn the head called forth reflexly during the real or imagined rotation, for the purpose of compensation. Many subjects feel a marked straining in the muscles of the neck on the side in question. The interesting results with the localization of sound were somewhat as follows:

If the sound of a telephone was given for a second during the objective rotation, it was *usually misplaced in the direction opposite to the rotation;* it was rarely put at the right place, and in the limited number of our experiments was never misplaced in the direction of the rotation. E.g., negative rotation: objective sound at 45° r., subjective 135° r.; Bu., obj. 135° l., subj. 90° l. Positive rotation: P., obj. 135° r., subj. at first 70° r., this changing slowly to 110° r., etc.

If the sound of the telephone was given immediately after the cessation of the objective rotation, the eyes being still closed, it was for the most part *widely misplaced in the direction of the rotation just given.* With this question the greater part of our experiments were especially concerned. The result was that in 82% of all the cases the misplacement took place in the direction of the preceding rotation; 14% were correctly localized, and 4% were misplaced in the direction opposite to the rotation. The latter, however, were also cases which can be looked upon as misplacements in the direction of the rotation, if the well-known confusion between front and behind is considered. The following examples may serve here: After positive rotation—P., obj. 90° r., subj. 125° r.; obj. 100° r., subj. 140° r.; obj. 45° r., subj. 60° r. moving to 80° r; obj. 125° l., subj. 90° l., etc., in short positive misplacements throughout: then obj. 180°, subj. 45° r. moving to 25° r., which is evidently no negative misplacement, but rests rather upon the confusion between 180° and 0°. Bi., obj. 110° r., subj. 160° r.; obj. 90° r., subj. 150° r.; obj. 90° l., subj. 20° l., etc. With negative rotation the misplacement was exactly the reverse. P., obj. 90° l., subj. 135° l.; B., obj. 30° r., subj. 0, etc. The experiments upon eight persons agree perfectly in this respect. *The misplacements result therefore, during and after the rotation, in the direction of the compensatory impulses to movements of the head,* which in reality indeed produce a strain of the muscles, but no actual movement (since the head is supported from behind), and which therefore do not alter the relation of the ears to the surrounding body.

It seems that these results indicate above all that the localization of auditory sensations rests upon sensations of movement and not upon the comparison of auditory intensities. The relation of the intensities immediately after the rotation is exactly the same as under normal conditions, and yet an almost constant misplacement takes place in the direction opposite to that of the subjective illusory movement, and that too from 30° to 50°—a degree of misplacement that could not possibly be conditioned by the purely auditory factors. Further, it is not a general feeling of dizziness that renders orientation difficult; for otherwise the localization would not be so definite, and above all would not be in a constant direction. Still further, it does not rest upon an imperfect orientation of the head to the body, since the position of the sound is not designated by the finger, but is given in terms of degrees. If therefore the judgment, '45° r.', is associated with a definite ratio of sound for the two ears, why should the same ratio be judged during the positive rotation as, perhaps, 20° r. and after the rotation as 80° r.? If, however, we suppose that the localization rests upon sensations of movement, it is at once clear that an increase of the impulse by means of the compensatory reflexes produces the illusion of a constant misplacement.

The localization is independent of the misplacements in the visual field, which as is known originate from nystagmus of the eyes, and which have been widely studied since Purkinje. If the telephone was concealed by a screen, so that its position was not seen, and the eyes were then opened after a positive rotation, the visual field made a negative rotation, while the sounding body did not appear to shift at all. *The fixedness of the auditory localization can indeed influence the optical impression.* When the sounding telephone was visible it seemed to the subject several times that the whole room was turning, but the telephone itself remained alone unmoved. If after the rotation, but while the eyes were still closed, the sound was given continuously for a time, it seemed to make the illusory movement too; it remained, that is, in constant orientation with the body. E.g., Mü. after fourteen rotations had the feeling of six reverse rotations, during which the sound followed continuously like a buzzing fly. It is indifferent whether the person himself or the visual field seems to turn: the sound is localized in every case in reference to the head, and that too with a displacement dependent upon the direction of the compensating strain.

As strongly as these experiments support the genetic view of localization, just as little, finally, do they say about the physiological apparatus which calls forth the impulses to movement under the influence of the sound. Had we found no influence at all from the rotation, or had we found a complete lack of orientation, the result would have had but one meaning. The above results, however, admit of several interpretations. The compensatory movements of the head are without question given by the canals, but whether the additional impulse from the sound has the same or a different source is not shown. The stronger impulse to movement can originate from the stronger stimulation of the semicircular canals, or it may be that the canal stimulation given by the rotation receives an added impulse from another possible source—viz., from the dissimilarity in the stimulation of the two ears. In either case this remains finally a physiological issue which has no essential interest for the psychologist. The psychological question can be only this: Does the localization of auditory impressions consist in the addition to them of sensations of movements, or not? And the positive answer to this question has received a new and unexpected support from the experiments made with the rotating chair.

D. Association. (I.)

By Mary Whiton Calkins.

The investigation, an account of whose beginning is given here, was undertaken as an attempt to answer experimentally the question of the relative significance of *frequency*, *vividness*, *recency*, and *earliness* as conditions of association. The experiments were carried out in 1892–93 and in 1893–94 in the Harvard Psychological Laboratory. The subjects were ten regular students of the laboratory, with an average of 80 experiments each; besides this, the same experiments were repeated with 25 members of the writer's Wellesley College class in experimental psychology with an average of 16 experiments each. In these 1200 experiments, here discussed, is not included the long series of preliminary experiments, which was made with a view to finding the best methods, and especially to give the subjects a fair degree of practice and training in this special work. None of the subjects knew during the experiments anything about their points and purposes; they gave their attention therefore equally to every part of the experiment. It is obvious that a previous knowledge of the purpose of the experiment on the part of the subjects would seriously interfere with the value of its results. The whole question is surely one of those for which the statistical method surpasses the method of individual analysis. The calculation has to be based, therefore, on a combination of all experiments, and as those of the Harvard students generally coincide with those of the Wellesley students, the following tables will combine both.

The experiments here reported represent only one of the two main types which we selected. The one type was acoustical, the other optical. In the acoustical experiments the associated elements were nonsense-syllables and numerals, both pronounced; the optical experiments employed colors and numerals, both shown to the subject. The twelve hundred experiments which I discuss at present *were all of the optical type.* Their method was briefly as follows: The subject sat before a white screen large enough to shield the conductor of the experiment. Through an opening, 10 cm square, a color was shown for four seconds, followed immediately by a numeral, usually black on a white ground, for the same time. After a pause of about eight seconds, during which the subject looked steadily at the white background, another color was shown, succeeded at once by a second numeral, each exposed for four seconds. The pause of eight seconds followed, and the series of 7, 10, or 12 pairs of quickly succeeding color and numeral was continued in the same way. At the close the subject at once saw a series of the colors, but in altered order, and was asked, as each color appeared, to write down the suggested number, if any such occurred. The time was kept by following the ticks of a watch suspended close to the experimenter's ear. Color and numeral were placed together in their position behind the opening of the screen, the numeral at first concealed by the color, which was then slipped out. There was thus a merely momentary pause between color and numeral. During the eight-second pauses the opening was filled by a white ground, ½ cm behind the screen. The subject thus did not see anything in the opening except this white ground or the color, which filled the whole square or the large printed number of two digits; the moving fingers, etc., could not be seen at all. The whole series, of course, was always carefully prepared, and placed in order beforehand.

Each series was arranged to present some one color once only, neither at the very beginning nor at the very end of the series in connection with any numeral, and to present this same color also in some emphasized combination. Such combinations were either of frequency (in this case the color was two or three times repeated with another numeral) or of recency (in this case the color occurred at the end of the series) or of vividness (here three methods were used, which will be described later). I copy actual descriptions of series of the three main varieties:

Series 89. *Frequency* 3 : 12.

Position : First series, frequent 5, 7, 9; normal 3. Second series, 4.

I. Medium gray, 29; blue, 82; violet, 61 (n); red, 23; violet, 12 (f); peacock, 79; violet, 12 (f); strawberry, 47; violet, 12 (f); light brown, 53; dark gray, 34; light green, 72.

II. Peacock, red, green, violet, medium gray, brown, strawberry, dark gray, blue.

Series 38. *Recency.*

Position : First series, recent : last, normal 2. Second series, 2.

I. Light violet, 25; light gray, 14 (n); medium green, 47; brown, 73; red, 28; light brown, 54; light gray, 32 (r).

II. Brown, gray, green, violet, red, light brown.

Series 178. *Vividness.*

Position : First series, vivid 3, normal 7. Second series, 3.

I. Dark red, 24; dark green, 40; blue, 783 (v); orange, 57; light peacock, 15; brown, 82; blue, 61 (n); gray, 29; strawberry, 78; dark violet, 36.

II. Peacock, red, blue, violet, orange, brown, gray, green, strawberry.

To such a series the definite question of the experiment is of course the following: In what proportion of cases will the accentuated color, e.g. violet (as in series 89, just quoted), suggest the numeral—here 12—with which it is repeatedly (or vividly or recently) combined instead of the other numeral—here 61—with which also it appeared. This comparison of the suggestiveness of a color in frequent, in recent, or in vivid combination with its power to suggest when it is only once and unemphatically connected with a numeral, shows the value of frequency, of recency, and of vividness as factors of association; it is at the same time preparatory to our ultimate purpose—the determination of the comparative value of these conditions of association.

To gain a basis of comparison about six hundred of the series have been studied as a mere memory test, leaving out of account for the time being the frequently, recently, or vividly combined numerals which they contain. Roughly speaking, about one fourth of the ordinary combinations in the longer series (10 to 12 pairs) and one third in the shorter series (7 pairs) are remembered.

TABLE I. CORRECT ASSOCIATIONS.

	Number of Series.	Possible Correct Associations.	Actual Correct Associations. Full.	Half.	%
Long series (freq., viv.)....	591	5291	1190	401	26.3
Short series (rec.).........	175	700	210	68	34.8

The tabulated results of the experiments on frequency as a condition of association are as follows:

TABLE II. FREQUENCY.

	Number of Series.	Both.			Normal Alone.			Frequent Alone.		
		Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
Freq. 3 : 12....	216	32	4	15.7	9	15	7.6	100	6	47.7
Freq. 2 : 12....	143	16	7	13.6	8	16	11.2	29	3	21.3

The table shows the number of those cases in which both numerals were recalled, then the number of cases in which the color suggested only the numeral with which it has been but once associated, and in the last group the number of times in which the color suggested only the numeral with which it had been three times or twice combined. Under the heading 'Half' are given those cases in which one digit of the numeral was recalled, and in estimating per cents these cases are rated as half correct. The comparison of the frequent with the normal shows that with a frequency of one fourth the frequently-associated numeral is recalled 63.4% (47.7 + 15.7), the normal one only 23.3% (7.6 + 15.7), and the frequent is recalled more than six times as often as the once-associated numeral in those cases in which one numeral only is suggested. The comparison of both these per cents with that representing the likelihood of recall for such long series (Table I) leads to the same result. The frequently-associated numeral is remembered more than twice as often (63.4% instead of 26.3%), while the other numeral is remembered less than the average (23.3% instead of 26.3%). This latter result shows the negative result of habit, since the effect of habitual association with a given stimulus is seen to be a small decrease of the likelihood of ordinary association with the same stimulus.

It seems very remarkable how much this influence of frequency is lowered when the frequent association is only twice repeated instead of three times. The second line of Table II gives the results. While the negative influence of the habitual association on the once-associated numeral becomes a little smaller (it is recalled 24.8% instead of 23.3%), the positive influence of the repetition decreases rapidly; the frequent numeral is recalled 34.9% (21.3 + 13.6); that is, only 8.6% more than the average of ordinary associations without repetition, and 28.5% less than the three times repeated associations.

In the attempt to fix a rate of associative recency series were used varying in length from 4 to 7 pairs; the color in question was always last in the first series and second in the following series. Only the series of 7 pairs proved suitable to the purpose, for in the shorter ones both numerals were usually remembered, so that the comparison became impossible.

TABLE III. RECENCY.

Number of Series.	Both.			Normal Alone.			Recent Alone.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
54	5	2	11.1	5	2	11.1	21	4	42.6

The last of the series is thus recalled 53.7% (42.6 + 11.1), the other numeral associated with the same color only 22.2%. We saw that the average for series of 7 pairs was 34.8% (Table I). The likelihood of recall increases, therefore, by the recency of the position 18.9%, while the negative influence on the second associated numeral is 12.6%.

The next table summarizes the records of all the experiments in vividness, separating the results according to the different devices used to make the combinations vivid. Since the color remained the same the result could be gained only by varying the numeral, which was accordingly either black of two digits but much smaller than the other numerals, or black of usual size but of three digits, or of usual size and of two digits, but red or of usual size, but of three digits and red.

TABLE IV. VIVIDNESS.

Nature of Vividness.	Number of Series.	Both.			Normal Alone.			Vivid Alone.		
		Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
Black, 3 digits.......	147	9	6	8.2	11	2	8.2	63	4	44.2
Black, 2 small digits.	102	7	2	7.8	12	6	14.7	21	3	22.1
Red, 3 digits........	132	11	5	14.7	11	0	8.3	39	18	36.3
Red, 2 digits........	159	18	6	13.2	12	4	8.8	53	21	39.9
Total vivid......	540	51	19	11.2	46	12	9.6	176	46	36.8

Regarding the totals only we find that 48% (36.8 + 11.2) of the vividly associated numerals are recalled and 20.8% of the normal associations of the same colors. The general average for series of this length was 26.3%. The vivid, like the habitual and the recent association, obliterates therefore the ordinary association. On the other hand, the increasing influence of the vividness (48%) is by far not so strong as that of frequency with three times repetition (63.4%), but stronger than two repetitions (34.9%). The comparison of the different forms of vividness shows an interesting preponderance of associating three-place black numerals (52.4%) over associating two-place black numerals (29.9%). Since the latter, by reason of their quite unaccustomed small size, were decidedly impressive visually, it seems not unlikely that this difference is due to the fact that the numerals of three digits introducing as pronounced the word 'hundred' offer important aid to articulatory memory whose method is the repetition of the word. The effect of vividness is also shown by the relatively great number of cases in which numerals associated with bright colors are remembered compared with those in which the color was dark or indifferent.

A fourth kind of prominence can be given by the earliness of the association, especially by its position at the very beginning of a series; the term 'primacy' may be used. We found for all series of 12 pairs an average recollection of 26.3%, for the three-times repeated associations 63.4%, for the vivid associations 48%, for the twice repeated 34 9%. The total result for the influence of primacy is 33.6%—that is, 7.3% more than the unemphasized association. These 33.6% of primacy-associations are the result for those series in which the color of this first pair was given in no other combination; therefore no negative influence of the competition with frequent or vivid associations existed. This competition, on the other hand, is brought out in the following experiments. At first a direct comparison of this factor of primacy with that of frequency was made by showing shorter series, in which the same color appeared with one numeral in the first place of a series, and was then twice repeated with another numeral.

TABLE VI. FREQUENCY AND PRIMACY.

Number of Series.	Both.			Primacy.			Frequency.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
60	13	2	23.3	3	2	6.6	32	1	54.2

The frequent is recalled 77.5%, the early only 29.9%. The record leads to the significant pedagogical conclusion that early associations, in spite of their tenacity, may be replaced by later ones if they are sufficiently repeated.

The influence of recency, too, can be studied in those series which were arranged without this immediate end in view; that is, in the frequent and vivid series. The recent—that is, the last of the series—is here again in no competition with others; the general result for all these series is here only 22.9% for recency, still less than the average. This decrease is evidently the result of fatigue; the twelfth pair is not observed with the same attention as the earlier ones, as we saw that the last pair in a series of seven pairs was recalled 53.7%. In any case those 22.9% indicate clearly the relative unimportance of the recent connection. Direct comparisons of recency with vividness and with frequency tend to the same result; they show an undoubted preponderance of the vivid or frequent numeral over the recent. The series was composed of seven pairs only.

TABLE VII. RECENCY AND VIVIDNESS.

Number of Series.	Both.			Recent.			Vivid.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
60	13	4	25	6	2	11.7	13	10	30.0

The vivid numeral is recalled 55%, the recent only 36.7.

TABLE VIII. RECENCY AND FREQUENCY.

Number of Series.	Both. Full.	Half.	%	Recent. Full.	Half.	%	Frequent. Full.	Half.	%
62	19	6	35.5	7	1	12.1	14	6	27.4

The twice-repeated numeral is recalled 62.9%, the recent one only 47.6%.

These last experiments must be continued in order to present a broader basis for conclusions. All the experiments together suggest, however, the hopeful probability that vivid or multiplied lines of association may be established in the individual consciousness, firm enough to withstand the force of the recent and the accidental, and powerful enough to counteract the pressing influences of the environment.

Æsthetics of Simple Forms.

(1) SYMMETRY.

By Edgar Pierce.

The experiments I am about to describe were carried on during the last two winters in the Harvard Psychological Laboratory. They in some way form a connected series. So I shall begin with the more simple and continue later with the more complicated. But first of all I must describe the instrument used.

The aim of this instrument is to furnish a uniform black surface, on which various forms or lines can be moved by some device which shall not disturb the plain black background desired. Moreover, means for recording the position of the lines are necessary. To meet these requirements we have a surface of hard rubber about 1 m square; this surface is covered with black cloth. Two slits are cut straight across the board from side to side; these slits are 5 cm apart, and are so narrow and so carefully finished with cloth and black velvet on the back that no light can pass through; yet they are wide enough to allow a thin piece of tin to be inserted. Now suppose you take a piece of tin 10 cm long and 1 cm wide, and bend at right angles but in the same direction sections of this strip 2.5 cm from each end. These bent ends can now be put into the two slits and the effect will be that of a line of tin 5 cm long resting on the black surface. Now if the two ends be so arranged that they can be attached to a slide moving on a track on the back of our board, and if the track be divided into millimetres, we then have the means of moving our line along the board from one side to the other and of recording the exact position at any moment. Of course any form may be attached to our piece of tin, and the number of slides may be increased to as large a number as desired,—we used six slides,—thus furnishing the means for an unlimited number of complicated arrangements of forms or lines in one plane along one line. Moreover, the whole instrument is fixed on a stand in such a way that the whole board can be turned in any direction. So that the slit across the centre can be made to run horizontally, vertically, or obliquely, at any angle. By an arrangement of pulleys on the side of the board it is possible for one to sit at a distance from the board in front of it and by small strings to move the lines as one pleases. The instrument stands in the dark room, which is painted black throughout; an artificial light placed at the side of the subject furnishes illumination; the eyes of the subject are protected by a black screen between him and the light.

At the present time I wish to treat the experiments from one point of view only—in respect to the æsthetical feeling of symmetry. More especially I wish to study the effect of contents of an unlike nature appearing on either side of some point regarded as a centre; for instance, the same form on both sides but different colors, or lines of the same length but not of the same breadth, and especially how far differences in content may be compensated for by other variations; for example, differences in length of lines by distance from the centre, difference in colors by variations in length, etc. The question then is: Can a feeling of symmetry, that is, of æsthetical equality of the two halves, remain when the two sides are not geometrically identical, and if so, what are the conditions under which this can result—what variations of one side seem æsthetically equal to the variations of the other side? It is clear that our instrument is fitted for an experimental study of these questions. Take the simplest case. We have in the middle of our black field one large white vertical line 20 cm long; on the right of it is a white line of 10 cm at a distance of 20 cm; on the left is a movable line of only 5 cm. If now the movable line be placed at the same distance from the centre as the fixed line,—that is, 20 cm away,—our feeling of symmetry is not satisfied, because the lines on each side are of different lengths; but if the movable line be pushed farther from the centre a point is finally reached where the arrangement pleases, just as if the greater distance of the short line were a substitute for the greater length of the other line. Our feeling of symmetry is now satisfied, although the figure is divided into parts of very different length.

An experiment of this sort seems to be open to an objection. It may be said that the final result pleases not because the parts are æsthetically equal, but because they are in a certain pleasing proportion one to the other, but not that of equality. I refer to the well-known doctrine of the golden proportion, which rightly holds that the division of a whole into two unequal parts according to a special proportion is very pleasing. It may then be said that the impression that our figure pleased through the feeling of æsthetical equality is illusory. Indeed, thè very careful experiments of Dr. Witmer have shown what great influence those proportions which roughly correspond to the golden section have on our æsthetic feelings, and how strongly they compete in the division of a horizontal line with the symmetrical division. Our instruments readily furnish the opportunity for corroborating this. If we took three vertical white lines 10 cm long by 5 cm wide, fixed two of these lines 60 cm apart and had the middle line movable, every one of six subjects chose as most agreeable a position for the third line roughly corresponding to the golden section: here two equal halves appear too monotonous, and something must be done to give variety. The golden proportion seems to be this, and yet to give some unity as well.

In view of these facts our first question must be under what conditions this pleasure in the golden proportion is in competition with that undeniable pleasure we get from equality and repetition. The following series of experiments was therefore undertaken with six subjects. I used not only three, but four, five, six, seven, and even eight such vertical lines 10 cm long: the question was to divide a distance which was one half, one third, one fourth, one fifth, or two thirds or three fourths or two fifths of a larger distance, the other parts being represented by one or more lines. For instance, the lines were fixed 0, 20, and 60 cm, respectively; the movable line must divide the space of 40 cm between 20 and 60 in the most agreeable way; or four lines fixed at 0, 15, 30, 60, respectively; a fifth movable between 30 and 60; or five fixed at 0, 12, 24, 36, 60, and a sixth movable between 36 and 60; or three fixed at 0, 15, 60, the fourth between 15 and 60; etc. The part to be divided was alternately on the right and on the left; the movable line in this series moved in half of the experiments slowly from the right, in half from the left. The subject, who sat at 4 m from the field, the eyes at the height of the lines, was asked to stop the movable line when it reached the most agreeable position. It will be seen that the movable line in all cases divided a space into two parts; this space, however, was a part of a more complicated figure which suggested symmetry. The question was to what extent the pleasure in the golden proportion which controlled the division of the simple space would enter into competition with the pleasure in symmetry. I confine myself here to a mere outline of the general results, as these experiments were only preparatory. The results for all six subjects for the right and left position agreed in the general tendency. If there are more than three lines the tendency to a symmetrical arrangement quickly increases. The preference for the golden proportion is finally given up. Sometimes exceptional cases occur where quite irregular forms are chosen; this is due either to a conscious association, or to the desire to break up the monotony of the figure, but not to the preference for any proportion such as the golden section. The tendency to symmetrical arrangement is strong with four lines. Still more so with five. With six or more lines the tendency changes again; the pleasure in symmetry decreases and the demand for variety increases. The explanation of these results seems clear: the pleasure in these simple forms is due to intellectual enjoyment of unity and variety. Variety increases with the number of the parts, and with four or five lines is sufficient if all the lines are placed symmetrically, especially as the attention fluctuates in apperceiving the one or the other of these parts. For two parts the lack of variety is annoying, and suggests that unequal division of the golden section which gives variety, but which yet seems to give sufficient unity, for the two parts are still regarded as making one whole; three or more unsymmetrical parts, however, lose their unity. When we come to more than six lines we find the same conditions as with these: there is unity but no variety, for the parts are so near to each other that the object appears like a fence; no parts are discriminated, and the result is a demand for some irregularity, while the number of symmetrical parts is sufficient to give unity. The general result then is: the principle of unity is the more important and, in fact, is the only one where the parts give variety; the pleasure in the golden section as giving variety is apparent only where the lines are so few or so many as to give monotony, but even in these cases unity is easily apperceived. Thus in our future experiments if we offer variety of form and content by lines of different colors, length, and breadth, and if any æsthetical pleasure results, it must be the pleasure of unity, not analogous to that obtained from the golden section, which is more essentially variety. Where the content is so varied, only an arrangement which gives unity will be pleasing; and when the figure consists of two halves, the pleasure must be a feeling of æsthetical symmetry.

I may mention in passing that I made the same experiments in a vertical position; the results were very different from the horizontal: associations seemed to overwhelm the elementary æsthetic principles. The bottom was always of a different value from the top, and symmetry evidently played a very subordinate part. The principle here seems to be that of stability; the distances between the lines here suggest the idea of masses, and the effect must be stable, and not seem as if it were going to topple over.

We now come finally to the chief question, whether or not there is any substitution for form, color, size, etc., which will satisfy the feeling of æsthetical symmetry, and how far this substitution, if there is any, suggests an explanation of our sense of symmetry. The first experiments were done with very simple material. A white line 20 cm long was fixed in the middle; on one side a white line 10 cm long, 1.5 cm wide, 8 cm from the centre; on the other a movable line 1.5 cm wide, but only 5 cm long. At what distance will the movable line be placed in order that a feeling of balance, of æsthetical symmetry, may result? The general average for all the subjects is 24.2 cm, the minimum 15.9, the maximum 29.1. The movable line proceeded alternately toward and from the central line. The question asked the subject was, When do you like the movable line best? After they had decided they were then asked if the figure gave any feeling of symmetry or balance. With the few exceptions in which associations influenced the judgment, the answer was always in the affirmative for all the experiments.

In the next group, still with white lines only and with the same central line of 20 cm, there were two lines on each side—on one two lines of 10 cm each, on the other of 5 cm. The 10-cm lines were 15 and 20 cm from the central line. One of the 5-cm lines was also 15 cm from the centre; the other 5-cm line was alone movable. The preferred average position was 33.7 cm, minimum 29.7, maximum 38.0. It is obvious that here a greater distance is the æsthetical substitute for length of line.

There is not such a great uniformity in the results when the lines are of the same length but of different area. For example, we had on both sides lines of the same length, but on one side 1.5 cm wide, on the other 0.5 cm wide. Some of the subjects are inclined also here to substitute greater distance for greater area; others seem to abstract from the difference in area.

As these introductory experiments proved that our method of experimenting is valid, and that the answers to the questions were given with subjective certainty by the subjects, we went on to more complicated experiments, in which not only different forms but also different colors were introduced. In describing these experiments a more detailed account must be given than was necessary in the first series. Seventeen groups of experiments with different forms were made, and in each case the form appeared in six different colors. Five subjects made the whole series—Messrs. Buck, MacDougal, Rogers, and Shipp, and the writer.

The constant centre in all these experiments consisted of three vertical lines arranged symmetrically; in all cases the central line was white and 1.5 cm wide; in groups I–XIII it was 30 cm long, in groups XIV–XVII 5 cm long. In all the groups there were also two blue lines each 12 cm distant from the middle line; they were 10 cm long, 0.5 cm wide. This large centre suggested of course much more strongly than the white line alone a comparison of the two halves from the point of view of æsthetic symmetry. Beyond these constant lines there was always on the one side a fixed line, on the other a movable figure or line of some sort. In groups I–IX the fixed line was 10 cm long, 1.5 wide, 12 cm beyond the blue line. This fixed line was in half the cases dark blue, in half light red; this was done to counteract the effect of color. The movable part on the other side was variable, the following forms being used: a line 10 cm long, 1.5 cm wide; a line 10 cm long, 0.5 cm wide; a line 5 cm long, 1.5 cm wide; two lines 10 cm long, 1.5 cm wide, 5 cm apart, moving together; two lines 5 cm long, 1.5 cm wide; a square with sides of 5 cm, a star of 5 cm diameter; a square frame with sides 1 cm wide, 5 cm long; a square on end, with diagonals vertical and horizontal. In groups X–XIII the same arrangements of constant lines was used, with the addition of five lines of different colors and sizes in the space of 12 cm between the blue line and the outer constant line; the movable parts on the other side were again a 10 cm line, a square, a star, or a square frame of the same dimensions as in the first series. In groups XIV–XIX the arrangement is the same as for the first group, that is to say, without the filling of the interval, with the one exception that the central line was 5 cm long, not 30 cm. The movable parts were again a line 10 cm long, or two lines 10 cm, a square, or a star. In each of the 19 groups the lines, squares, stars, or frames appeared in six colors—white, blue, red, orange, maroon, and green.

At first we shall examine the results from the point of view of the influence of form only, leaving the question of color until later. In the first group every number is the average of 36 experiments for each subject; in half of them the constant line on the right is blue, in half red; the movable line is given six times, once in each of the above-named six colors. In the other groups most of the numbers are averages of only 12 or 18 experiments, but here also the forms were given in the six colors. The experiments covered the whole winter 1893-94, so that each subject repeated all the experiments several times after a considerable interval.

The numbers give the average of the distance of the movable form from the left blue line. With squares, stars, double lines, the number means the distance of the middle point.

I–IX. Centre line 30 cm. On the right constant line 10 cm long, 1.5 wide, 12 cm distant. On the left movable:

I. One line 10 cm long, 1.5 cm wide. P. 18.3; R. 13.7; M. 16.9; S. 18.0; B. 15.8.

II. One line 5 cm long, 1.5 cm wide. P. 19.7; R. 14.1; M. 20.1; S. 20.4; B. 16.1. It is obvious that all five subjects place the shorter line farther away than the longer.

III. One line 10 cm long, 0.5 cm wide. P. 18.6; R. 13.9; M. 19.7; S. 19.8; B. 16.4. Also here the line is a little farther out than in I. The line here is thinner, not shorter, but the substitution still occurs.

IV. Square of 5 cm. P. 17.0; R. 14.0; M. 14.6; S. 15.8; B. 13.2. If we compare IV with II, a square of 5 cm with a vertical line of 5 cm, we find the square nearer to the centre by an average of 3.0 cm, and as this is the middle point of the square its inner edge is nearer to the centre by an average of of 5.5 cm. The star, square frame, and square on end are farther out than the square, as the following figures show:

V. Star. P. 18.5; R. 14.1; M. 17.4; S. 16.6; B. 16.6. On the average 1.7 cm farther out than the square.

VI. Square frame of 5 cm. P. 17.4; R. 14.4; M. 16.1; S. 16.1; B. 14.2.

VII. A square on end. P. 17.4; R. 15.1. M. 14.8; S. 16.1; B. 14.7. The square frame is only very little farther out than the solid square.

VIII. Two lines each 5 cm long, 1.5 wide, 2 cm apart, so that their outer edges again form a square of 5 cm. P. 19.2;

R. 16.9; M. 24.1; S. 17.1; B. 14.5. It is to be noted how much farther this square made by the two lines is placed from the centre than the solid square, the average difference being 3.4 cm.

IX. Two lines each 10 cm long, 1.5 cm wide. P. 17.9; R. 16.8; M. 17.9; S. 17.4; B. 14.4.

For the four following groups the right side had the same 10-cm line as in I–IX, but the interval between the right-hand blue line and the constant line was filled with five lines of different sizes and colors. The movable piece on the left where the interval was not filled varied as follows:

X. A line 10 cm long, 1.5 wide. P. 19.4; R. 14.7; M. 18.9; S. 21.8; B. 15.5.

XI. Square 5 cm. P. 17.7; R. 15.0; M. 16.8; S. 17.4; B. 14.8.

XII. Star 5 cm diameter. P. 18.9; R. 14.3; M. 20.6; S. 18.2; B. 15.7.

XIII. Square frame 5 cm. P. 17.8; R. 14.7; M. 17.2; S. 18.2; B. 14.8.

The four movable parts of X–XIII correspond to the movable parts of I, IV, V, VI; that is to say, a line, a square, a star, and a frame are on the left, while on the right one series has the interval filled, the other not. If we compare the averages of the two series we get the following: for

I, IV, V, VI. P. 17.8; R. 14.0; M. 16.2; S. 16.6; B. 14.9.

X, XI, XII, XIII. P. 18.4; R. 14.7; M. 18.4; S. 18.9; B. 15.2.

The distance of the left-hand form is greater in the second or filled series for all subjects; but it is to be noticed that while the difference is considerable for M. and S., for the three others it is some millimetres only.

In the four following groups the conditions of I, IV, V, and IX are given again, with the exception that the central line here is not 30 cm but only 5 cm long. The movable part is given as follows:

XIV. Line, 10 cm long, 1.5 wide. P. 16.5; R. 13.3; M. 19.9; S. 18.8; B. 16.2.

XV. Square 5 cm. P. 15.6; R. 13.0; M. 15.1; S. 16.5; B. 14.0.

XVI. Star. P. 16.6; R. 13.0; M. 21.3; S. 16.3; B. 16.9.

XVII. Two lines 10 cm long, 1.5 wide. P. 13.9; R. 16.7; M. 16.7; S. 18.0; B. 15.5.

Here again we find the differences resulting from the special forms similar to those of the first set: the square is nearer to the centre than the line, and the star farther out than the square; but the important point is that this group differs so much from the corresponding groups I, IV, V, IX, while the forms on both right and left remain the same, the central line only being changed. Now if we compare the averages,

I, IV, V, IX. P. 17.9; R. 14.7; M. 16.7; S. 16.9; B. 15.0;

XIV–XVII. P. 15.7; R. 14.0; M. 18.2; S. 17.4; B. 15.7,

it is evident that the variations are in both directions. M., S., and B. place the movable line farther from the centre when the central line is short; R. and the writer place it nearer under the same conditions.

In general, then, an examination of these 17 groups with regard to the influence of form tends to show that the feeling of symmetry resulting from a combination of parts not symmetrical is subject to great individual differences, as the averages of all the groups together give:

P. 17.6; R. 14.6; M. 18.1; S. 17.8; B. 15.3.

On the other hand there is such an agreement of all the subjects in regard to the relative value of the different forms, that the figures are surely not to be regarded as a matter of chance, but as the expression of constant relations. A long line must be farther out than a short one, a narrow farther than a wide; a line farther than a square, a star farther than a square; an empty interval must be larger than one filled, and so on. How are we to interpret these much-neglected elements of the æsthetic impression? Are the results to be traced to a sensational or an intellectual origin, or more especially are the sensations resulting from the muscular action of the eyes or the suggested ideas or associations the determining factor? In favor of a purely sensational explanation it seems to me are the results of groups X–XIII as compared with I, IV, V, VI. The filling of the interval had here the effect of pushing the movable part a few millimetres farther out.

If there had been an intellectual association, and if the attempt had been to balance the figure from a purely mechanical point of view, the movable form would have been placed much farther out. The small increase, however, corresponds to the increased difficulty with which the eye moves over the filled space as compared with the empty one.

Still stronger in favor of a sensational explanation are the results of groups XIV–XVII as compared with I, IV, V, IX. The only difference here is in the central line, which is 5 cm long in the groups XIV–XVII instead of 30 cm. If the sides are precisely the same in both series there can be no reason if the comparison is intellectual in changing the relations of the movable line when the central line is changed, but we have seen that such a variation does occur for each subject. The eye-movements are, however, much changed by the change in the central line. The new eye-movements suggest new ideal lines connecting the ends of the various forms, and as every new combination allows new ideal lines, it can be understood that here the differences for each subject should become more apparent.

It is to be noted that the greatest individual differences result where there are two movable lines: here it is obvious that the subject is free to choose between the inner and outer line as the outer end for the ideal connecting lines.

On the other hand, it seems to me impossible to explain all by eye-movements alone. Two lines forming a square are regularly put farther out than a solid square of the same outlines. The association of solidity seems here to be the deciding factor. The same explanation applies where the star is farther out than the square and a narrow line farther out than a wide line.

We turn now to the question of color. It will be remembered that in the first three groups the fixed line to the right was in half the cases red, in the other half blue. The numbers above give both colors together. If we now separate them we obtain the following result:

A. Red-line groups I, II, III: P. 19.0; R. 14.2; M. 20.0; S. 20.1; B. 16.5.

B. Blue-line groups I, II, III: P. 18.2; R. 13.6; M. 17.8; S. 18.8; B. 15.7.

The movable line on the left was placed farther out when the right fixed line was red than when it was blue.

More striking are the results from the six colors used in the movable forms: these forms appeared in each of the 17 groups equally often in each color. The averages are as follows:

P. blue 18.5, green 18.0, maroon 17.8, red 17.6, orange 17.3, white 17.0. R. blue 15.2, maroon 14.7, green 14.6, white 14.5, red 14.4, orange 13.7. M. blue 20.4, maroon 18.7, green 18.1, red 17.6, white 17.1, orange 17.0. S. blue 20.3, maroon, 18.7, green 18.3, red 17.9, orange 17.1, white 16.1.

For all these subjects, blue, maroon, and green, the dark colors are the farthest out; white, red, and orange, the bright colors are nearest the centre. This means that a dark color must be farther out than a bright one to compensate for a form on the other side. The brightness of an object is then a constant substitute for its distance in satisfying our feeling of symmetry. The order of the colors is, however, somewhat changed for Mr. Buck. B. blue 17.5, orange 16.6, maroon 15.3, white 15.1, red 15.0, green 14.7. Orange and green have here changed places. The explanation is simple: Mr. Buck is red-green color-blind.

It is remarkable that most of the men felt subjectively sure that the colors have little influence. In fact it was not until after having done many experiments on myself that I was sure the colors did have any effect, and not until I had tabulated the results could I tell in what directions they did work.

I am inclined to think that here again we have to do with the strength of eye-movements. Red, orange, and white stimulate the eye more strongly than blue, green, and maroon, and call forth stronger eye-movements by which a form with a bright color gets the importance of a larger object. This would destroy the feeling of symmetry if the forms were not placed farther in.

Our feeling of symmetry which demands unity for the two parts can then be fully satisfied by arrangements of geometrically different forms and by different colors; variations in the size of the forms and the brightness of the colors can be compensated for by variations in the distance from the centre. The general law seems to be that the feeling of symmetry is satisfied when both parts call forth eye-movements of like energy; this energy increases with the distance from the centre or the larger size of the objects, and with the greater brightness of the color. The judgments tend to agree if a given set of eye-movements is necessary; the difference between individuals, and even of the same individuals at different times, is greater when there is a possibility of various combinations of eye-movements. All these sensational differences can be supplemented or destroyed by intellectual associations which give special parts a greater importance, as solidity, impressiveness, and so on. Greater importance works like a substitute for greater energy of the eye-movements, and a more important object must come nearer the centre to satisfy the feeling of symmetry.

THE IMAGERY OF AMERICAN STUDENTS.

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Wesleyan University.

(With the Assistance of Mr. C. H. JUDD.)

The data on which the following paper is based were obtained from 188 sets of answers to the well-known questions of Mr. Francis Galton on 'Visualizing and other Allied Faculties.' Of these, 37 sets came from members of the classes of 1881 and 1882 in Princeton College, and were kindly furnished to the writer by Professor H. F. Osborn, then of Princeton, now of Columbia College.* The remaining 151 sets were collected from four successive classes, 1890–93, in Wesleyan University. The answers, as a whole, represent the American student in the latter half of his college life (junior or senior year), and at a probable average age of twenty to twenty-two years. The replies were in general given after some preliminary acquaint ance with the subject. The Princeton papers came from volunteers who had enjoyed the advantage of a lecture by Professor Osborn on Mr. Galton's work and a subsequent free discussion by various members of the audience. At the close of the evening 100 blanks were distributed, of which 43 were returned (6 of these being thrown out in the present inquiry as not coming from students, or for other neutral reasons). The Wesleyan papers were the answers of pupils in (prescribed) elementary psychology, and in this case nearly every member of the four classes concerned answered; in fact, many of them did it as task-work, though in the majority of cases they had already made some study of the imagination and had heard some discussion of Mr. Galton's investigations in particular.

* The Princeton answers have been discussed in part by Mr. Galton in his *Inquiries into Human Faculty* and by Professor Osborn in the *Princeton Review.*

The data may, therefore, be fairly held to represent more intelligent self-observation than can be secured from persons ignorant of psychological study altogether; and the exaggeration of personal experience, which might have been looked for under the circumstances, we have been forced by careful study to believe to have been for the most part absent. At first sight it seemed that some unconscious self-deception was indicated by such phenomena as the excess of number-forms over percentages recorded by other observers, and the remarkable number of good visualizers in the case of color reproduction; but the more the papers were considered, the stronger the conviction became that our results were fair averages for the class of persons under discussion. The Princeton volunteers gave no higher results than the Wesleyan returns. The class of '92 at Wesleyan, which appeared to have overstepped all bounds of care in their answers, were shown by a second, test set, taken from about one-half the class a year after the first series, to be entirely reliable. Cross-examination, moreover, of some of the members of another class led to the same conclusion; and in regard to at least one of the doubtful questions —No. X—there was corroborative evidence, in the shape of drawings of the number-forms which the subjects claimed to possess. Finally, it may be noted that the variations within the group run parallel to those remarked by other observers, notably by Mr. Galton himself.

The first three questions are intimately related:

"Before addressing yourself to any of the questions on the opposite page, think of some definite object—suppose it is your breakfast-table as you sat down to it this morning—and consider carefully the picture that rises before your mind's eye.

"1. *Illumination.*—Is the image dim or fairly clear? Is its brightness comparable to that of the actual scene?

"2. *Definition.*—Are all the objects pretty well defined at the same time, or is the place of sharpest definition at any one moment more contracted than it is in a real scene?

"3. *Coloring.*—Are the colors of the china, of the toast, bread-crust, mustard, meat, parsley, or whatever may have been on the table, quite distinct and natural?"

The answers to I. may be divided into five classes. The first class consists of those in whom the faculty is present to a very high degree; the illumination here is 'as bright as the original,' or, as another member of the class puts it, 'clear, not at all blurred, as bright as the actual.' The second class yields answers such as this: 'clear, comparable to the original, but not quite so bright.' The members of the third class have images which are fairly clear, but hardly bright enough to be comparable to reality; one of them describes his imagery as "fairly distinct, but not like the original. It is as if seen through a thin mist or steam." The images of class IV are dim and not to be described as even fairly clear, while the comparison to reality is entirely dropped; and class V includes those cases in which the power is shown in its very lowest stage, or, perhaps more accurately, tends to disappear altogether. The replies from this lowest grade often indicate a lack of comprehension of the subject, with no real conception of the meaning which the questions are intended to convey. One subject says that he "feels the breakfast-table rather than sees it"; a second declares that he "never did have and does not now have any images whatever." * The following table shows the number of cases in each of these five classes, and the percentages relative to the whole number (188):

Class.................	I	II	III	IV	V
No. of Cases.........	24	91	54	14	5
Percentage...........	13	48	29	7	3

It will be noted that the large majority of cases fall within classes II and III, proving the prevalence of moderate to high degrees of capacity. The first or highest class is also well represented, but the fourth and fifth show a marked falling off.

This class-division based on I. is in general one of the most important that can be made. Thus the answers to Mr. Galton's question IX—"What difference do you perceive

* Cf. Galton, *Inquiries into Human Faculty*, p. 84 *et seq.*, and James, *Principles of Psychology*, II. pp. 56, 57, on the typical character of these answers. The text diverges chiefly by the attempt at *statistical* classification.

between a very vivid mental picture called up in the dark and a real scene?"—show, as was to be expected *a priori*, a close connection between the degree of illumination and the sense of reality. The replies indeed are hardly susceptible of statisti cal presentation, but a very large proportion exhibit the relation in question. Members of the first class declare with great unanimity that they perceive no difference between the vivid image and the real scene. Those of the second and third classes usually say that the image is not so bright. Even the fifth or lowest class attribute their lack of images to the fact that all is dark; as one says, "there seems at times to be an image of a very general color, but it is too dark to be seen with any certainty."

Retaining the same fivefoia division in our consideration of 2. we find that two natural subdivisions appear under each class—A, those subjects who have all the objects on the imaged table pretty well defined at the same time, and B, those whose place of sharpest definition is somewhat contracted. These subclasses are, of course, relative to the main division, for where the whole field is dim the defined portion may be called distinct only in a comparative sense. This understood, the results are these:

Class (by degree of illumination).	I	II	III	IV	V
Whole No. in each Class........	24	91	54	14	5
A (full definition)..............	19	50	21	5	0
B (definition contracted)........	5	41	33	9	5

This table shows that the extent of clear definition varies somewhat as the degree of illumination; as the latter decreases, the former contracts, with an almost continuous gradation among the several classes. Those under class I. who notice any contraction state with evident caution that "the place of sharpest definition is slightly contracted." In classes II and III most of the cases range from 'somewhat' to 'considerably contracted.' Class IV will have it 'much more contracted'; and in class V the replies indicate that such vague images as are sometimes seen are confined to single objects or a very small point. It is worthy of notice, further, that persons who image clearly less than the real scene usually bring the fact into relation to the action of attention. As one of them says: "A point is clearly defined when my attention is turned upon it, while the objects around it are rather hazy."

The replies to 3. argue a widespread faculty of color visualization. No less than 172 of our subjects, 91% of the whole number, think that their color imagery is relatively distinct and natural, while only 16 or 9% fail in this respect. Some of the former go so far as to state that "colors are much more distinct than the general outline"; and a few in the latter division are able to bring out the missing quality by turning their attention toward it. An emphatic testimony is given by one who ranks in the lowest class under 1. when he says that if he could be persuaded to believe that he had any visual images at all, it would be because he sometimes thinks that he can (mentally) see colors.

Mr. Galton's question V presents some interesting phenomena and may best be taken up next. It runs:

"5. *Distance of Images.*—Where do mental images appear to be situated—within the head, within the eyeball, just in front of the eyes, or at a distance corresponding to reality? Can you project an image upon a piece of paper?"

Of our students 133 or 71% * state that with them the distance corresponds to reality, while 6 see their images at positions more distant than the real scene, 'as in a kind of bird's-eye view." The remaining 49 are divided as follows: 19 localize their images in the head, 14 just in front of the eyes, 4 in the eyeballs, and 12 at variable distances. The margin of error here is probably considerable, for the 'situation of mental images' may be conceived in various ways. Thus, we are convinced, a subject may locate his images as *ideas* in the head and so reply to the question, though their apparent position in his scheme of imagery, if he could be induced to attend to this alone, would prove different. Again, the imaged break-

* This is apparently contrary to the observations of Galton, who says, p. 99, "Most persons see it [the image] in an indefinable sort of a way, others at a distance corresponding to reality."

fast-table may be thought of at its actual known distance from the subject at the moment; or he may imagine the table with himself sitting at it, and estimate distances from the standpoint of the recalled self; or he may interpret the question in the sense intended by its framer and state what position his images tend to assume apart from special influences. But these variable factors aside, there is evident in some cases a marked connection between the apparent distance of images and the effort of attention necessary for their production. Thus our students write: "Some mental images seem to be in front of the eyes. These are generally some small objects, as a photograph, inkstand, etc., or images that have cost me severe strain to recall. Images of scenes, views, etc., and all such as come without special mental strain are just behind the eyes"; "Large objects as in reality. Smaller in the head or just before the eyes"; "If a reproduction of a particular scene, at a distance corresponding to reality. If imagining some small object without regard to a previous scene it appears to be about four yards in front of me. Large objects as far distant as I can regard them with best advantage"; "Mental images appear situated at a distance corresponding to reality when recalled without intense effort." Two things are to be noted here: first, that the images of small objects tend to be situated closely in front of the eyes; and second, that when the image is recalled with difficulty a like effect is produced on the apparent localization. In regard to the former phenomenon it may be enough to refer to the fact that we habitually look at small objects from a near position, but in reference to the second we may go a little deeper. Here the influence of the effort to recall on the apparent distance must be considered in connection with the further facts that of these 49 persons with whom the position of the image is other than that of the real scene, 39 report (in answer to 2.) the point of sharpest definition in their imagery to be considerably contracted, and that such contraction, as noted above, is often a function of the strained attention necessary for clear visualization. It is impossible not to conclude that, at least in some cases, the situation of visual images depends on the attention involved; while the inference is near that this phenomenon may be connected with the feelings of change in convergence and accommodation which so often accompany concentrated visual attention in the first instance. If the strain of recall should suggest these usual associates of concentrated visual attention, and if these in turn should exercise their wonted influence as determinants of distance, the case would be clear.*

The remaining questions are of a more special character and deal for the most part with particular modifications of the visualizing function. The fourth, which we may now take up, runs:

"4. *Extent of Field of View.*—Call up the image of some panoramic view (the walls of your room might suffice); can you force yourself to see mentally a wider range of it than could be taken in by any single glance of the eyes? Can you mentally see more than three faces of a die, or more than one hemisphere of a globe, at the same instant of time?"

To this 130 of our subjects, 69%, either return a general negative or give doubtful answers. With 58 or 31% the capacity is very variable. Thus one can see mentally 'just a little more' than the range of a single glance; another is confident that he can 'see a whole sphere'; and a third describes the image of his "whole room as if he were looking through his head." The replies of others suggest Mr. Galton's statement that "some persons have the habit of viewing objects as though they were partly transparent"; while two of our cases throw light on the manner of effecting the feat described in the question which some of his subjects term 'a kind of touch-

* A striking example of the effect of attention on the position of images was observed in the case of Mr. A. G. C., who, as he is not a student, could not be included in the statistics of the text. Mr. C. is a gifted visualizer. His images are so habitually localized as in reality that cross-examination failed to show a single exception. He is also endowed with the gift of mentally 'seeing around objects,' and can image four faces of a die without difficulty. The visualization of all six faces, however, costs him an effort of attention, and when he was asked to accomplish it, it was noted that the hand which he held in front of him to aid his visual imagination (as though the die were held in it) *moved up toward the eyes.* Questioned about the fact, Mr. C. recognized the movement, although he had performed it involuntarily, and was able to estimate it in inches since his business and habits accustom him to measurement. Without strain he holds the hand and visualizes images at about 14 inches from the eyes; with strain they move up to a distance of 4 inches.

sight.' One student says that he is in doubt as to his ability to visualize more than can be taken in at one glance, but adds in a note: "I can easily imagine putting my fingers around the six sides of a cube at the *same* time, and so take in (perceive) more than one or two or three sides of the cube." Mr. A. G. C., it will be also remembered (cf. note above), held the imaginary die in the hand when endeavoring to visualize it; so that we may surmise that with these gentlemen experiences of active touch, imagined or actually present, aid the effort of vision to grasp the whole field. A second student cannot visualize the spots on the die, but can readily recall more than three sides of a plain cube; and here it is possible that ocular movement or adjustment may account for the ease in imaging the general form in contrast to the colored spots. But we should be slow to deny that some gifted visualizers can in fact 'mentally see' around objects with little or no help from other than purely visual sources. The positiveness of their assertions on the matter go far to offset any theoretical difficulties which may be raised.

Question VI relates to command over images:

"6. *Command over Images.*—Can you retain a mental picture steadily before the eyes? When you do so, does it grow brighter or dimmer? When the act of retaining it becomes wearisome, in what part of the head or eyeball is the fatigue felt?"

The answers to the first two clauses of this question may be divided into five classes. First, 11 persons say that they cannot retain their images at all. Of the remaining 177 or 94%, the second class find no change in the illumination of the retained picture; while the third notice an increase of brightness, this being shown by their replies to depend in many cases on the focussing of attention. As one says, "By severe strain I can retain an image; it grows brighter and more distinct; the object comes out better"; and a second, "It tends to grow dimmer; but that depends on the passivity of the state: could will it to grow brighter." The fourth and much the largest class shows a decrease of illumination, the diminution varying to the extreme limit. With some in this class the image vanishes suddenly after a period of retention;

others notice that 'the images gradually fade out in thirty seconds or a minute.' The fifth class show a kind of rhythm in their imagery which is very interesting, a period of brightness being followed by a period of dimness, which in turn gives place to bright images again, etc., etc. In the words of one member of the class, 'the images are intermittent'; or as another characteristically puts it, "Images come by flashes, then die away. Flashes can be produced in quick succession and continued indefinitely without weariness." The several classes may be thus tabulated:

Class	I	II	III	IV	V
No. of Cases	11	30	43	88	16
Per cent of whole No.	6	16	23	47	8 +

The final clause of the question brought out no very definite results. In a number of cases no fatigue at all is felt; when it does occur, it is usually located in or about the eyes.

In answer to question X.

"10. *Numerals and Dates.*—Are these invariably associated in your mind with any peculiar mental imagery, whether of written or printed figures, diagrams, or colors? If so, explain fully, and say if you can account for the association?"

That they have no such associations whatever is stated by 105 persons or 56%, 67 or 36% say broadly that there is an association with the written or printed figures, and 16 or 8% that they have number-forms. The last of these classes is relatively very large. Among its members there is one interesting case in which a number-form used in childhood had fallen out of use in later life and even out of mind. When the gentleman in question began the study of French in his Freshman year, however, the old form was revived and the French names for the numerals learned in association with it. The matter thus being brought back to memory, the same subject recollected further that he had once possessed an alphabet-form as well, and that as he first learned the Greek alphabet he had made a modification in the form, apparently to fit the different order of the Greek letters. All these forms have now been again discarded ; and the case as a whole is a good illustration of the decay of visualization as age advances and abstract thought increases.

Such, then, are the general results for the visual imagery of American college students so far as indicated by the data at our command, for the remainder of the questions developed no very new or noteworthy facts. The most striking phenomenon shown is the intimate relation of imagery and attention and the effect of the latter on the various phases and characteristics of the former. The question of the relative capacity of our students and other classes of persons is not shown by these results ; but in general we estimate that the students stand higher than the English scientists and men of letters whom Mr. Galton examined. Within the group of Wesleyan students an attempt was made to compare the power of visualization with the standard of scholarship as tested by college grades. The outcome proved the data to be inadequate for the determination of the problem, but as far as the figures go they point to a little lower average of imaging power among those who stand highest in their respective classes. It would be instructive to have a comparative report from female students of a similar age and stage of advancement. The few sets of answers which we have obtained from ladies at Wesleyan show, as might have been expected, a higher development of the faculty among them than among the men ; and a small number from Vassar College point to the same conclusion.

THE PENDULUM AS A CONTROL-INSTRUMENT FOR THE HIPP CHRONOSCOPE.

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The necessity of controlling the constant error of the Hipp chronoscope for longer times than 100σ, the maximum obtainable with the Cattell drop-screen, and the many objectionable features of the new enlarged Krille control-hammer, led me in the fall of 1892 to investigate the accuracy with which some form of pendulum could be made to record a suitable range of absolute time-periods. From an examination of the mechanical principles involved in both instruments it would seem that the constancy and accuracy of a pendulum must be at the very least as great as that of the control-hammer; for the latter is in principle a pendulum also, set free from a point above the axis of suspension and arrested in the first part of its swing.

The tests were made upon a compound pendulum of very simple construction. An iron bar 141 cm in length, in cross-section an ellipse 2×1 cm, was hung upon knife-edge bearings of steel in such manner that the axis of suspension was at a distance of 72.3 cm from the lower end. A heavy bob of iron weighing 5 kg moved up and down the lower portion of the bar and could be secured by a thumb-screw at any desired point. A lighter bob, 2 kg in weight, was similarly adjustable on the part of the bar above the axis of suspension. The knife-edge bearings restricted the amplitude of oscillation to about 60°, or 30° to each side of the vertical position. A small brass marker projected below the lower end of the pendulum and moved to and fro just above a metal scale, conforming to the arc of oscillation, and graduated in tenths of degrees from the vertical position, the zero point, to 25° in both directions. Two contact-pieces were movable upon this arc-scale and could be set at any desired mark on the scale. These contact-pieces were self-adjusting. When set free, the pendulum swung from left to right; the projecting marker, striking the first contact and turning the disk of the contact-piece with a minimal expenditure of energy, closed an electric circuit; the pendulum moved on, the marker struck the second contact, and the circuit was broken. On the return swing of the pendulum, the marker in similar manner readjusted the contacts for the next experiment. The contact-pieces could also be so arranged that the swing of the pendulum first breaks and then closes a circuit. The accuracy and constancy of the time-records of the pendulum depend upon the accurate working of the contact-pieces. Those made for these tests were supplied the laboratory by Mr. J. D. Brown, of Camden, N. J., and were the best that I had seen up to that time. Some difficulty, however, was experienced in properly adjusting the amount of pressure just sufficient to prevent the blow of the pendulum from sending the circular disks of the contacts too far around. Because of this difficulty and on account of a subsequent injury to the readjusting mechanism, the projecting pointers of the contact-pieces, which are struck by the marker of the pendulum-bar, were put in place by hand before each experiment of the series tabulated below. When the self-adjusting mechanism worked at all, the results were as satisfactory as when the contacts were reset by hand. On a day, for example, when the chronoscope registered 100.1σ as the fall-time of the Cattell drop-screen, the average of 20 experiments, the mean variation being 0.69σ, the same chronoscope recorded a time-value of 96.4σ given by the pendulum, the contacts readjusted by the return swing, with but 0.76σ as the *mv* of 40 experiments. This mean variation is about the same as the average mean variation of the tabulated experiments. A second set of contacts, in which the difficulty in adjusting the pressure properly had been done away with, was furnished the laboratory, but was sent to Chicago before its accuracy could be tested and was there stolen. Mr. Brown has lately supplied the laboratory of Columbia College with an improved form of this recording device.

The pendulum was held in place at the extreme left of the arc by a metal catch, and was released and replaced on its return swing by hand. More convenient is a magnet of sufficient strength to catch up the pendulum-bob on its return swing. The whole apparatus is then self-adjusting for any number of experiments after the contact-pieces have once been put in place on the arc-scale, and is controllable at a distance by an on-and-off key.

The present experiments had as their object to compare the variable error of the pendulum with that of the Cattell drop-screen. The latter instrument is used in the laboratory of the University of Pennsylvania to control the Hipp chronoscope. The screen occupies exactly 100σ in falling through the distance between the contacts, as determined by calculation and by the chronographic method. Spring and current are adjusted until the chronoscope gives a reading of 100σ as an average of 10 experiments, with a mean variation of not more than 1σ. If the mean variation exceeds 1σ, the circuit or drop-screen or chronoscope is overhauled until the variable error falls within the proper limits. The large *mv* 2.2σ obtained by Külpe and Kirschmann * from ten records with the screen, whose average result was 63σ, was no doubt due to some imperfection in the contacts and would have been diminished with care and use (the instrument was a new one). Of six series of 20 chronoscopic records of the maximum drop, 100σ, the average mean variation as shown in Tables I and II was 0.66σ. Within the limits of the time-values given by the free drop of the screen, the instrument has given complete satisfaction as a control to the chronoscope.

The experiments of Tables I and II were made and averaged in series of twenty. No experiment was excluded from the series of Table I for any cause whatever, and averages throughout represent 20 successive experiments. But to obtain the longer times of Table II the pendulum was allowed to swing to and fro a number of times, and the experimenter on several occasions failed to arrest the pendulum at the proper moment. For this reason it was necessary to exclude four experiments while the series of 20 tests was being made.

* *Ein Apparat zur Controle zeitmessender Instrumente.* Beschrieben von O. Külpe und A. Kirschmann. Wundt, Phil. Stud., Bd. VIII. Heft 2, p. 148.

If the first experiment or occasionally the first two seemed to vary by a greater amount from the average than any of the remainder, one or two additional records were taken immediately, and if the last two varied less than the first two from the general average, the last twenty were used to obtain the tabulated average. But no results were excluded for any reason from the body of the twenty successive records after the series had been closed and the operation of calculating the average and mean variation begun.

In Table I are given the results of 280 chronoscopic records of a time-period of *circa* 100σ, given by the swing of the pendulum from contact to contact. No attempt was made to measure by the chronographic method the absolute time-period; what this actually was is to be inferred by comparison with the fall-time of the drop-screen. To obtain the results of the first series of No. 1 (*Av.* 101.0σ, *mv* 0.25σ), the upper bob was taken off, the lower bob raised as high as it would go, the pendulum consequently moving at its fastest rate. One contact—the making contact—was placed as far as possible to the left of the arc (at $-$ 24.7°), the pendulum being set free at $-$ 28°. The breaking contact was then tried in various positions until the chronoscope recorded a time-interval of approximately 100σ. When this had been determined and the position ($-$ 15.95°) noted, the twenty records were taken from which the average result of 101σ with *mv* 0.25σ was calculated. The contact-pieces, with bobs unchanged, were next tried near the centre of the arc, at the point of maximum velocity and momentum. With the contact-pieces at $-$ 7° and + 5.6° the chronoscope recorded an average result of 99.5σ with 0.5σ *mv*. The lower bob was then moved to the bottom of the bar, reducing the rate of oscillation, and again the positions at the bend and centre of the arc were found at which the chronoscope registered *circa* 100σ (Nos. 3 and 4). By using the upper bob and raising the lower, the rate was further reduced (Nos. 5, 6, and 7). Four rates were used, which are noted in the table as 'very fast,' 'fast,' 'slow,' and 'very slow'; the corresponding time of a single oscillation was roughly estimated at just less than $\frac{1}{2}$ sec., $\frac{2}{3}$ sec., 1 sec., and 4 secs. respectively. After the seven series had

Table I.

CHRONOSCOPIC RECORDS OF SWING OF CONTROL-PENDULUM AND OF FALL OF CONTROL-SCREEN. AVERAGES AND MEAN VARIATION OF 280 EXPERIMENTS IN TERMS OF SIGMA. SERIES OF 20 EXPERIMENTS.

Pendulum.						Drop-screen.	
No.	Position of Bobs.	Rate of Swing.	Position of Contacts.	Av. Result.	MV.	Av. Res.	MV.
I	Upper bob off. Lower bob raised to highest point.	Very fast.	$-24.7°, -15.95°$ To left of arc and beginning of swing.	101. 100.9	0.25 0.45 0.35	100.1 100.1 100.0	0.92 0.68 0.65
2	"	"	$-7°, +5.6°$ Centre of arc.	99.5 98.1	0.5 0.58 0.54	100.2 100.2	0.69 0.48
3	Upper bob off. Lower bob at bottom of bar.	Fast.	$-24.7°, -17°$	99. 99.8	0.5 0.63 0.56		
4	"	"	$-7°, +4.5°$	100.2 99.6	0.68 0.74 0.72		
5	Upper bob at top. Lower bob at bottom.	Slow.	$-24.7°, -20.3°$	96.1 97.8	0.84 0.84 0.84		
6	"	"	$-7°, +0.1°$	102.1 101.0	0.46 0.35 0.40		
7	Upper bob at top. Lower bob raised to balance upper.	Very slow.	Centre of arc. Very close together. No record in degrees.	100.5 100.	0.6 0.55 0.57		
				Av. Mv.	0.57	Av. Mv.	0.68

been completed for the seven different positions of bobs and contacts, they were repeated by placing the contacts without preliminary trials at the marks found in making the first series. The average variation of each series of two average results from the average of the two series is a measure of the accuracy with which the contacts may be set to a mark to record a desired time-period. This average variation is not given in the table, as no particular attention was paid at the time to the accurate replacing of the contacts. But this average variation for all results is only 0.44σ, the maximum being 0.85σ (No. 5).

The average results of the 14 series of 20 tests with their *mv* are given in vertical columns, together with the positions of bobs and contacts and with the rates of speed. Before, during, and after these tests, 5 series of 20 chronoscopic records each were taken of the fall-time of the drop-screen, to see that the constant error of the chronoscope had been reduced to zero and remained so, and to obtain data for comparing the variable error of the pendulum with that of the drop-screen. The results of these 100 records are given in vertical columns, next to the results of the pendulum series. The general averages at the bottom of the table show the average mean variation of a record with chronoscope and drop-screen in 100 experiments to be 0.68σ; that with the chronoscope and pendulum in 280 experiments to be 0.57σ. From this we conclude that for the time of 100σ the pendulum is as satisfactory a control-instrument as the drop-screen.

A comparison of the mean variations among themselves, for different positions of bobs and contacts, shows that with the pendulum swinging 'fast' and 'very fast' the best results were obtained at the beginning of the swing; but with 'slow' the centre of the arc is better. A medium rate would seem to give the best results. Too severe a blow upon the contacts or a too slow movement of the disks is relatively the more unfavorable condition. But the difference is inconsiderable and all *mv* fall within a desirable limit of accuracy.

Table II contains results with the control-pendulum arranged to give a considerable range of absolute times. The constant error of the chronoscope was corrected for 100σ. Differences in the length of the periods of magnetization probably induce variations in the constant error.* The average result for longer and shorter times than 100σ is therefore cor-

* Külpe and Kirschmann. as above, p. 149 and p. 169 (Table V).

TABLE II.

CHRONOSCOPIC RECORDS WITH CONTROL-PENDULUM FOR TIMES BETWEEN 18.9σ AND 3½ SECONDS.

	Pendulum.					Drop-screen.	
No.	Position of Bobs.	Rate of Swing.	Position of Contacts.	Av. Res.	MV.	Av. Res.	MV.
1	Upper bob off. Lower bob raised.	Very fast.	$-24.7°, -15.95°$	100.5	0.65	100.0	0.55
2	" " "	"	$-24.7°, 0°$	238.5	0.9		
3	" " "	"	$-24.7°, +24°$	481.5	0.8		
4	" " "	"	$-24.7°, +24°$ 3 oscillations	2034.7	0.77		
5	" " "	"	$-24.7°, +24°$ 5 oscillations	3589.8	0.9		
6	Upper bob off. Lower bob at bottom.	Fast.	$-24.7°, +24°$	543.9	0.63		
7	Upper bob at top. Lower bob at bottom.	Slow.	$-24.7°, 0°$	439.0	1.0		
8	" " " "	"	$-24.7°, +24°$	894.7	1.03		
9	Upper bob at top. Lower bob raised.	Very slow.	$-24.7°, +25°$	2927.1	1.11		
10	Upper bob off. Lower bob raised.	Very fast.	At centre of arc in closest contact	18.9	0.83		
11	" " "	"	1 mm apart	19.3	1.16		
12	" " "	"	2 mm apart	19.5	1.0		
13	" " "	"	3 mm apart	19.5	0.85		
	Av. MV. of 13 series				0.89		0.55
	Av. MV. of 14 series (Table I)				0.57	Av. MV. 5 series	0.68
	Av. MV. of 27 series (540 experiments)				0.70	Av. MV. 6 series (120 exp.)	0.66

rect only within a few sigma. It is only the *mv*'s, however, that are under consideration. After the control records with the fall-screen the bobs and contacts were set as given in No. 1 of Table I, and the average result of 100.5σ, *mv* 0.65σ, obtained. The breaking contact was then moved on to 0, and twenty trials gave an average time of 238.5σ, *mv* 0.9σ. Moving the contact to the other end (+24°) of the scale increased the absolute time to 481.5σ, *mv* 0.8σ. In the next series, No. 4, the position of the contacts remained the same, but the pendulum was allowed to make three single oscillations before the breaking-contact piece was thrown into operation. The average time recorded was 2034.7σ and the *mv* 0.77σ. In No. 5 the pendulum made five single oscillations, recording 3589.8σ, with 0.9σ *mv*. In Nos. 6, 8, and 9 a single oscillation was measured with varying rates of movement, and in No. 7 a half oscillation with the rate 'slow.' The averages of No. 10 were obtained by placing the contact-pieces as near together as possible at the centre of the arc-scale, while the bobs were arranged for the fastest rate. 18.9σ was therefore the shortest time measurable with this pendulum. Moving the contacts 1 mm apart (No. 11) increased the absolute time by 0.4σ; a second mm added but 0.2σ (No. 12), while a third had no effect on the average chronoscopic record. These results demonstrate the accuracy with which the contacts may be set to a mark on the scale, as a considerable movement of the contacts on the arc-scale produced no measurable variation in the absolute time.

The average variation of the 260 experiments of Table II is slightly greater than that of the 280 experiments of Table I and also greater than that of the drop-screen control records. No control records were taken during or after the pendulum tests, or an increased variation might have shown some source of increased error in the circuit or chronoscope. But the averages of Table II are within satisfactory limits of accuracy, and the general average variation of 0.70σ for the 540 experiments of Tables I and II when compared with the average Mv of 0.66σ from the 120 records with the drop-screen justifies the conclusion that the pendulum is as efficient a control for time-periods between 19σ and 4 sec. as the screen has been found to be for 100σ.

We are in a position to compare the accuracy of the Cattell drop-screen and of the pendulum-control with that of the enlarged Krille control-hammer. Külpe and Kirschmann* report an average mean variation of 1.05σ for about 100 chronoscopic records of ten different time-periods between 616σ and 56.6σ. If we assume the same degree of accuracy for the chronoscopes used in the Leipzig and Philadelphia laboratories, the variable error of the screen and of the pendulum is somewhat less than that of the control-hammer; for the average variation of a number of records is a variable error composed of the variable error of the chronoscope and of the variable error of the pendulum, screen, or hammer, as the case may be. In the range of absolute times the pendulum has the advantage; it has practically no upper limit, while the hammer has probably exceeded its maximum at 616σ. The practical advantages of the pendulum as a satisfactory laboratory instrument over the hammer are considerable. The pendulum may be managed during a series of experiments by a simple on-and-off key; it may serve any of the many purposes for which a pendulum may be used in a laboratory, or any pendulum may be fitted with an arc-scale and contacts and serve as a control-instrument (for example, the pendulum of the Cattell light-stimulus apparatus has been thus adapted at Columbia College). On the other hand, the control-hammer requires considerable manual attention in replacing the hammer in position and in resetting two pairs of contacts; it is expensive, cumbersome, and the wear and tear on the instrument from the shock of the falling hammer must in time diminish its accuracy.

The experiments both at Leipzig and at Philadelphia demonstrate a remarkable accuracy of the chronoscope. Lange† determined the probable error of the arithmetical mean of 10 records with the Leipzig chronograph to be 0.03σ, in each of two series. Cattell‡ has pointed out that the variable error of the chronoscope must, from the research of Külpe and

* Külpe and Kirschmann, as above, page 169, Table V, B.

† *Ein Chronograph nebst Control-Apparat für sehr genaue Zeitmessungen.* Phil. Stud., Bd. IV. p. 469.

‡ Am. Journal of Psych., vol. IV. No. 4, p. 597. See also Wundt's reply to this —*Chronoscop u. Chronograph,* Phil. Stud., Bd. VII. No. 4, p. 653.

Kirschmann, be assumed to be about the same as that of the chronograph, so that Lange's determination of the probable error of the latter instrument holds for the chronoscope also. With the control-pendulum, 100σ was recorded in one series with but 0.25σ *mv.* Over $3\frac{1}{2}$ seconds was measured with a mean variation of but 0.9σ. The length of the period of magnetization seems to have no effect on the variable error, and no doubt the latter is in largest part made up of the variable error of the contacts of hammer, screen, or pendulum. The Hipp chronoscope has proved itself a most satisfactory time-piece, and perhaps the same principle might be made use of in a chronoscope that would record with an equivalent degree of accuracy the ten-thousandth part of a second.

DISCUSSION.

THE PHYSICAL BASIS OF EMOTION.

In the year 1884 Prof. Lange of Copenhagen and the present writer published, independently of each other, the same theory of emotional consciousness. They affirmed it to be the effect of the organic changes, muscular and visceral, of which the so-called 'expression' of the emotion consists. It is thus not a primary feeling, directly aroused by the exciting object or thought, but a secondary feeling indirectly aroused; the primary effect being the organic changes in question, which are immediate reflexes following upon the presence of the object.

This idea has a paradoxical sound when first apprehended, and it has not awakened on the whole the confidence of psychologists. It may interest some readers if I give a sketch of a few of the more recent comments on it.

Professor Wundt's criticism may be mentioned first.* He unqualifiedly condemns it, addressing himself exclusively to Lange's version. He accuses the latter of being one of those *psychologischen Scheinerklärungen* which assume that science is satisfied when a psychic fact is once for all referred to a physiological ground.

His own account of the matter is that the immediate and primary result of 'the reaction of Apperception † on any conscious-content' or object is a *Gefühl* (364). *Gefühl* is an unanalyzable and simple process corresponding in the sphere of *Gemüth* to sensation in the

* Philosophische Studien, VI. 349, (1891).

† In this article, as in the 4th edition of his Psychology, Wundt vaguely completes his *volte-face* concerning 'Apperception' and dimly describes the latter in associationist terms. "Apperception is nothing really separable from the effects which it produces in the content of representation. In fact it consists of nothing but these concomitants and effects. [A thing that 'consists' of its concomitants !] . . . In each single apperceptive act the entire previous content of the conscious life operates as a sort of integral total force" (364, 365), etc. The whole account seems indistinguishable from pure Herbartism, in which Apperception is only a name for the interaction of the old and the new in consciousness, of which interaction feeling may be one result.

sphere of intellection (359). But *Gefühle* have the power of altering the course of ideas—inhibiting some and attracting others, according to their nature; and these ideas in turn produce both secondary *Gefühle* and organic changes. The organic changes in turn set up additional *sinnliche Gefühle* which fuse with the preceding ones and strengthen the volume of feeling aroused. This whole complex process is what Wundt calls an *Affect* or Emotion—a state of mind which, as he rightly says, 'has thus the power of intensifying itself' (358–363). I shall speak later of what may be meant by the primary *Gefühl* thus described. Wundt in any case would seem to be certain both that it is the essential part of the emotion, and that currents from the periphery cannot be its organic correlate. I should say, granting its existence, that it falls short of the emotion proper, since it involves no *commotion*, and that such currents *are* its cause. But of these points later on. The rest of Wundt's criticism is immaterial, dealing exclusively with certain rash methodological remarks of Lange's; emphasizing the 'parallelism' of the psychical and the physical; and pointing out the vanity of seeking in the latter a causal explanation of the former. As if Lange ever pretended to do this in any intimate sense ! Two of Wundt's remarks, however, are more concrete.

How insufficient, he says, must Lange's explanation of emotions from vaso-motor effects be, when it results in making him put joy and anger together in one class ! To which I reply both that Lange has laid far too great stress on the vaso-motor factor in his explanations, and that he has been materially wrong about congestion of the face being the essential feature in anger, for in the height of that passion almost every one grows pale—a fact which the expression 'white with rage' commemorates. Secondly, Wundt says, whence comes it that if a certain stimulus be what causes emotional expression by its mere reflex effects, another stimulus almost identical with the first will fail to do so if its *mental* effects are not the same? (355). The mental motivation is the essential thing in the production of the emotion, let the 'object' be what it may.

This objection, in one form or another, recurs in all the published criticisms. "Not the mere object as such is what determines the physical effects," writes Mr. D. Irons in a recent article* which, if it were more popularly written, would be undeniably effective, "but the subjective feeling towards the object. . . . An emotional class is not something objective ; each subject to a great extent classifies in this regard for itself, and even here time and circumstance make alteration

* Professor James's Theory of Emotion, Mind, p. 78, 1894.

and render stability impossible. . . . *If I were not afraid, the object would not be an object of terror*" (p. 84). And Dr. W. L. Worcester, in an article * which is both popularly written and effective, says: "Neither running nor any other of the symptoms of fear which he [W. J.] enumerates is the necessary result of seeing a bear. A chained or caged bear may excite only feelings of curiosity, and a well-armed hunter might experience only pleasurable feelings at meeting one loose in the woods. It is not, then, the perception of the bear that excites the movements of fear. We do not run from the bear unless we suppose him capable of doing us bodily injury. Why should the expectation of being eaten, for instance, set the muscles of our legs in motion? 'Common-sense' would be likely to say that it was because we object to being eaten; but according to Professor James the reason we dislike to be eaten is because we run away" (287).

A reply to these objections is the easiest thing in the world to make if one only remembers the force of association in psychology. 'Objects' are certainly the primitive arousers of instinctive reflex movements. But they take their place, as experience goes on, as elements in total 'situations,' † the other suggestions of which may prompt to movements of an entirely different sort. As soon as an object has become thus familiar and suggestive, its emotional consequences, *on any theory of emotion*, must start rather from the total situation which it suggests than from its own naked presence. But whatever be our reaction on the situation, in the last resort it is an instinctive reaction on that one of its elements which strikes us for the time being as most vitally important. The same bear may truly enough excite us to either fight or flight, according as he suggests an overpowering 'idea' of his killing us, or one of our killing him. But in either case the question remains: Does the emotional excitement which follows the idea follow it immediately, or secondarily and as a consequence of the 'diffusive wave' of impulses aroused?

Dr. Worcester finds something absurd in the very notion of acts constituting emotion by the consciousness which they arouse. How is it, he says, with voluntary acts? "If I see a shower coming up and run for a shelter, the emotion is evidently of the same kind, though perhaps less in degree, as in the case of the man who runs from the bear. According to Professor James, I am afraid of getting wet because I run. But suppose that instead of running I step into a shop and buy

* Observations on some points in James's Psychology. II. Emotion.—The Monist, vol. III. p. 285 (1893).

† In my nomenclature it is the total situation which is the 'object' on which the reaction of the subject is made.

an umbrella. The emotion is still the same. I am afraid of getting wet. Consequently, so far as I can see, the fear in this case consists in buying the umbrella. Fear of hunger, in like manner, might consist in laying in a store of provisions ; fears of poverty in shovelling dirt at a dollar a day, and so on indefinitely. Anger, again, may be associated with many other actions than striking. Shylock's anger at Antonio's insults induced him to lend him money. Did the anger . . . consist in the act of lending the money ?" (291). I think that all the force of such objections lies in the slapdash brevity of the language used, of which I admit that my own text set a bad example when it said 'we are frightened because we run.' Yet let the word 'run' but stand for what it was meant to stand for, namely, for many other movements in us, of which invisible visceral ones seem by far the most essential ; discriminate also between the various grades of emotion which we designate by one name, and our theory holds up its head again. 'Fear' of getting wet is not the same fear as fear of a bear. It may limit itself to a prevision of the unpleasantness of a wet skin or of spoiled clothes, and this may prompt either to deliberate running or to buying an umbrella with a very minimum of properly emotional excitement being aroused. Whatever the fear may be in such a case it is not constituted by the voluntary act.* Only the details of the concrete case can inform us whether it be, as above suggested, a mere ideal vision of unpleasant sensations, or whether it go farther and involve also feelings of reflex organic change. But in either case our theory will cover all the facts.

Both Dr. Worcester and Mr. Irons are struck by this variability in the symptoms of any given emotion ; and holding the emotion itself to be constant, they consider that such inconstant symptoms cannot be its cause. Dr. Worcester acutely remarks that the actions accompanying all emotions tend to become alike in proportion to their intensity. People weep from excess of joy ; pallor and trembling accompany extremes of hope as well as of fear, etc. But, I answer, do not the subject's feelings also then tend to become alike, if considered in themselves apart from all their differing intellectual contexts ? My theory maintains that they should do so ; and such reminiscences of extreme emotion as I possess rather seem to confirm than to invalidate such a view.

In Dr. Lehmann's highly praiseworthy book, 'Die Hauptgesetze des menschlichen Gefühlslebens,' † much is said of Lange's theory ; and in

* When the running has actually commenced, it gives rise to *exhilaration* by its effects on breathing and pulse, etc., in this case, and not to *fear*.

† Leipzig, 1892.

particular this same alleged identity of the emotion in the midst of such shifting organic symptoms seems to strike the critic as a fact irreconcilable with its being true. The emotion ought to be different when the symptoms are different, if the latter *make* the emotion; whereas if we lay a primary mental feeling at its core its constancy with shifting symptoms is no such hard thing to understand (p. 120). *Some* inconstancy in the mental state itself, however, Dr. Lehmann admits to follow from the shifting symptoms; but he contrasts the small degree of this inconstancy in the case of 'motived' emotions where we have a recognized mental cause for our mood, with its great degree where the emotion is 'unmotived,' as when it is produced by intoxicants (alcohol, haschisch, opium) or by cerebral disease, and changes to its opposite with every reversal of the vaso-motor and other organic states. I must say that I cannot regard this argument as fatal to Lange's and my theory so long as we remain in such real ignorance as to what the subjective variations of our emotions actually are. Exacter observation, both introspective and symptomatic, might well show in 'motived' emotions also just the amount of inconstancy that the theory demands.

Mr. Irons actually accuses me of self-contradiction in admitting that the symptoms of the same emotion vary from one man to another, and yet that the emotion has them for its cause. How can any definite emotion, he asks, exist under such circumstances, and what is there then left to give unity to such concepts as anger or fear at all (82)? The natural reply is that the bodily variations are within limits, and that the symptoms of the angers and of the fears of different men still preserve enough *functional* resemblance, to say the very least, in the midst of their diversity to lead us to call them by identical names. Surely there *is* no definite affection of 'anger' in an 'entitative' sense.

Mr. Irons finds great difficulty in believing that both intellectual and emotional states of mind, both the cognition of an object and the emotion which it causes, contrasted as they are, can be due to such similar neural processes, viz., currents from the periphery, as my theory assumes. "How," he asks, "can one perceptive process of itself suffuse with emotional warmth the cold intellectuality of another? . . . If perceptions can have this warmth, why is it the exclusive property of perception of organic disturbance (85.)?" I reply in the first place that it is not such exclusive property, for all the higher senses have warmth when 'æsthetic' objects excite them. And I reply in the second place that even if secondarily aroused visceral thrills were the only objects that had warmth, I should see no difficulty in accepting the fact. This writer further lays great stress on the vital difference between the receptive and the reactive states of the mind, and considers that the theory under discussion takes away all ground for the distinction. His account of the inner contrast in question is excellent. He gives the name of 'feeling-attitude' to the whole class of reactions of the self, of which the experiences which we call emotions are one species. He sharply distinguishes feeling-attitude from mere pleasure and pain—a distinction in which I fully agree. The line of direction in feeling-attitude is from the self outward, he says, while that of mere pleasure and pain (and of perception and ideation) is from the object to the self. It is impossible to feel pleasure or pain *towards* an object; and common language makes a sharp distinction between being pained and having bad feelings towards somebody in consequence. These attitudes of feeling are almost indefinitely numerous; some of them must always intervene between cognition and action, and when in them we feel our whole Being moved (93–96). Of course one must admit that any account of the physiology of emotion that should be inconsistent with the possibility of this strong contrast within consciousness would thereby stand condemned. But on what ground have we the right to affirm that visceral and muscular sensibility cannot give the direction from the self outwards, if the higher senses (taken broadly, with their ideational sequelæ) give the direction from the object to the self? We do, it is true, but follow a natural analogy when we say (as Fouillée keeps saying in his works on *Idées-forces*, and as Ladd would seem to imply in his recent Psychology) that the former direction in consciousness ought to be mediated by outgoing nerve-currents, and the latter by currents passing in. But is not this analogy a mere superficial fancy, which reflection shows to have no basis in any existing knowledge of what such currents can or cannot bring to pass? We surely know too little of the psycho-physic relation to warrant us in insisting that the similarity of direction of two physical currents makes it impossible that they should bring a certain inner contrast about.

Both Dr. Worcester and Mr. Irons insist on the fact that consciousness of bodily disturbance, taken by itself, and apart from its combination with the consciousness of an exciting object, is not emotional at all. "Laughing and sobbing, for instance," writes the former, "are spasmodic movements of the muscles of respiration, not strikingly different from hiccoughing; and there seems no good reason why the consciousness of the former two should usually be felt as strong emotional excitement while the latter is not. . . . Shivering from cold, for instance, is the same sort of a movement as may occur in violent fright but it does not make us feel frightened. The laughter excited in children and sensitive persons by tickling of the skin is not necessarily accompanied by any mirthful feelings. The act of vomiting may be the accompaniment of the most extreme disgust, or it may occur without a trace of such emotion" (289). The facts must be admitted; but in none of these cases where an organic change gives rise to a mere local bodily perception is the reproduction of an emotional diffusive wave complete. Visceral factors, hard to localize, are left out; and these seem to be the most essential ones of all. I have said that where they also from any inward cause are added, we *have* the emotion; and that then the subject is seized with objectless or pathological dread, grief, or rage, as the case may be. Mr. Irons refuses to accept this interpretation. The bodily symptoms do not here, he says, when felt, constitute the emotion. In the case of fear they constitute rather the object of which we are afraid. We fear *them*, on account of their unknown or indefinite evil consequences. In the case of morbid rage, he suggests, the movements are probably not the expression of a genuine inner rage, but only frantic attempts to relieve some inward pain, which outwardly look like rage to the observer (80). These interpretations are ingenious, and may be left to the reader's judgment. I confess that they fail to convert me from my own hypothesis.*

Messrs. Irons and Wundt (and possibly Baldwin and Sully, neither of whom accept the theory in dispute, but to whose works I have not access where I write, so that I cannot verify my impression) think that the theory carries with it implications of an objectionable sort philosophically. Irons, for example, says that it belongs to a psychology in which feeling can have no place, because it ignores the self and its unity, etc. (92). In my own mind the theory has no philosophic implications whatever of a general sort. It assumes (what probably every one assumes) that there must be a process of some sort in the nerve-

* Mr. Irons elsewhere says that "an object on being presented suddenly may cause intense fear. On being recognized as familiar the terror may vanish instantly, and while the mental mood has changed, for a measurable time at least, all the bodily effects of the former state are present" (86). Their dying phase certainly is present for a while; but *has* the emotion then 'vanished instantly'? I should rather say that there is then a very mixed emotional state, in which something of the departing terror still blends with the incoming joy of relief. The case of waking from nightmare is for us civilizees probably the most frequent experience in point. On such occasions the horror with me is largely composed of an intensely strong but indescribable feeling in my breast and in all my muscles, especially those of the legs, which feel as if they were boiled into shreds or otherwise inwardly decomposed. This feeling fades out slowly and until it is gone the horror abides, in spite of the fact that I am already enjoying the incomplete relief which comes of knowing that the bad experience is a dream, and that the horror is on the wane. It were much to be wished that many persons should make observations of this sort, for individual idiosyncrasy may be great.

centres for emotion, and it simply defines that process to consist of afferent currents. It does this on no general theoretic grounds, but because of the introspective appearances exclusively.

The objective qualities with which perception acquaints us are considered by psychologists to be results of sensation. When these qualities affect us with pleasure or displeasure, we say that the sensations have a 'tone of feeling.' Whether this tone be due to a mere form of the process in the nerve of sense, as some authors (e.g. Mr. Marshall) think, or to additional specific nerves, as others (e.g. Dr. Nichols) opine, is immaterial. The pleasantness or unpleasantness, once there, seems immediately to inhere in the sensible quality itself. They are beaten up together in our consciousness. But in addition to this pleasantness or painfulness of the content, *which in any case seems due to afferent currents*, we may also feel a general seizure of excitement, which Wundt, Lehmann, and other German writers call an *Affect*, and which is what I have all along meant by an emotion. Now whenever I myself have sought to discover the mind-stuff of which such seizures consist, it has always seemed to me to be additional sensations often hard to describe, but usually easy to identify, and localized in divers portions of my organism. In addition to these sensations I can discern nothing but the 'objective content' (taking this broadly so as to include judgments as well as elements judged), together with whatever agreeableness or disagreeableness the content may come tinged by.* *Such*

* The disagreeableness, etc., is a very mild affection, not drastic or grasping *in se* in the case of any objective content except localized bodily *pain*, properly so called. Here the feeling seems in itself overpowering in intensity apart from all secondary emotional excitement. But I think that even here a distinction needs to be made between the primary consciousness of the pain's *intrinsic quality*, and the consciousness of its degree of *intolerability*, which is a secondary affair, seeming connected with reflex organic irradiations. I recently, while traversing a little surgical experience, had occasion to verify once more the fact that it is not the mere *bigness* of a pain that makes it most unbearable. If a pain is honest and definite and well localized it may be very heavy and strong without taxing the extreme of our endurance. But there are pains which we feel to be faint and small in their intrinsic amount, but which have something so poisonous and non-natural about them that consent to their continuance is impossible. Our whole being refuses to suffer them. These pains produce involuntary shrinkings, writhings, sickness, faintness, and dread. For such emotion superadded to the pain itself there is no distinctive name in English. Prof. Münsterberg has distinguished between *Schmerz* as an original 'content' of consciousness and *Unlust* as due to flexor reactions provoked thereby; and before his Essay appeared, I remember hearing Dr. D. S. Miller and Dr. Nichols maintain in conversation that painfulness may be always a matter of 'intolerability,' due to the reflex irradiations which the painful object may arouse. Thus might even the mildest *Gemütsvorgänge* be brought under the terms of my theory.

organic sensations being also presumably due to incoming currents, the result is that the whole of my consciousness (whatever its inner contrasts be) *seems to me to be outwardly mediated by these.* This is the length and breadth of my 'theory'—which, as I apprehend it, is a very unpretending thing.

It may be, after all, that the difference between the theory and the views of its critics is insignificant. Wundt admits tertiary feelings, due to organic disturbance, which must fuse with the primary and secondary feelings before we can have an 'Affect;' Lehmann writes: "Constrained by the facts, we are obliged to concede to the organic sensations and tones of feeling connected with them an essential participation in emotion (wesentliche Bedeutung für die Affecte") (p. 115); and Professor Ladd also admits that the 'rank' quality of the emotions comes from the organic repercussions which they involve. So far, then, we are all agreed; and it may be admitted, in Dr. Worcester's words, that the theory under attack 'contains an important truth,' and even that its authors have 'rendered a real service to psychology' (p. 295). Why, then, is there such strong opposition? When the critics say that the theory still contradicts their consciousness (Worcester, p. 288), do they mean that introspection acquaints them with a part of the emotional excitement which it is psycho-physically impossible that incoming currents should cause? Or, do they merely mean that the part which introspection can *localize* in the body is so small that when abstracted a large mass of unlocalizable emotion remains? Although Mr. Irons professes the former of these two meanings, the only prudent one to stand by is surely the latter; and here, of course, every man will hold by his own consciousness. I for one shall never deny that individuals may greatly differ in their ability to localize the various elements of their organic excitement when under emotion. I am even willing to admit that the primary *Gefühlston* may vary enormously in distinctness in different men. But speaking for myself, I am compelled to say that the only feelings which I cannot more or less well localize in my body are very mild and, so to speak, platonic affairs. I allow them hypothetically to exist, however, in the form of the 'subtler' emotions, and in the mere intrinsic agreeableness and disagreeableness of particular sensations, images, and thought-processes, where no obvious organic excitement is aroused.*

* Mr. Irons contends that in admitting 'subtler' forms of emotion, I throw away my whole case (88, 89); and Dr. Lehmann enters into an elaborate argument to prove (as he alleges, against Lange and me) that primary feeling, as a possible accompaniment of any sensation whatever, must be admitted to exist (§§ 157-164). Such objections are a complete *ignoratio elenchi*, addressed to some imaginary

This being the case, it seems almost as if the question had become a verbal one. For which sort of feeling is the *word* 'emotion' the more proper name—for the organic feeling which gives the rank character of commotion to the excitement, or for that more primary pleasure or displeasure in the object, or in the thought of it, to which commotion and excitement do not belong? I myself took for granted without discussion that the word 'emotion' meant the rank feeling of excitement, and that the special emotions were names of special feelings of excitement, and not of mild feelings that might remain when the excitement was removed. It appears, however, that in this assumption I reckoned without certain of my hosts.

Dr. Worcester's quarrel with me at the end of his article becomes almost exclusively verbal. All pleasure and pain, he says, whether primary and of the higher senses and intellectual products, or secondary and organic, should be called 'emotion' (296).* Pleasure or pain revived in idea, as distinguished from vivid sensuous pleasure and pain, he suggests to be what is meant by emotion 'in the sense in which the word is commonly used' (297); and he gives an array of cases in point:

"Suppose that I have taken a nauseous dose and made a wry face over it. No one, I presume, would question that the disagreeableness lay in the unpleasant taste, and not in the distortion of the countenance. Now suppose I have to repeat the dose, and my face takes on a similar expression, at the anticipation, to that which it wore when I took it originally. How does this come about? If I can trust my own consciousness, it is because the vivid reproduction, in memory, of the unpleasant taste is itself unpleasant. . . . If this be the fact, what can be more natural than that it should excite the same sort of associated movements that were excited by the original sensation? I cannot make it seem any more credible that my *repugnance* to a repetition of the dose is due to my involuntary movements than my discomfort in taking it originally was due to the similar movements that occurred then. . . . I hardly think that any one who will consult his own consciousness will say that the reason he likes the taste of an orange is that it makes him laugh or smile to get it. He *likes* it because it tastes good, and is sorry to lose it for the same reason." (*Ibid.*)

theory with which my own, as I myself understand it, has nothing whatever to do, all that I have ever maintained being the dependence on incoming currents of the *emotional seizure* or *Affect*.

* 'The essence of emotion is pleasure and pain,' he adds. This is a hackneyed psychological doctrine, but on any theory of the seat of emotion it seems to me one of the most artificial and scholastic of the untruths that disfigure our science. One might as well say that the essence of prismatic color is pleasure and pain. There are infinite shades and tones in the various emotional excitements, which are as distinct as sensations of color are, and of which one is quite at a loss to predicate either pleasant or painful quality.

Now, accepting Dr. Worcester's description of the facts, I remark immediately that the nauseousness and pleasantness are due to incoming nerve-currents—at any rate in the cases which he selects—and the feeling of the involuntary movements as well; so whatever name we give to the phenomena, so far they fall comfortably under the terms of my theory. The only question left over is what may be covered by the words 'repugnance' and 'liking,' which I have italicized, but which Dr. Worcester does not emphasize, as he describes his instances. Are *these* a third sort of affection, *not* due to afferent currents, and interpolated between the gustatory feelings and reactions which are so due? Or are they a name for what, when carefully considered, resolves itself into more delicate reactions still? I privately incline to the latter view, but the whole *animus* of my critic's article obliges me to attribute to him the opinion, not only that the like and dislike must be a third sort of affection not grounded on incoming currents, but that they form the distinctive elements of the 'emotional' state of mind.

The whole discussion sharpens itself here to a point. We can leave the lexicographers to decide which elements the word 'emotional' belongs to; for our concern is with the facts, and the question of fact is now very plain. Must we (under any name) admit as an important element in the emotional state of mind something which is distinct both from the intrinsic feeling-tone of the object and from that of the reactions aroused—an element of which the 'liking' and 'repugnance' mentioned above would be types, but for which other names may in other cases be found? The belief that some such element does exist, and exist in vital amount, is undoubtedly present in the minds of all the rejectors of the theory in dispute. Dr. Worcester rightly regrets the deadlock when one man's introspection thus contradicts another's (288), and demands a more objective sort of umpire. Can such a one be found? I shall try to show now that it possibly has been found; and that Dr. Sollier's recent observations on complete anæsthetics show that in some persons at least the supposed third kind of mental element may exist, if it exists at all, in altogether inappreciable amount.

In my original article I had invoked cases of generalized anæsthesia, and admitted that if a patient could be found who, in spite of being anæsthetic inside and out, could still suffer emotion, my case would be upset. I had quoted such cases as I was aware of at the time of writing, admitting that so far as appearances went they made against the theory; but I had tried to save the latter by distinguishing between the objective reaction which the patient makes and the subjective feeling which it gives him. Since then a number of cases of generalized anæsthesia have been published, but unfortunately the patients have not been interrogated from the proper point of view. The famous 'theory' has been unknown to the reporting doctors. Two such cases, however, described by Dr. Berkley of Baltimore,* are cited by Dr. Worcester 'for what they are worth' in its refutation (294). The first patient was an Englishwoman, with complete loss of the senses of pain, heat and cold, pressure and equilibrium, of smell, taste, and sight. The senses of touch and of position were not completely gone, but greatly impaired, and she could hear a little. As for visceral sensations, she had had no hunger or thirst for two years, but she was warned by feeling of the evacuative needs. She laughs at a joke, shows definitely grief, shame, surprise, fear, and repulsion. Dr. Berkley writes to Dr. Worcester as follows: "My own impression derived from observation of the patient, is that all mental emotional sensibilities are present, and only a little less vivid than in the unanæsthetic state; and that emotions are approximately natural and not at all coldly dispassionate."

The second case was that of a Russian woman with complete loss of cutaneous, and almost complete loss of muscular, sensibility. Sight, smell, hearing preserved, and nothing said of visceral sensation (in Dr. Worcester's citation). She showed anger and amusement, and not the slightest apathy.

This last case is obviously too incompletely reported to serve; and in the preceding one it will be noticed that certain degrees of visceral and of muscular sensibility remained. As these seem the important sorts emotionally, she may well have felt emotion. Dr. Berkley, however, writes of her 'apathy'; and it will be noticed that he thinks her emotions 'less vivid than in the unanæsthetic state.'

In Dr. Sollier's patient the anæsthesia was far more complete, and the patient was examined for the express purpose of testing the dependence of emotion on organic sensibility. Dr. Sollier, moreover, experimented on two other subjects in whom the anæsthesia was artificially induced by hypnotic suggestion. The spontaneous case was a man aged forty-four; the hypnotic cases were females of hysteric constitution.† In the man the anæsthetic condition extended so far that at present every surface, cutaneous and mucous, seems absolutely insensible. The muscular sense is wholly abolished; the feelings of hunger and satiety do not exist; the needs of defecation and micturition

* Brain, Part IV, 1891.

† The paper, entitled 'Recherches sur les Rapports de la Sensibilité et de l'Émotion,' will be found in the *Revue Philosophique* for March of this year, vol. XXXVII. p. 241.

are unfelt; taste and smell are gone; sight much enfeebled; hearing alone is about normal. The cutaneous and tendinous reflexes are lacking. The physiognomy has no expression; speech is difficult; the entire muscular apparatus is half paralyzed, so that locomotion is almost impossible.

"'I *know*,' this patient says, 'that I have a heart, but I do not feel it beat, except sometimes very faintly.' When an event happens which ought to affect it [the heart, as I understand the text], he fails equally to feel it. He does not feel himself breathe, or know whether he makes a strong or a weak inspiration. 'I do not feel myself alive,' he says. Early in his illness he several times thought himself dead. He does not know whether he is asleep or awake. . . . He often has no thoughts. When he does think of anything it is of his home or of the war of 1870, in which he took part. The people whom he sees come and go about him are absolutely indifferent to him. He does not notice what they do. 'They do not appear,' he says, 'like natural men to me, but more like mechanisms.' Similar perturbations of perception occur also in hearing. 'I do not hear in the old way; it is as if it sounded in my ear, but did not enter into my head. It does not stay there long.' His *aprosexia* is complete, and he is incapable of interest in anything whatever. Nothing gives him pleasure. 'I am insensible to everything; nothing interests me. I love nobody; neither do I dislike anybody.' He does not even know whether it would give him pleasure to get well, and when I tell him that his cure is possible it awakens no reaction—not even one of surprise or doubt. The only thing that seems to move him a little is the visit of his wife. When she appears in the room 'it gives me a stroke in the stomach,' he says; 'but as soon as she is there I wish her away again.' He often has a fear that his daughter may be dead. If she should die I believe I should not survive her, although if I never were to see her again it would make no difference to me.' His visual images are non-existent, and he has no representation of his wife when she is gone. The weakness of the sensations remaining to him gives him a sense of uncertainty about all things: 'I am never sure of anything.' Nothing surprises or astonishes him. His state of apathy, of indifference, of extreme emotionlessness, has developed slowly *pari passu* with the anæsthesia. His case realizes, therefore, as completely as possible the experiment desiderated by W. James."

In the hypnotic experiments, Dr. Sollier provoked in his subjects sometimes visceral and sometimes peripheral anæsthesia, and sometimes both at once. He registered the organic reactions (by pneumograph, etc.) as far as possible, and compared them with those produced in the same subject when an emotion-exciting idea was suggested, first in the anæsthetic and then in the normal state. Finally, he questioned the subject on the impressions she had received. For the detailed results the reader must consult the original paper. I will only mention those which seem most important, as follows:

(1) Complete peripheral anæsthesia abolishes completely the power of movement. At the same time the limbs grow cold and sometimes blue (247).

(2) When visceral anæsthesia is added, the patient says she feels as if she no longer were alive (*ibia*).

(3) When totally anæsthetic she feels no normal emotion whatever at the suggestion of hallucinations and delusions which have the power of moving her strongly when the sensibility is restored. When the anæsthesia is less complete she may say that she feels not the usual emotion, but a certain stroke in the head or stomach at the reception of the moving idea (250, 254).

(4) When the anæsthesia is solely peripheral, the emotion takes place with almost normal strength.

(5) When it is solely visceral, the emotion is abolished almost as much as when it is total, so that the emotion depends almost exclusively on visceral sensations (258).

(6) There is sometimes a very slight motor reaction shown by the pneumograph in visceral anæsthesia when an exciting idea is suggested (Figs. 2, 7 *bis*), but M. Sollier thinks (for reasons of a highly speculative kind) that in complete *inemotivity* the visceral reactions themselves do not take place (265).

The reader sees that M. Sollier's experimental results go on the whole farther than 'my theory' ever required. With the visceral sensibility not only the 'coarser' but even the 'subtler' forms of emotion depart. Some people must then be admitted to exist in whom the amount of supposed feeling that is not due to incoming currents is a negligible quantity. Of course we must bear in mind the fallibility of experiments made by the method of 'suggestion.' We must moreover remember that the male patient's inemotivity may have been a co-ordinate result with the anæsthesia, of his neural lesions, and not the anæsthesia's mere effect. But nevertheless, if many cases like those of M. Sollier should be found by other observers, I think that Prof. Lange's theory and mine ought no longer to be treated as a heresy, but might become the orthodox belief. That part, if there be any, of emotional feeling which is not of afferent origin should be admitted to be insignificant, and the name 'emotion' should be suffered to connote organic excitement as the distinctive feature of the state.

William James.

Harvard University.

PSYCHOLOGICAL LITERATURE.

Introduction à la psychologie expérimentale. A. BINET. Paris, Alcan, 1894. Pp. 146.

It is to be regretted that M. Binet has not seen fit to lead the readers of the present volume to anything like a thorough and systematic grasp of the elements of experimental psychology. At a time when the scientific character of psychology is so much urged and disputed in France and America, it is of extreme importance that every contribution to the didactic literature of the subject should indicate the progress made towards enstating it in the high place claimed for it by its advocates—and by none more forcibly than M. Binet himself.

The volume opens with a long and circumstantial description of several of the more prominent laboratories in Europe, including a list of apparatus in the one at Paris, and a bibliography of the studies performed there. These details, which are of little interest to the novice, and which the more advanced worker would expect to find in monographs of a different character, might better have been relegated to a preface or appendix. After a short discussion of psychological methods, the author proceeds to give a few of the more striking and unusual experiments under sensation, attention, etc. These have the effect of arousing interest in the subject. Yet they will not, it is feared, impress the novice with the idea that the subject in hand is exact, natural, and capable, within ever-widening limits, of yielding precise numerical results. The student who takes up experimental psychology is not a child, and his wonder has no need to be stimulated in order to lead him onward in the pursuit. It would seem preferable, therefore, to subordinate these few startling anJ isolated facts to the many carefully-studied, interwoven phenomena which furnish the bulk of the science. Such a detailed treatment, while it may lack the literary beauty of a popular essay, need not, I believe, lessen its interest to the beginner, while it would add immensely to its value as a presentation of the elements of the newer psychology. The chapter on Movement is to be commended, on account of its appreciative treatment of that side of psychology, and its indication of the direction which recent research in this line has taken. But even here too much attention is bestowed on particular apparatus, and on special workers and studies. The treatment of association (under Ideation) and psychometry are more satisfactory ; of special interest is the discussion of the uses and meaning of the time measurements ; yet here again one might wish for more figures and definite results ; for though the absolute times, as M. Binet concludes, may not have any great significance or universal application, the *relative* length of the distinct phases of an act, and of different acts, are the most important determinations in that branch, as yet. In a chapter on Observation, the author devotes considerable space to a discriminating defence of the statistical method employed with such effect by Galton and others.

One cannot help confessing to a feeling of disappointment when it is considered what even a short book like this might have been and how much it might have accomplished for the science,—coming, as it does, from one amply qualified to judge the relative importance of experiments, and to give any number of them, so chosen as to arouse the deepest interest and, at the same time, picture accurately the present status of experimental psychology. H. C. WARREN.

PRINCETON COLLEGE.

Practical Lessons in Psychology. BY WILLIAM O. KROHN. Chicago, The Werner Company, 1894. Pp. 402.

This work is best described in the preface of the author. It originated in ' lectures on psychology, chiefly on those phases that must and do come in for a large share of consideration on the part of every successful teacher' ; and these, having first been ' delivered at various institutes and other gatherings of teachers,' are now collected into book form, with little change in plan or manner of expression, but with the addition of considerable new material. It is, therefore, only a ' collection of personal letters ' designed to ' develop *tact* on the part of the teacher,' to assist him in ' ministering to the wants of the growing child-nature,' and to interest him in child-study alike for the purposes of science and of practice.

This twofold aim—psychological and pedagogical in one—is shown in the divisions of the volume. Of the twenty-six chapters or ' lessons' two may be described as introductory ; four are on the brain and nervous system ; eight discuss sensation, including the development of the senses and sense-illusions ; three are devoted to association, memory, and imagination ; while the fifteenth takes up ' The Contents of the Child's Mind on Entering School' ; the twenty-fifth, methods of testing and measuring the mental faculties, especially in school-children ; the twenty-sixth, child-study ; and an appendix by Miss Cary of Champaign, Illinois, commends the principles and methods of kindergarten instruction to the educational public.

Professor Krohn writes in full sympathy with the recent developments of psychological science, and from the standpoint of a wide acquaintance with the latest results of physiological and experimental inquiry. He shares the conviction of many other workers in the same field, that the new science is fitted to furnish not only a sound basis for pedagogics, but also a needed corrective to much that is useless or even harmful in our present educational system. His work in its published form, therefore, will excite the same interest in the mind of the class to which it is addressed as it has already aroused when delivered from the lecture-platform. Further, it will serve to give many teachers a preliminary introduction to the newer forms of psychological and pedagogical science. But this, unhappily, marks also the limit of its influence. The selection of topics and the manner of treatment both display the effects of popular discourse in a way that is often tantalizing in the extreme. Sometimes the desire for vividness and attractiveness leads the author not only into statements of doubtful psychological value, but also into an imperfect use of his pedagogical opportunities. It is doubtful, for instance, whether the uninstructed reader will gather a clear understanding of the phenomena of aphasia, agraphia, and alexia from the discussions of p. 78 *et seq.*, even though the subject is undoubtedly calculated to enforce the fact of the correlation of mind and brain. The treatment of memory and imagination, again, in Lessons XIX–XXI is confused even to the student of psychology; and the omission of all the thought-functions except reasoning neglects the capital chance of driving home the author's excellent remarks on the importance of the imagination in intellectual development.

The careful reader will also notice the lack of reference to the sources of the plates, though many of them are old friends, and the entire absence of an index.

A. C. Armstrong, Jr.

Wesleyan University.

Man and Woman: a Study of Human Secondary Sexual Characters. Havelock Ellis. Illustrated. London and New York, Charles Scribner's Sons, 1894. Pp. 409.

There is a keen general interest in the problem with which this book is concerned—the real differences between the sexes, and the light thrown by them upon the possible future position of woman in social and political life. The author has had such questions distinctly in view through his long and conscientious study of his theme; but at its close he candidly acknowledges that the mass of material he has brought together, however valuable for many purposes, does not answer the inquiries for which many would turn to his volume.

"Our investigation shows us in what state of mind we ought to approach the whole problem; it can scarcely be said that it gives us the definite solution of definite problems. It is not on that account fruitless. It is something to have asked the right question and to have been put on the right road." No doubt of this; and a brief synopsis of Mr. Ellis' work will show that he has approached the subject from the right side.

The earlier chapters are devoted to the secondary physical differences of men and women—such as the growth and proportions of the body, the construction of the pelvis, the skull, and the brain in the two sexes; the variations which they exhibit in the acuteness of the special senses, in muscular power and manual dexterity. The crucial question of the intellectual contrasts between the sexes he enters into at length, with inconclusive expressions.

"It cannot be said that in this chapter we have reached any very definite results. A few careful experiments, which need confirmation and extension; a certain number of observations on irregular masses of data, accumulated in the practical experiences of life, which have their value, although they are open to various misinterpretations,—this is about all that experimental psychology has yet to show us in regard to the intellectual differences of men and women."

Positive differences between the sexes are the quicker reaction to physical and psychic stimuli in women—called by our author 'affectability'; their much less spontaneous and pronounced artistic impulses; their decidedly inferior tendency to variability in both the physical and intellectual fields; and their greater rapidity of growth, countervailed by a tendency not less distinct to an earlier arrest of development, which traits are quite as marked in their mental as in their bodily life. Women have the 'infantile diathesis.' They share their special characteristics with children. "We have found over and over again that, when women differ from men, it is the latter who have diverged, leaving women nearer to the child-type."

This sounds ominous for the fair sex. Immature in their essential differences, children all their lives, how can they claim equality with man? But with a *tour de force*, such as he displays in several critical conjunctures, Mr. Ellis saves his science and his chivalry at once. The child, the infant, in fact, alone possesses in their fulness 'the chief distinctive characters of humanity.' "The highest human types, as represented in men of genius, present a striking approximation to the child-type." "In man, from about the third year onward, further growth is to some extent growth in degeneration and senility." Hence the true tendency of the progressive evolution of the race is to become child-like—to become feminine.

"It would not be difficult to multiply examples of the ways in which women are leading evolution. In the saying with which Goethe closed his 'Faust' lies a biological verity not usually suspected by those who quote it."

All will admire the spirit of fairness which pervades Mr. Ellis' work. He has no hobby to ride, no pet theory to sustain; and his frank acknowledgment at the close, that "we have not reached the end proposed at the outset," will invest his extensive collation of facts with the more value to students from the certainty that they are presented with an unprejudiced mind; and as he rightly says that a man and a woman 'are the two most interesting beings in the world,' his book cannot fail to please and instruct a large number of readers.

DANIEL G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

The Psychic Factor: an Outline of Psychology. C. VAN NORDEN. New York, D. Appleton & Co., 1894. Pp. vii, 223.

This is a text-book written from an objective point of view. An elementary book of this kind is needed; and it is to the credit of Mr. Van Norden that he has anticipated the original workers in the field of the borderland of mental science in presenting an outline of their more striking results, which is, as far as it goes and in the main, accurate. The author devotes more than half of his book to the results of such 'objective' (i.e., pathological, comparative, genetic) results under the caption 'Mind in General.' Some of the sections are Mind in Plants ('Living matter is always psychic'), Mind in Animals (from a thorough-going genetic and evolution point of view), Subconsciousness, Sleep, Somnambulism, Hypnosis, Thought-transference, Lucidity, Hysteria, Criminality. The treatment of these topics is generally well informed, but dogmatic, on a basis of too little literature; although the author's authorities are generally well chosen. The rest of the book is an attempt to say as little as possible of the psychology of consciousness—the subjective point of view. 'The Feelings' are given five pages, and 'Willing' four, under the general caption 'Mind in Detail.' The treatment of the intellectual operations is about as inadequate. The book will prove useful, doubtless, for supplementary reading in connection with an elementary work in general psychology, in the hands of teachers who know their authorities; but it is in no sense an 'Outline of Psychology,'—whether it be or be not a good treatise on what the author privately means by the barbarous major title,—and should not be substituted for a text-book on academic psychology. J. M. B.

Classification objective et subjective des arts, de la littèrature, et des sciences. R. DE LA GRASSERIE. Paris, Alcan, 1894.

The author proposes a classification which includes not only the sciences, but also the arts and literature, i.e., all the manifestations of the mental activity of man. We are able to notice only two points from this compact volume: the place given to psychology and the author's examination of the classification of Wundt.

I. Wundt proposed a new classification in the *Philosophische Studien* (1888), the last before this of M. Grasserie. He criticised Comte and Spencer, holding that Comte gave an inadequate recognition to sociology and still less to psychology. This is surprising to those who are familiar with the work of Comte: for he was the first to propose the term Sociology, and he devoted one of the six volumes of his *Cours* to the discussion of this science, advocating its place.

As to Spencer, Wundt charges him with attaching psychology to biology and with giving too great place to sociology. Further, Wundt claims that both Comte and Spencer were quite wrong in eliminating philosophy, or metaphysics, entirely from the body of the sciences. In this connection we may recall the *Discours sur l'organization des sciences philosophiques*, in which Jouffroy, the psychologist of pure eclecticism, holds that the sciences must separate from metaphysics in proportion as they discover their own proper methods of research. Wundt's principle of classification is that the world and the mind, despite their apparent opposition, are not really distinct, or at least are not universally recognized as such. On this ground he distinguishes the 'formal,' or abstract, from the 'real' sciences. The latter are further divided, following the formula of Bentham, Ampère, and Stuart Mill, into Sciences of Nature and Sciences of Mind. To the Sciences of Mind psychology belongs, and it has several branches: (1) *Psychology of Animals*, which deals with the presence of consciousness in the phenomena of life; (2) *Race Psychology*, which investigates consciousness in different peoples and races; (3) *Psychology Proper*, which studies the relation of mental life to certain bodily processes; (4) *Psychology as Natural History*, which inquires into the development of humanity, on the basis of ethnology and anthropology. Finally, dominating both the domain of these different sciences and that of morals, art, and religion, Wundt places *Philosophical Psychology*, or the philosophy of mind considered apart from all this material: the fundamental and synthetic idea of the life of mind. Thus considered, psychology is the general foundation of all the mental sciences.

II. M. de la Grasserie's classification is drawn from all that precede him. He divides the manifestations of intellectual activity into three groups: (1) the arts and literature; (2) the sciences of nature; (3) the so-called sciences of man. Psychology belongs to the third group, and beside it are law and political economy. It unites with anthropology on the physiological side; but it is at the opposite extreme from the group of studies formerly designated by the vague word Philosophy. Metaphysics is an inquiry into the origin and end of the world and man: it escapes the method of direct observation. But psychology is quite different. It is the science of the soul according to some; of the cerebral functions according to others. In either case it is a science of functions and phenomena.

For a long time psychology has vainly attempted to constitute itself deductively or *a priori*. To-day it proceeds inductively and rests upon directly observed facts like other sciences. Psychology includes: (1) The psychology of the facts of our lower activity—those which are less intense in proportion as we descend in the scale of beings. This study is destined to explain the phenomena of the unconscious life of man. (2) Morbid psychology, which studies the abnormalities of human activity. This science is still in process of formation, having been hindered by the theories of charlatans. It studies, in acute forms, the phenomena of which normal psychology studies the regular forms. (3) Normal psychology: the study of the mental functions in all their manifestations as the old psychology studied them. But it frees itself from the dogmatism of the old and its metaphysical method. The new psychology proceeds scientifically.

J. PHILIPPE.

SORBONNE, PARIS.

Hegel's Philosophy of Mind. Translated from the Encyclopædia of the Philosophical Sciences, with five Introductory Essays. W. WALLACE. Oxford, Clarendon Press. 1894. Pp. cciv, 202.

This new work from the pen of the eminent English exponent of Hegel has a twofold value. The translation itself presents, with the well-known excellence of Prof. Wallace's earlier version of the 'Logic,' the third and concluding part of Hegel's 'Encyclopædia.' Students of psychology should be very grateful for a work which is itself extraordinarily suggestive on many modern points of view, coming as it does in a translation which is itself an interpretation.

The 'Five Introductory Essays' occupy more than half of the work. They cover many topics of current interest, and give Professor Wallace's reflections on modern psychology. The titles are: 'On the Scope of a Philosophy of Mind,' 'Aims and Methods of Psychology,' 'On Some Psychological Aspects of Ethics,' 'Psycho-genesis,' 'Ethics and Politics.' These essays are discursive, sometimes grievously repetitious, always suggestive, occasionally valuable, i.e., as stating a point of view in exposition or criticism with luminous clearness. Their greatest value, I think, resides in the fact that they show the essential sympathy of thinkers of the idealistic school with the natural-history method, and with the experimental method, as well, now so zealously advocated by competent psychologists. And with this goes the implied rebuke which the work itself will administer to some who are only psychologists. Some, I venture to say, will read Wallace's part of the work without reading Hegel's. Raillery at metaphysics, and especially at the Hegelian metaphysics, has been indulged in by many men who do not see that such talk only betrays ignorance of philosophical system and absence of philosophical culture. For it is safe to say that Hegel's influence in bringing the philosophical thought of the time up to the freedom of genetic and comparative research in psychology is due as much to Hegel as to the much-talked-of but little-read Herbart—I personally should say more. Let us remember that, however successful we hope the attempt will be to throw off the bonds of crude and incompetent speculation, yet psychology can never place herself outside the organic development of thought in the sphere of the humanities. If she should succeed in this it would only render her own independence temporary and unfruitful, and she would lose her opportunity of service to modern culture and life. The permanent justification of all science is philosophy; and it is philistinism in spirit and fatuity in policy to secure independence of the mother discipline by a hasty attempt to stab her in the back.

A great defect of Professor Wallace's essays is what I may call their 'personal ambiguity.' Possibly it is because Plato set so flagrant an example of it that all the writers of Professor Wallace's coterie prefer to conceal their personality behind a screen of history. To any one not familiar with Hegel's treatise—I had almost said, *in the German*—the essays in this book will be extremely ambiguous in respect to what is Hegel's and what is Wallace's. The section on Hypnotism is an example (pp. clxiv ff.). Surely it is not fair that a reader should be obliged first to read Wallace as a help to Hegel, then to read Hegel to find out the faithfulness of Wallace, and then to construct a calculus of difference on the basis of wider reading of both Hegel and Wallace. It is all very well to discourse about the continuity of historic insight; but in psychology we emphasize doctrines more than insight, and we want to know both who our teachers are and when they were born.

J. M. B.

SENSATIONS OF THE SKIN.

The Relative Sensitivity of Men and Women at the Nape of the Neck (By Weber's Test). FRANCIS GALTON. Nature, May 10, 1894.

Mr. Galton has tabulated the results of Weber's test upon 932 men and 377 women, with the object of presenting whatever characteristic differences between the sexes might appear. The results were gathered at Mr. Galton's laboratory in London, and may be regarded as pertaining to the general public, or at least such portion of the public as would be sufficiently interested in the matter to submit to the test. The nape of the neck was chosen because it was a portion of the skin, not used or worn, in which all persons would be equally unpractised, which could not be seen, and which was of fairly coarse sensitivity. The record consists of the minimum distance in millimetres between the compass-points which when applied could be perceived as two points. The smallest perceptible interval for the average man is 13.8 mm; for the average woman, 11.8. In general, then, "the average delicacy of female discrimination between the two points is to that of the male in a ratio that lies somewhere between 7 to 6 and 8 to 7, or thereabouts." The question of the distribution of men and women in this respect is presented with the exactness and ingenuity in statistical method that characterizes Mr. Galton's contributions. It appears that the variability of women is greater than that of men, and again in a ratio of about 8 to 7. This variability may be a physiological fact, or it may be a psychological fact due to the greater variability in the powers of attention and observation required in the test. The latter factor, in the author's view, is at least a partial cause of the difference in question.

Mr. Havelock Ellis, in his recent work on *Man and Woman*, has indicated the bearing of researches of this kind upon the general view of secondary sexual differences; and the present results form a valuable contribution to the anthropometry of the tactile sensitivity.

JOSEPH JASTROW.

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FUNCTIONS OF THE INTERNAL EAR.

Funktion und Funktionsentwicklung der Bogengänge. KARL L. SCHAEFFER. Zeitsch. f. Psych., VII. 1–9. 1894.

Akustische Versuche an einer labyrinthlosen Taube. W. WUNDT. Phil. Stud., IX. 496–509. 1894.

Schaeffer's article is in continuation of his whirling-table experiments described in part in Bd. III. pp. 185–192 of the Zeitsch f.

Psychologie. He there showed that invertebrates, i.e., animals without the labyrinth, were not made dizzy by being whirled on the table. The present article promises a publication of copious results confirming the previous investigation, and in addition describes an *experimentus crucis* on tadpoles from which it appears that whirling produced no effects of 'manège' motion on these animals until the semicircular canals had become developed. Symptoms of giddiness appeared as soon as the formation of the canals was completed. The article is prefaced by a scanty history of the development and literature of the problem. Curiously enough Ewald's research on the eighth nerve is not mentioned.

In vol. VIII. of the Phil. Stud., Wundt discussed some experiments (Scripture, Phil. Stud. VIII. p. 638; Cross and Goodwin, Proc. Am. Acad. Arts and Sciences, XXVII. 1891) on the direct excitation of the auditory nerve by sound-waves, with especial reference to their bearing on the doctrine of specific energy of the nerves. Among other facts in support of his views Wundt mentioned the experiments by Ewald on the hearing powers of pigeons from which both labyrinths had been extirpated. As a result of this Ewald sent Wundt a pigeon from which the labyrinths had been removed, with the condition that if Wundt should become convinced that the pigeon could hear he should give some public expression to his conviction. The present article is practically the 'protocol' of the experiments on Ewald's pigeon and a normal 'standard' pigeon. The sounds used were clangs, compound clangs, and noises (rapping on the door, electric bells, etc.). The birds were observed in a dim light by means of a telescope placed in the adjacent room. First Ewald's pigeon was experimented upon by a series of stimuli occurring at intervals of two minutes, and then the normal bird was subjected to a like series. The 'reactions' consisted in sudden starts, movements of the head, of the eyes, and of the eyelids. With the exception that Ewald's bird gave but doubtful signs of being affected by tones above 440 vibrations, there was little difference in the general way in which the pigeons 'reacted' on the sounds. Both birds reacted on a majority of the stimuli, and both often failed to react, especially when a stimulus had already been several times repeated. To meet the objection that reactions were in response to sensations of touch, the tympana of Ewald's bird had been removed. Moreover, for both birds the sound-stimuli were produced in an adjacent room, while both failed to react on violent disturbances of the air from bellows worked immediately beneath the cages.

After completion of Wundt's experiments, Ewald's pigeon was handed over to Dr. Held, Docent in Anatomy in Leipzig, for microscopic examination of the labyrinths and adjacent parts. Held found that the semicircular canals had either disappeared or were without nerve-tissue. There were no traces of pus and the cerebellum was uninjured.

In how far the pigeon deprived of labyrinths perceived sound it is almost impossible from the nature of the reactions to say; but the same may be said in regard to the normal bird.

The facts that the reactions increased with the intensity of the sounds and that reactions on a new sound followed inaction after a series of like sounds would seem to indicate a perception of quantitative and qualitative differences in the stimuli. But at any rate this evidence, together with the experiments on the intercranial conduction of tones, make it highly probable that the action of the labyrinth is not essential to the perception of sound. F. ANGELL.

STANFORD UNIVERSITY.

THE PERCEPTION OF DISTANCE.

Das Verhältnis von Accommodation und Konvergenz zur Tiefenlokalisation. FRANZ HILLEBRAND. Zeitsch. f. Psychol., VII. 97–151. 1894.

This article reports the following experiments: An apparatus is so arranged that the subject, with one eye closed, looks with the other through a tube whose further end is closed by a diaphragm with an opening measuring 1 cm × 1.5 cm; and fixates the sharply-cut vertical edge of a piece of black cardboard which fills up one half of the field of view, the other half being occupied by a brilliantly-illuminated white glass plate, which forms a background at a distance of a little more than one meter from the eye. The black cardboard can be moved smoothly backward and forward, its edge retaining constantly the same position in the field of view. By this arrangement all 'empirical' factors—varying size of retinal image, double-images on the two retinæ, etc.—which might reveal the distance of the edge fixated are excluded, and it becomes possible to determine the influence of accommodation and of the convergence which accompanies it even when one eye is closed. Experiments performed under these conditions showed:

1. That when the fixated object was moved backward or forward within the range of easy accommodation, and with a rapidity which allowed it to be constantly accommodated for, it was impossible to tell with any accuracy whether the object was moved nearer or farther away.

2. That when the fixated object changed its distance so suddenly that it was impossible to follow it with accommodation, each observer was able to tell accurately in which direction the movement had taken place, if the difference in distance was sufficiently great.

In another series of experiments, in place of the black cardboard was substituted another cardboard with an adjustable diaphragm which entirely concealed the white background except so much as could be seen through the diaphragm. If now the opening remained unchanged in distance but was made smaller or larger, the observer obtained the impression of an increase or decrease in distance, although accommodation and convergence did not alter. If the cardboard was moved nearer, and the opening at the same time was more than proportionally diminished, then in spite of increasing strain of accommodation the observer believed that the object had been moved farther away.

From these results Dr. Hillebrand concludes that accommodation and convergence, when thus isolated, give us no information in regard to the distance of the point of fixation; in other words, that 'so-called muscle-sensations' have no existence, or at least have no importance in visual localization. Without them it is easy to explain the results mentioned under (2) above, on the supposition that when the change in distance is sudden the observer voluntarily changes his accommodation, and knows, since it is voluntary, whether the new accommodation is for a nearer or farther object; and hence knows also whether the object has moved nearer or farther away, because if his first change of accommodation has not succeeded in making the image of the object clearer, a second change in the opposite direction will do so. When, however, the change in accommodation is involuntary, as in the experiments under (1), there is no means of knowing the direction or even the fact of movement, since, as these experiments establish, the changes in accommodation and the always accompanying changes in convergence yield no muscle-sensations to reveal the nature or the existence of these changes.

E. B. Delabarre.
Brown University.

ASSOCIATION, REACTION.

Minor Studies from the Psychological Laboratory of Cornell University. Communicated by E. B. Titchener. I. *'Mediate' Association.* H. C. Howe. II. *'Sensorial' and 'Muscular' Reactions.* A. R. Hill and R. Watanabe. Am. Jour. of Psychol., vi. 239–246. Jan. 1894.

Professor Titchener contributes the first-fruits of the Psychological Laboratory of Cornell University, from which much admirable work in experimental psychology may be expected. Mediate association and sensory and muscular reactions seem to be receiving more than their due share of attention just now, but these experiments are presented in a concise form which should serve as a model.

Mr. Howe repeated and varied Dr. Scripture's experiment on mediate or subconscious association, and found (as Prof. Münsterberg had previously found) that he could not confirm Dr. Scripture's results. Dr. Scripture's experiment consisted in presenting for a short time words with obscure signs, which latter the observer was supposed not to notice distinctly nor remember. When one of the words was presented a second time, a word having the same sign was likely to recur. The present writer remarked in *Mind* some years ago that subconscious association seems to play an important part in the sequence of ideas. When an observer is asked to name the first word suggested by a given word, the word actually named seems often to be connected with the original word by links which are not given in distinct consciousness. But the process is one difficult to observe or to study by experiments which are unambiguous.

Messrs. Hill and Watanabe made experiments on sensory and motor reactions in which the movement was made with the lips and with the closed thumb and index-finger. They found the distinction to obtain in these cases as well as when the movement was made in the usual fashion. The Wundt controlling hammer was out of order, and the times and differences are said not to be absolute, but in most cases the sensory times were 78–155σ longer than the motor times. With some of the observers a distinction between motor and sensory reactions could not be found and the writers conclude that "not every person is able to function as a reaction-subject. Rather is there required for the work a special kind of mental disposition or *Anlage*." The present writer does not consider the interpretation of motor and sensory reactions given by Professor Wundt and Dr. Lange as valid, but—as the President of the University from which these studies come has remarked —a man has before this gone out to look for asses and found a kingdom. In the manner suggested independently by Professor Flournoy and Professor Baldwin the attitude of the subject in reacting may throw light on the important distinction between visual, auditory, and motor 'types.'

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. J. J. van Biervliet. Philos. Stud., x. 160–167. 1894.

Professor van Biervliet describes with unusual clearness experiments made with unusual exactness on the relation between the rate of the pulse and the length of the reaction-time. In the case of 10 of the 11 observers the reaction-time tended to become shorter as the pulse was quicker. Thus with one observer the following results were obtained:

Pulse	70 to	80	Reaction-time 130σ
	80	90	126σ
	90	100	121σ
	100	110	117σ

In other cases there is less regularity, but the number of experiments and careful methods justify the conclusion that "in general the reaction-time for sound is lessened as the rate of the pulse increases." The reactions were 'sensory,' but the times and their variability are about normal, and it does not seem possible that they could be much reduced by directing the observer to make the reactions 'motor.' These 11 observers and the 8 tested by Dr. Dessoir must be counted among those who have not the *Anlage* necessary for experiments on sensory and motor reactions.

Mediate Association. W. G. SMITH. Mind, N. S. III. 289–304. July, 1894.

The first place in the July No. of *Mind* is given to experiments made by Dr. Smith at Leipzig and used in an inaugural dissertation. Dr. Scripture's experiments were once more repeated, and a variation was introduced by partly memorizing the series. The result may be readily summed up in the words of the author. "Münsterberg's results were entirely negative; so are those of the present research." While Dr. Smith's paper does not contain any important advance in experimental method, the discussion is timely and interesting. The connection of non-contiguous terms in a series which makes it easier to learn the alternate terms when the whole series has been previously memorized (Ebbinghaus) is attributed to motor activity, and Hume's reference to indirect association is quoted and criticised. We may expect further experiments on mediate association from the Yale Laboratory which will clear up discrepancies between Dr. Scripture's original experiments and the results of later investigations.

Ein Beispiel von Association durch unbewusste Mittelglieder. Dr. W. JERUSALEM. Philos. Stud., X. 323–328. 1894.

Sind die Mittelglieder einer Mittelbaren Association bewusst oder unbewusst? W. WUNDT. Philos. Stud., X. 326–328. 1894.

Dr. Jerusalem reports a case given him by a careful observer. The observer, immersed in work, suddenly saw in imagination a scene of which he had not thought for many years. Tracing the cause he found it to be the unnoticed fragrance of a flower in the room, which flower had been part of the original scene.

Professor Wundt adds a note to this observation in which he argues that the smell of the flower was unnoticed but not unconscious—it was perceived but not 'apperceived.' J. McK. C.

PLEASURE AND PAIN.

The Psychological Analysis and Physical Basis of Pleasure and Pain. LIGHTNER WITMER. The Journal of Nervous and Mental Disease, April 1894, pp. 209–228.

In view of the prevalence of the theory that pleasure and pain are qualities attributable to all states, but not themselves independent sensations, and especially in view of Mr. Marshall's presentation of this theory, Dr. Witmer shows that this is not the most simple and intelligible doctrine, nor that to which the latest discoveries in physiology point. Our current classifications, that of the five senses, for instance, are not founded primarily on introspection or upon the arrangement of feeling accordings to their inward similarity, but rather these classications are based on psycho-physical observation, that is, they are groupings of sensations according to the identity of their organs or the continuity of their known external causes. This is the case with heat and cold, and with pleasure and pain. Thus the definition of a sensation may be psycho-physical or introspective ; it may mean the whole mental effect of exciting a peripheral organ or it may mean the simplest unanalyzable element of consciousness. Now in both senses pleasure and pain are probably sensations : in the former, because recent discoveries show the existence of pain-nerves and suggest the possibility of nerves for pleasure ; in the latter, because in cases of extreme pleasure or pain there is hardly any image present, and because, most perceptions being very complex, the existence of pain or pleasure as elements in them is perfectly compatible with the essential independence of these elements. G. SANTAYANA.

HARVARD UNIVERSITY.

EMOTION.

Recherches sur les rapports de la sensibilité et de l'émotion. P. SOLLIER. Revue Philosophique, XXXVII. 241–266. 1894.

Prof. James' Theory of Emotion. D. IRONS. Mind, N. S. III. 77–97. 1894.

Both the above articles deal with James' theory of emotion. The first seeks to confirm it by experimental evidence, the latter subjects it to a searching and, in the author's judgment, annihilating criticism.

The opening of Dr. Sollier's paper is not calculated to inspire confidence. In the very first paragraph he mixes up the dates at which James and Lange first published their theories of emotion, makes James, who is certainly no pedant, deny that the objective corporeal phenomena which invariably accompany the emotion are its expression, ambiguously states the theory as being that these phenomena are 'its constituents, not to say its cause,' quite overlooking James' admission that there may be purely cerebral emotion, makes the absurd claim that Lange gave the theory a more physiological and *therefore* more scientific basis "in showing that all the phenomena which constitute the emotion are of two orders, vaso-motor and motor," and attributes to Lange the fanciful opinion that the emotional process has its seat in the medulla oblongata. The sequel takes as its point of departure James' suggestion as to what would afford proof positive of his theory, namely, a subject perfectly anæsthetic inside and out who should manifest all the bodily phenomena which usually accompany an emotion without feeling it. But even here Sollier does not report correctly. He makes James say that such cases of absolute anæsthesia are impossible because they would involve death; what James does say is that such cases are extremely hard to find. Parenthetically, however, it may be remarked that James' idea of a test case contains a manifest contradiction. If a subject were totally anæsthetic, an emotion-inspiring object could not affect him at all, much less would he be able to give an account of his experience. The anæsthesia would be necessarily limited to muscular and tactile anæsthesia. But the conditions can be partially realized. There is the well-known case of Strümpell's boy, which, however, as far as the evidence went, was unfavorable to the theory. Sollier refers to a recent case—that of a man who spontaneously developed profound visceral and peripheral anæsthesia and at the same time and to the same degree emotional apathy. It is to be observed, however, that this man still has feelings of distress on waking, of a moving sort on seeing his wife, and of fear lest his daughter should die, a fear so great that he feels that if she were to die he would not survive the shock. Sollier takes no account of these facts, but simply dwells on the parallel development of the general insensibility and the general apathy. His questions in examining the man do not appear to have been particularly searching nor his methods of investigation very exact. He does not, for instance, attempt to discover what bodily processes may have accompanied the afore-mentioned emotions; indeed, to have done so would have involved him in a certain perplexity. For if, on the one hand, none of the ordinary phenomena had been discoverable, it would have proved conclusively that they at any rate could not have been 'constituents, not to say causes,' of the emotions ; if, on the other hand, they had been found to be present, the same conclusion would have been necessary, provided the body were really anæsthetic. Nor is the slightest consideration given to the facts which indicate a central origin, or at least a central element in the origin, of the emotion ; and yet, with most of the special senses profoundly and all to some degree impaired, with the apperceptive processes so reduced that the significance of many things is no longer noted, and with the functions of the brain so generally restricted that there is often no thought at all of anything, enough is surely suggested pointing to this view that was worthy of more thorough investigation.

Similar remarks apply to Sollier's treatment of the two hysterical subjects in whom he developed visceral and peripheral anæsthesias by suggestion. He concludes as the results of his experiments that the suppression of general and sensorial sensibility involves the abolition of emotivity to the same degree as the anæsthesia, that the phenomena of muscular and special sensibility play in this relation but a very small part, and that in all cases the state of emotion depends directly on the sensibility and not on motor phenomena as such. Now these results are certainly important, and in themselves are not surprising. But one cannot help entertaining a certain reserve towards them as general principles when one considers the small number of the cases, the extremely limited range of the emotions sought to be excited (really only two, joy and grief), the laxness of procedure, and the conflicting state of the evidence. As to the last points : the patients say in their anæsthetic state that they feel nothing and yet they also say that their members feel numb and cold ; one of them declares that, in a certain experiment, she felt nothing, having just previously declared that she felt an impulse to laugh; and the same patient is supposed to be free of emotion when, as she states, she derives no pleasure from the knowledge that her lover adores her, but is only flattered ! In spite of the fact that James' ideal case supposes, along with the anæsthesia, the full play of the emotional expression, little is done to determine the latter and its relation to the emotional process ; the pneumograph is used but not the plethysmograph, and we are never sure that we have the full emotional expression in any case, the indications being rather the other way. We have, indeed, a case like this : M., profoundly hypnotized and 'insensible,' is told that her father is dead. There is a deep respiration and a very slight contraction of the face as though she is about to cry. But she feels, we are told, no emotion, but only physical sensations of having been struck in the head and stomach. But these are very far from being all the normal expressions of grief ; and so far as they are such expressions, they suggest that there are ways of feeling *them* without feeling emotion, and here, of course, the condition of bodily anæsthesia is wanting. But even if it should prove true that bodily and especially organic sensibility is essential to emotion, it is still far from being proved that emotion is constituted solely by feelings of somatic resonance; it might still be that the bodily phenomena were produced, as Sollier once inadvertently writes, 'under the influence of emotion,' or at least of some form of cerebrally-originated feeling. To prove the contrary, it would be necessary to show either that no such thing exists as cerebral sensibility, or that, if it does exist, it is independent of general bodily sensibility.

The theory of the mechanism of emotion which rests on the identification of kinæsthetic and cœnæsthetic sensations and centres, and which assumes a cortical vaso-motor centre in the region of the sensorial centres as the seat of the emotional process, can only be mentioned.

The thorough overhauling which James' theory meets with in the paper of Mr. Irons proves this much, at any rate: that, in order to avoid natural misunderstandings, both the theory itself and some of the arguments to support it require to be carefully restated. It is contended, and strongly, that the evidence to show that bodily changes follow the perception of the exciting fact without intervening emotion, *in cases where there is any emotional experience at all*, is meagre and questionable; and against the second part of the theory, to wit, that the emotion is the feeling of the bodily changes, it is urged that though consciousness of bodily disturbance almost always involves emotion, in and for itself, this consciousness is not emotional at all. What James describes as the vital point of the whole theory, the challenge to imagine an emotion after abstracting from the bodily feelings, is met by the logical rejoinder that inseparable correlates are not indistinguishable, and by the appeal to introspection, which very well distinguishes between organic sensations, the consciousness of bodily affections, and emotion as feeling-attitude towards an object. James himself bears indirect testimony to this view in describing doubt and belief by this very term 'psychic attitude,' and against the universality of his theory by teaching that æsthetic emotion is *directly* sensational. As to this last point, Irons remarks acutely that in replying to the objection that the theory breaks down, James' answer is an insistence that it does (see *Principles of Psychology*, II. 468). Then there is the experimental evidence of Strümpell's boy, the force of which, it is urged, is not to be set aside by an abstract doubt. The psychological principles on which the theory rests are thoroughly sensationalistic. The true view can only be given by a spiritualistic psychology. The self reacts as a whole to the stimulus. Emotion is essentially a purely psychical process with bodily results. It is not mere pleasure or pain, but a species of feeling-attitude, excited by the object but mediated by the activity of the self, and differing from other feeling-attitudes, such as moods and temperaments, interest, expectation and indifference, by its felt diffusedness and strength.

There is much in this contention to agree with, especially the recognition of the unique character of emotion as neither an aggregate of pleasure-pain feelings nor mere sensation of qualities, but as feeling-in-regard-to an object—it might have been added, with impulsive tendency. But we cannot help suspecting that the controlling idea in the author's criticism of Prof. James rests on a misconception, a misconception for which the latter is partly responsible. Irons seems to think that James denies the reality of emotion altogether, that he reduces it to the bare consciousness of the objective bodily phenomena as such. Hence he substitutes for the phrase 'feeling of the bodily changes as they arise' the phrase 'consciousness of bodily changes,' and speaks of 'this *feelingless* theory of emotion.' But is this necessarily James' meaning? Emotion is what it is, however it may be constituted; and just as the sensation of color might be spoken of as the feeling of and as constituted by the minute molecular changes occurring in the visual centre, notwithstanding that it is not the objective consciousness of these cerebral phenomena at all, so emotion might be spoken of as the feeling of physical disturbances in the body in the sense that it is a form of the subjective reflex in which those disturbances are manifested to consciousness. It would not be necessary then to deny the distinction between emotion and the sense-feeling of the bodily states themselves. We should rather distinguish two aspects in the 'feeling' of the bodily phenomena; for instance, (1) sensations with affective tone entering into the painful consciousness of the fact that the heart flutters, the breathing is hindered, etc., and (2) the state of fear arising from the synthesis of these and other connected sensations, the feeling-impulse attitude of the subject towards the object. Whether any such distinction as this lurks hidden beneath James' conveniently ambiguous terminology we do not know; only without it it hardly seems possible to do justice to the experienced facts. To say that bodily resonance is essential to emotion is one thing, although Prof. James does not insist even on that; but to say that emotion is nothing but the feeling of the bodily resonance is quite another, and is contradicted by the only possible authority in this matter, introspective consciousness, if by 'feeling' be meant nothing more than consciousness of a distinguishable aggregate of sensations in muscles and viscera and on the surface of the body. Physiological psychology is not called on to deny the 'spiritual element' in experience, but to explain the conditions of its genesis ; and the question here is whether the 'psychic attitude' of feeling and impulse which we designate as an emotion arises as a subjective reaction on the cerebral excitement connected with the perception of an 'object' prior to the subsequent changes in the rest of the body, or whether it is one form of the consciousness of those changes themselves. Perhaps it may be necessary to more carefully distinguish than has sometimes happened the different phases in the development of an emotion. On the other hand, the courage of a so-called spiritualistic psychology is to be more wondered at than imitated, which at the present day insists, as Mr. Irons appears to do, that there are certain elements in states of consciousness which represent a purely subjective reaction without any corresponding bodily processes whatever.

Le sentiment et l'analyse. F. RAUH. Revue Philosophique, XXXVII. 499-513.

Analysis sometimes multiplies ('Egotists') and sometimes suppresses or attenuates feeling (Spinoza) ; sometimes it 'troubles' feelings in their development (Bourget) and sometimes it enters in a peculiar way into their normal development (Rauh). To account for these different effects, M. Rauh makes use of Paulhan's and Fouillée's theory of psychological dynamism : the feelings are regarded as quasi-independent entities. The dynamism, however, is not only or mainly 'pure,' but also and more generally 'qualitative,' and in certain important respects 'intellectual.' In other words, feelings appear now as brute forces simply, measured solely by their effects and their duration, now as brute forces but of specific quality, and now as unconscious intelligent forces. In the first case, analysis may serve simply to add to the original pleasure the pleasures derived from consideration of the details, a passion by its sheer strength may suppress the intellectual pleasures or the intellectual activity may calm or suppress the emotion. In the second case, the feelings influence one another as like or unlike according to their several affinities or repulsions, and to these are referred the changes in the original feeling consequent on the new feelings aroused by distraction of the attention in reflection. The kind and variety of the changes thus brought about require the further supposition that each feeling tends to assimilate from among the elements of consciousness present at a given time those that are favorable to it and to repel those that are opposed, the promptness and completeness with which it does this being dependent on its strength. This leads, in the third place, to the recognition of the teleology of feeling : emotion, when it has attained a definite character, tends to select and reject with reference to its own satisfaction. Here feeling appears not only as a force, but as an instinctively intelligent force, attaining its end not merely by its strength, but more particularly by its tact in selection. This is the cardinal point in the theory, from which the practical conclusion follows that persons with the mania of analysis are maladroit. Passion, in order to master the spirit of analysis, must absorb it.

To the objection that the language used in the foregoing theory is metaphorical, the author replies that a metaphor which serves to explain the facts comes near to being a scientific conception. However, the dynamic conception is not, in his judgment, adequate to cover all cases. "The feelings can no more be regarded from a single point of view than the other facts of mind." There are certain anomalous phenomena, such as Descartes' predilection for squint eyes, which seem to represent, not tendencies in process of evolution, but absolutely rigid facts, without even the history or the capacity for modification of a habit, and these are best treated from the point of view of 'English phenomenism.'

On the Nature of Æsthetic Emotion. BERNARD BOSANQUET. Mind, N. S. vol. III. 153–166. April, 1894.

Most English writers have inclined to regard æsthetic enjoyment as an aggregate of pleasurable feelings passively received from the beautiful object; Bosanquet holds that the mind of one who, in contemplating the object, goes beyond the first undiscriminating impression which finds utterance in the exclamation 'How beautiful!' is essentially active and tends to assume the attitude of the 'maker.' His theory is that the central characteristic of æsthetic emotion is an aspect of the 'expressiveness' which is the central characteristic of æsthetic presentation, and that the two aspects, the presentational and the emotional, being strictly correlative, we have no need to violate a logical principle by ever assuming plurality of alternative causes for the 'circle of effects' known as beauty. 'Æsthetic emotion first arises in and is constituted by expression for expression's sake'—the phrase marking the generally accepted distinction between the æsthetic and other points of view. Emotions aroused by mere associations irrelevant to the context of the universal pervading the presented content are not properly æsthetic at all. In this connection there is a sharp criticism of the tendency of associationist æsthetics to obliterate the line between what is beautiful and what is personally interesting and to count among the æsthetic feelings the suggestions of mere dumb gratification of the senses. No truly æsthetic emotion, not even where it concerns particular sense-elements,—for instance, a particular color,—is ever quite dumb ; it is always felt as in a sort an articulate utterance. H. N. GARDINER.

SMITH COLLEGE.

Sur les idées générales. G. MARCHESINI. Rev. Philos., XVIII. 489–498. 1893.

A chapter from an unpublished work on 'Psychological Monism.' The author takes the position that the source of every mental fact is a datum of the senses, and every abstract form "merely a logical aspect which the sense-datum and the association of sensations acquire by means of analysis." "An idea is a sensation which has left a mental residue capable of elaboration." Through this latent persistence of sense-data (memory) we come to recognize that qualities belonging to one concrete belong to several concretes; and this resemblance may pass over into a general concept, i.e., into the idea of the possibility of generalizing qualities which are perceived in succession. The rest of the article is an elaboration of this position and a refutation of the Kantian view of *a priori* mental forms. H. C. WARREN.

PRINCETON COLLEGE.

NEW BOOKS.

Locke's Essay concerning the Human Understanding. A. C. FRASER. Oxford, The Clarendon Press, 1894. Pp. cxi + 535, 495.

The Psychic Factor : an Outline of Psychology. C. VAN NORDEN. New York, D. Appleton & Co., 1884. Pp. vii + 223.

Psychologie des grands calculateurs et jouers d'échecs. A. BINET. Paris, Hachette et Cie., 1894. Pp. viii + 364.

Versuch einer Theorie der Existentialurteile. H. CORNELIUS. Munich, Rieger, 1894. Pp. 104.

Le teorie sulla formazione naturale dell' istinto. F. MASCI. Naples, 1893. Pp. 120.

L'année philosophique. Quatrième année (1893). Paris, Alcan, 1894. Pp. 316.

La psychologie de l'amour. G. DANVILLE. Paris, Alcan, 1894. Pp. iii + 169.

The Elements of Metaphysics. P. DEUSSEN. Translated by C. M. DUFF. London and New York, Macmillan & Co., 1894. Pp. xxiv + 337.

An Illustrated Dictionary of Medicine, Biology, and Allied Sciences. G. M. GOULD. Philadelphia, Blakiston, Son & Co., 1894. Pp. xvi + 1633.

Our Notions of Number and Space. HERBERT NICHOLS. Boston, Ginn & Co., 1894. Pp. vi + 201.

NOTES.

Professor G. M. DUNCAN has been promoted to a full Professorship of Philosophy in Yale University.

Mr. GEORGE H. MEAD and Mr. JAMES R. ANGELL have been appointed assistant professors in the University of Chicago.

Mr. F. C. FRENCH of Colgate University has been appointed Professor of Philosophy at Vassar College, while President TAYLOR still retains temporarily the Professorship of Ethics.

Miss E. SEBRING, M.A. (Columbia), has been appointed Instructor in Psychology in the Teachers' College, New York.

Professor JONES of St. Andrews has been called to the chair in Philosophy at Glasgow vacated by Professor EDWARD CAIRD.

A fourth edition of the great work of VOLKMANN, *Lehrbuch der Psychologie*, is announced. It is to appear in two volumes—one of which may be expected in 1894 and also in *Hefte* at short intervals. It may now be ordered in either form (Gotha, Cöthen). The editor, Professor CORNELIUS, supplies the new matter, which will include a thorough revision of the bibliographical sections to date.

Messrs Macmillan & Co. announce the publication of a *Columbia University Biological Series* edited by Professor OSBORN. Two volumes of the series are to be issued in September: *From the Greeks to Darwin* by Professor OSBORN and *Amphioxus and the Ancestry of the Vertebrates* by Mr. ARTHUR WILLEY.

All communications for the editors of THE PSYCHOLOGICAL REVIEW, together with books, reprints, etc., intended for review, should be sent during the year beginning Oct. 1st, 1894, to Professor J. Mark Baldwin, Princeton, New Jersey.

Vol. I. No. 6. November, 1894.

The Psychological Review.

THE THEORY OF EMOTION.

(I.) EMOTIONAL ATTITUDES.

BY PROFESSOR JOHN DEWEY,

University of Chicago.

In the following pages I propose, assuming Darwin's principles as to the explanation of emotional attitudes, and the James-Lange theory of the nature of emotion, to bring these two into some organic connection with each other, indicating the modifications of statement demanded by such connection. This close dependence upon results already reached, together with the impossibility of an adequate discussion of all details in the given limits (to say nothing of the immediate availability of most of the details in every one's experience), must be my justification for the generic, and even schematic, quality of the discussion. This may be regarded either as a sketch-map of a field previously surveyed, or as a possible outline for future filling in, not as a proved and finished account.

The necessity of bringing the two theories together may be seen from the fact that the very phrase 'expression of emotion,' as well as Darwin's method of stating the matter, begs the question of the relation of emotion to organic peripheral action, in that it assumes the former as prior and the latter as secondary.

1. Now this assumption, upon the basis of the discharge theory (as I shall call the James-Lange theory), is false. If one accept the latter theory, it is incumbent upon him to find the proper method of restating Darwin's principles, since there is no doubt of their substantial significance, however erroneous

may be their underlying assumption as to the relation of emotion and peripheral disturbance.*

Professor James himself does not seem to me to have adequately realized the inconsistency of Darwin's principles, as the latter states them, with his own theory; or the needed restatement would already have been performed by a much more competent hand than my own. At least he quotes, with apparent approval, explanations from Darwin which assume the priority of an emotion of distress to the contraction of the brows; and even suggests that Darwin does not go far enough in recognizing the principle of reacting similarly to analogous feeling stimuli.† Surely if James's conception of the origin of emotion is true, the statement that we react similarly to stimuli which feel alike must be translated into the statement that activities which involve, in like fashion, the same peripheral structures feel alike.‡

2. One does not, however, need to be committed to James's theory to feel the need of a different way of stating the particular undoubted facts discovered by Darwin. Physiologists

* While Darwin's language is that of the dependence of 'expression' upon emotion, it is interesting to note that so careful an observer has, in one place, anticipated and definitely stated the discharge theory, Expression of Emotions, p. 239. (My references are to the American edition.) "Most of our emotions are so closely connected with their expression that they hardly exist if the body remains passive —the nature of the expression depending in chief part on the nature of the actions which have been habitually performed under this particular state of mind." (Note in this latter phrase the assumption of the priority of emotion; but the continuation is unambiguous in the other sense.) "A man, for instance, may know that his life is in extremest peril, and may strongly desire to save it; yet as Louis XVI. said when surrounded by a fierce mob, "Am I afraid? Feel my pulse." So a man may intensely hate another, *but until his bodily frame is affected* he cannot be said to be enraged." (Italics mine.)

† Psychology, vol. II., pp. 480–81. The exactness of the latter statement may be doubted, as Darwin recognizes the facts, but includes them under the principle of serviceable associated habits (Expression, 256), as he certainly has a right to; for Mr. James himself recognizes (p. 481, footnote) that the 'analogous feeling' principle goes back to the teleology of the movements concerned.

‡ The *facts* conveyed in this principle seem to me of themselves a strong argument for the discharge theory. Left as Darwin and Wundt state it, all mediating machinery, physiological and psychological, is absent, and we cannot even start a hypothesis as to *how* a feeling (recognizing that it feels *like* another feeling!) sets out along the same afferent paths. Upon the discharge theory the mystery vanishes and we have the practical tautology: like affections of like structures give like feeling, the interest lying in the genetic tracing of the details.

agree that there are no muscles intended primarily for purposes of expression. A psychological translation of this would be that there is no such thing (from the standpoint of the one having the experience) as expression. We call it expression when looking at it from the standpoint of an observer—whether a spectator or the person himself as scientifically reflecting upon his movements, or æsthetically enjoying them. The very word 'expression' names the facts not as they are, but in their second intention.* To an onlooker my angry movements are expressions—signs, indications; but surely not to me. To rate such movements as primarily expressive is to fall into the psychologist's fallacy: it is to confuse the standpoint of the observer and explainer with that of the fact observed. Movements *are*, as matter of fact, expressive, but they are also a great many other things. In themselves they are movements, acts, and must be treated as such if psychology is to take hold of them right end up.

3. I shall attempt to show, hereafter, that this standpoint of expression of pre-existent emotion complicates and aborts the explanation of the relevant facts in the cases of 'antithesis' and 'direct nervous discharge.' At this stage I wish to point out that in the case of 'serviceable associated habits,' the principle of explanation *actually* used, whatever the form of words employed, is that of survival, in the form of attitudes, of acts originally useful not *qua* expressing emotion, but *qua* acts—as serving life. In the discussion of movements in animals (pp. 42–48) the reference to emotion is not even nominal. It is a matter of 'satisfaction of desire' and 'relieving disagreeable sensations'—practical ends. The expressions of grief and of anxiety (Chs. VI and VII) are explained, in their detail, whatever the general phraseology employed, by reference to acts useful in themselves. It would take up too much space to follow all cases in particular, but the book is open and the reader may easily discover whether in every case the idea of

* This, of course, is in no way inconsistent with the development of certain movements to serve as expressive. On the contrary, since movements take place in a social medium, and their recognition and interpretation by others is a fact of positive import in the struggle for existence, we might expect the development of gesture and signs through selection.

expression of emotion does not enter in only to confuse. *The reference to emotion in explaining the attitude is wholly irrelevant; the attitude of emotion is explained positively by reference to useful movements.*

An examination of one apparent exception may serve to clear up the principle. Of laughter, Mr. Darwin says, "We can see in a vague manner how the utterance of sounds of some sort would naturally become associated with a pleasurable state of mind" (p. 207). But Darwin does not use this idea, even in a 'vague' way. With his inevitable candor he goes on, "But why the sounds which man utters when he is pleased have the peculiar reiterated character of laughter we do not know."

Now I am not so rash as to attempt to deal in detail with laughter and its concomitant features, but I think something at least a little less vague than Mr. Darwin's account may be given. I cannot see, even in the vaguest way, why pleasure *qua* feeling (emotion?) should express itself in uttering sounds. As matter of fact it does not, nor even in smiles;* it is pleasure of a certain qualitative excitement or vivacity which breaks out in laughter, and what we can see, in a 'vague way,' is why excitement affecting the entire organism should discharge in the vocal apparatus. The problem is the discovery of that special form of excited action which differentiates the laugh from other excitations. Observe a crowd of amateurs just from a game. Note how, irrespective of what they say, you can judge whether they have won or lost. In one case postures are erect, lungs frequently expanded, movements quick, abrupt, and determined; there is much gesturing, talking, and laughing in high keys,—a scene which, looking at it 'ejectively,' we term one of liveliness, exhilaration, etc. In the other case there is little speaking, and that subdued; all movements tend to be slow, or, if rapid, indicate a desire to escape or expel something; meditative postures are frequently observed, etc.,—a scene of depression. It is the contrast between spontaneous overflow and lowering of overt activity.

* The 'pleasures' of eating have their characteristic attitude—smacking lips, rolling tongue; the pleasures of sex theirs, etc. Many pleasures are accompanied by holding the breath to maintain the excitation at its maximum, not at all by the expiration found in laughter.

What is the difference? In either case the energy, muscular, nervous and visceral, aroused in the game, persists to some extent. What determines the antithetical lines of discharge of this surplus energy (that antithesis of 'dejection' and 'elation' running through all our terms)? In one case, I answer, there are frictionless lines of action, harmonized activity; or, in more psychological language, all existing kinæsthetic images reinforce and expand one another; in the other case there are two more or less opposed lines of activity going on —the images of the present situation and those of the past game cannot be co-ordinated. The energy is largely directed 'inwards'; that is, it is used up in rethinking the game, in making hypothetical changes, in recalling blunders (that is, images which one wants to expel), etc. The movements appropriate to the present activity cannot be identified with the nervous and motor energies which image the game. In the case of exhilaration, etc., there is identification of the thoughts (the nerve and muscular activities relative to the past game) and the present motor discharges.

The connection between *il penseroso* and melancholy more or less mild, and between *l'allegro* and joy, is thus organic and literal, not one of chance or analogy—as if analogy were somehow a force! When one can put up with his defeat, it ceases to bother him, he does not consider it longer. That is, the 'downcast' emotion and the intellectual reflection vanish together—the moment there is identification of images. The essential identity of the attitudes of thought and of regret is because of the condition of divided activity; there is still a struggle. Means and end are apart. The identity of attitudes of joy and of activity, of life (alert, wide awake, brisk, animated, vivacious, cheerful, gay—showy, lively, sprightly) is because of the unification of activity. Meditation and regret are both activities of arrest, of conflict; joy and 'lively' movement, of stimulation—expansion. No wonder, then, they have the same signs.

Thinking, to be sure, in certain professions, though not for the ordinary man, is an end in itself. In so far as thinking is an end in itself, the activity is unified and has its own joys. It ceases to be occupied with merely instrumental, and (therefore) more or less burdensome, movements. Yet the pangs, the travail of thought, the arduousness of reflection, the loneliness of meditation, the heaviness of deliberation, are all proverbial. Only in rare cases is the whole system involved or unified, and the joy voluminous. Its ordinary form is the 'thrill' of identification or the satisfaction of 'good taste' in a clear, neat discrimination. When a long and comprehensive process is concluding and approaching its final successful or unified discharge, then, indeed, the hand of a Newton may tremble and joy become intoxicating. But I cannot admit, even in a half-hearted way, the idea that the sense of abundance and ease in thought (James, II. 477) may be purely cerebral.* It appears to me that it is in a literal sense that the object 'sets trains going'—these are revivals of motor discharge and organic reinforcement. Upon such occasions thinking becomes really whole-hearted; it takes possession of us altogether, and passes over into the æsthetic.

This, however, is only preparatory to the question of the specific 'sign' of joy, the laugh. How is that to be brought under this principle of being an actual portion of a useful activity? Why should the excitation, admitting that it affects the vocal organs, manifest itself in this form? While I feel pretty sure of the following explanation, I cannot hope that it will convince many. Though the result of considerable observation, it can be briefly summed up. The laugh is by no means to be viewed from the standpoint of humor; its connection with humor is secondary. It marks the ending (that is, the attainment of a unity) of a period of suspense, or expectation, an ending which is sharp and sudden. Rhythmical activities, as peek-a-boo, call out a laugh at every culmination of the transition, in an infant. A child of from one and a half to two years uses the laugh as a sign of assent; it is his emphatic 'I do' or 'yes' to any suggested idea to which he agrees or which suddenly meets his expectations.

A very moderate degree of observation of adults will con-

* Such distinctions as James makes here—in reality purely verbally—between spiritual and physiological, instead of between cerebral and viscero-motor, are what give the opponent the sole reason for labelling the theory materialistic—as if the bowels were really more material than the brain!

vince one that a large amount of laughter is wholly irrelevant to any joke or witticism whatever. It is a constant and repeated 'sign' of attaining suddenly to a point. Now all expectancy, waiting, suspended effort, etc., is accompanied, for obvious teleological reasons, with taking in and holding a full breath, and the maintenance of the whole muscular system in a state of considerable tension. It is a divided activity, part of the kinæsthetic images being fixed upon the immediately present conditions, part upon the expected end. Now let the end suddenly 'break,' 'dawn,' let one see the 'point' and this energy discharges—the getting the point is the unity, the discharge. This sudden relaxation of strain, so far as occurring through the medium of the breathing and vocal apparatus, is laughter. Its rhythmical character seems to be simply a phase of the general teleological principle that all well-arranged or economical action is rhythmical.* The laugh is thus a phenomenon of the same general kind as the sigh of relief. The difference is that the latter occurs when the interest is in the *process*, and when the idea of labor, slow and continuous, is at its height; while the laugh occurs when the interest is all in the outcome, the result—the sudden, abrupt appearance of the 'point.' In one case the effort is continued until it accomplishes something; in the other case the effort is arrested, and then the energy accumulated is set free from a seemingly outside source. The connection of humor with the laugh, and the ideas of relative superiority—triviality, and of incongruity, involved in humor, etc., seem to be simply more complex, and more intellectually loaded, differentiations of this general principle.

Not only are joy and grief practically in a peculiar qualitative antithesis, seeming to imply a common principle of which they are the extremes, but the 'signs' of joy and grief, especially when these become violent, are identical. This fact, otherwise so meaningless, becomes natural if we adopt the above explanation. Both crying and laughing fall under the same principle of action—the termination of a period of effort. If we fix our attention upon the conventional and

* Acute crying, etc., is non-rhythmical; when it does take the form of rhythmical sobbing, one experiences a sensation of relief—grief has 'moderated.'

literary conceptions of grief, this will seem far-fetched; if we take children and simple cases, it seems to stare us in the eyes. Crying is either a part of an effort to expel an intruder,* an effort so general as to engage spasmodically the lungs and vocal organs (a sort of general gripe); or, as we see so often in children, an explosion of energy, accumulated in preparation for some act, suddenly discharged *in vacuo* upon the missing of the essential part, the finishing factor of the act.†

Beginning with the simpler case, the phenomena of matured grief become easily explainable. They are phenomena of *loss*. Reactions surge forth to some stimulus, or phase of a situation; the object appropriate to most of these, the factor necessary to co-ordinate all the rising discharges, is gone; and hence they interfere with one another—the expectation, or kinæsthetic image, is thrown back upon itself.

4. In dealing with grief we have unconsciously entered upon a new field. The point of our third head is that the principle which Darwin calls that of 'movements useful in expressing an emotion' explains the relevant facts only when changed to read 'useful as parts of an act which is useful as movement.' In dealing with grief we have passed over into the phenomena of the breakdown of a given teleological co-ordination, and the performance of acts which, therefore, objectively viewed, are not only useless but may be harmful. My proposition at this point is that the phenomena referred to the principle of direct nervous discharge (the response to an idiopathic stimulus) are cases of the failure of habitual teleological machinery, through some disturbance in one or more of the adjusted members of the habit.

In order to avoid misconception, let me point out a great

* While Darwin's explanation of shutting the eyes—to protect blood-vessels from gorging on account of the violent screaming—undoubtedly accounts for the selection of this attitude, it can hardly account for its origin. I think originally it had the same end as screaming—to shut out or off some threatening object, as the ostrich, etc., or as one shuts his eyes on firing a gun the first time.

† I suppose every one has seen a young child go into a rage of screams and violent movements upon being handed, say, a broken cooky. The thing explains itself on the above principle. The concluding factor in a co-ordination of energy does not appear, and the child goes literally to pieces. I should like to see any explanation upon the anti-James theory, save that offered by Saint Augustine for similar phenomena of his infancy—total depravity.

ambiguity in the use of the term idiopathic. In one sense even the 'associated useful' movements are idiopathic, provided, that is, they originally were useful in reaching an end, and not simply in expressing an emotion. They are the reactions to their appropriate stimuli, and the sole difference between them and the liver changes, nausea, palpitation of heart, etc., usually classed as idiopathic, is that in them stimuli and reaction are more definitely limited to certain particular channels than in the latter cases; there is a defined, as against a vague and diffuse, direct nervous discharge. The fact that this defined discharge happens to be useful may state the kind of idiopathic response we have, but cannot make it other than a response. Furthermore, upon evolutionary principles, the limited, adjusted, and useful discharge must be a differentiation, selected and perpetuated because of its utility in the struggle for life, out of an original more diffuse and irradiating wave of discharge.

Admitting, then, that all emotional attitudes whatever are idiopathic in the broad sense, the sole difference being in the definiteness or limitation of the stimulus and its response, what are we to do with the cases now disposed of as 'idiopathic' in the narrower sense?—such phenomena as Mr. James briefly but excellently sums up on p. 482. My proposition, I repeat, is that all such idiopathic discharges, possessing emotional quality, are in reality disturbances, defects, or alienations of the *adjusted* movements. While not immediately teleological in the sense that they themselves are useful, they are teleologically conditioned. They are cases of the disintegration of associations (co-ordinations) which are serviceable, or are the use of means under circumstances in which they are totally inappropriate.

Idiopathic discharges which are not themselves adjusted movements or the disturbances of such adjusted movements do not appear to me to have any *emotional* quality at all. The trembling with cold or sheer fatigue is certainly qualitatively different from the tremble of rage or fear. The sensations of weakness in the bowels and of nausea, which are idiopathic to their appropriate stimuli, can be called emotional only by such a stretch of the term as renders all sensations and impulses emotions, Professor James seems to me wholly successful in dealing with the charge brought that, upon his theory, all laughing ought to give the mirthful emotion, all vomiting that of disgust, etc.* The diffusive wave in one case is incomplete; but is there no reason or meaning in this difference? There is no doubt, in my own mind, that, *under existing conditions*, the supplying of the missing organic excitations will change the laugh and the nausea into mirth and disgust as emotions—this without any change in the 'object.' But whence and why these 'existing conditions'? The change from mere cachinnation to mirthful emotion is a distinct change in psychical quality, and this change of quality does not seem to be adequately *accounted* for by mere addition of more discharges—though, I repeat, simply adding on more discharges will undoubtedly *make* this difference. If these supplementary factors report the meaning or value of past co-ordinations, this change of quality is reasonable and inevitable; if not, if they are simply some more accidental discharges, the peculiar qualitative 'feel' is miraculous—it admits of no explanation.

This is but to say, from the psychological side, that all normal emotion of terror has an *object*, and involves an attitude *towards* that object; this attitude, under the given circumstances, perhaps not being useful, nay, being harmful, but yet the reproduction of an attitude or, rather, a mixture of attitudes which have been useful in the past. The uselessness of the attitude is due to the fact that some feature in the stimulus (the situation or object) awakens its appropriate reactions, but these do not co-ordinate with the reactions aroused by other features of the situation. The pathological emotion is, as Mr. James calls it, the *objectless* emotion, but its content is controlled by the active attitudes previously assumed towards objects, and, *from its own standpoint*, it is not objectless; it goes on at once to supply itself with an object, with a rational excuse for being.† This immediate correlation of the emotion with an 'object,' and its immediate tendency to assume the 'object'

* Psychological Review, No. 5, p. 522.

† The pathological emotion is to the normal as hallucination is to perception. An unusual stimulus takes advantage of and controls the lines of co-ordination and discharge which have been built up with reference to the usual or normal stimulus. Psychologically the process is quite regular; it is only in its teleology that it is 'off.'

when it is not there, seem to me mere tautology for saying that the emotional attitude is normally rational in content (i. e., adjusted to some end), and, even in pathological cases, sufficiently teleological in form to subsume an object for itself.

In any case, upon James's theory, the admission of any idiopathic cases which cannot be reduced to abnormal use of teleological adjustments is more or less intolerable. Their permanent resistance to such reduction would be a strong objection to the theory. Hope, fear, delight, sorrow, terror, love, are too important and too relevant in our lives to be in the main* the 'feel' of bodily attitudes which have themselves no meaning. If the attitude is wholly accidental, then the emotion itself is brute and insignificant, upon a theory which holds that the emotion is the 'feel' of such an attitude.

One more word of general explanation. The antithesis here is between the merely accidental and the adjusted excitation—not between the mechanical and the teleological. I add this because of the following sentence in James: "It seems as if even the changes of blood-pressure and heart-beat during emotional excitement might, instead of being teleologically determined, prove to be purely mechanical or physiological outpourings through the easiest drainage-channels" (II. p. 482). Certainly, if these are the alternatives, I should go a step farther and say that even the clenching of the fist and the retraction of the lips in anger are simply mechanical outpourings through the easiest available channel. But these are not the alternatives. The real question is simply how this particular channel came to be the easiest possible, whether purely accidentally or because of the performance of movements having some value for life preservation. The ground taken here is that the easiest path is determined by habits which, upon the whole, were evolved as useful.†

*In the main, I say; for doubtless it is pedantry to hold that every slight feature of the attitude is conditioned by an activity directed towards an object.

† It is admitted, of course, as Mr. James puts it, that there are "reactions incidental to others evolved for utility's sake, but which would never have been evolved independently" (p. 484). Indeed, in one sense of the term 'incidental' this is a necessary part of my proposition. The only question is whether 'incidental' means purposeless, or means having their purpose not in themselves, but as relative to, as facilitating or reinforcing, some other useful act. The fact, once more,

Coming a little more to details, it is obvious that the teleological principle carries within itself a certain limitation. Normal and usual are identical; the habit is based upon the customary features of the situation. The very meaning of habit is limitation to a certain average range of fluctuation. Now if an entirely strange (forgive the contradiction in terms) stimulus occurs, there will be no disturbance of function, though the organism may be destroyed by the impact of the foreign force. But let some of the features of a situation habitually associated in the past with other features be present while these others fail, or let the ordinary proportion or relative strength of stimuli be changed, or let their mode of connection be reversed, and there is bound to be a disturbance and a resulting activity which, *objectively viewed*, is non-teleological. We thus get an *a priori* canon, as it were, for determining when, in a given emotion, we shall get symptoms falling under the 'serviceable associated habit' principle and when under the idiopathic. Whenever the various factors of the act, muscular movement, nutritive, respiratory, and circulatory changes, are co-ordinated and reinforce each other, it is the former; whenever they interfere (the 'idiopathic'), the 'feel' of this interference *is* (applying the general principle of James) the pathological rage, or terror, or expectation.

Once more, we work in a wrong, a hopeless direction when we start from the emotion and attempt to derive the movements as its expression; while the situation clears itself up when we start from the character of the movement, as a completed or disturbed co-ordination, and then derive the corresponding types of normal and pathological emotion. We can understand why the so-called idiopathic principle comes into play in all cases of extreme emotion, the maximum limit seeming to be the passage into spasm when it assumes a rigid type, of hysteria when it involves complete breakdown of co-ordination.

The attitude of normal fear may be accounted for upon direct teleological principles; the holding of breath marks

that upon Darwin's method of statement no such relative or incidental movements can be admitted is an undoubted objection to Darwin's mode of statement of the principle of useful habit.

the effort; the opening of mouth, the act arrested half-way; the opening of eyes, the strained attention; the shiver, of retraction; the crouching down, the beginning of escape; the rapid beating of heart, the working up of energy for escape, etc. Now if these activities go on to complete themselves, if, that is, they suggest the further reaction which will co-ordinate into a definite response, we get judicious fear—that is, caution. Now if these do not suggest a further movement which completes the act, some or all of these factors begin to assert themselves in consciousness, isolatedly or in alternation—there is confusion. Moreover, each particular phase of the act which is normal in co-ordination, as the more rapid beating of the heart, being now uncontrolled by lack of its relevant motor associates, is exaggerated and becomes more and more violent. The response to the normal demand for more nutrition finds no regular outlet in supplying the motor-energy for the useful act, and the disturbances of viscera and associated organs propagate themselves. The trembling marks, so far as I can see, simply this same disco-ordination on the side of the muscular system. It is the extreme of vacillating indecision; we start to do this, that, and the other thing, but each act falls athwart its predecessor.

Speaking roughly, there is exaggeration of the entire vegetative functions of the activity, and defect of the motor side—the unstriped muscles being included, on a functional basis, with the vegetative system. Now this is just what we might expect when there is a great stirring up of energy preparatory to activity, but no defined channel of discharge. Thus the agent becomes entirely taken up with its own state and is unable to attend to the object.

The pathological emotion is, then, simply a case of morbid self-consciousness. Those factors of the organism which relate most immediately to the welfare of the organism, the vegetative functions, absorb consciousness, instead of being, as they normally are, subsidiary to the direction of muscular activity with reference to the 'object.' This is equally true in extreme terror, and in being 'beside one's self' with anger. The cases in which sanguine excitement and apprehension affect the bladder will be found, I believe, to be almost uniformly cases where it is not possible to do anything at once with the aroused activities; they cannot be controlled by being directed towards the putting forth of effort upon the 'object,' that being too remote or uncertain.

Certainly, the principle for attitudes commonly called those of morbid self-consciousness is precisely the one just laid down. In these cases muscular (not vegetative) functions normally useful in the attainment of an end are first aroused in response to stimuli, and then, not being completely co-ordinated into action, are *not* used with reference to the end, and so stand out in consciousness on their own account. I shall not attempt any detailed statement here, but leave it to the reader to answer if the above does not give a precise generic description of the sensations of awkwardness, of bashfulness, of being ridiculous (as when one starts an appropriate movement, but is made conscious of it in itself apart from its end) on one side, and of affected grace, mincing ease, pomposity and conceit on the other.

All these facts taken cumulatively seem to me to render it fairly certain that the 'idiopathic' cases, as a rule, are to be conceived of as the starting of activities formerly useful for a given end, but which now, for some reason, fail to function, and therefore stand out in consciousness apart from the needed end.

5. I come now to the principle of antithesis. According to Mr. Darwin, when certain movements have been habitually of service in connection with certain emotions, there is a tendency, when a directly opposite state of mind is induced, to the performance of movements of a directly opposite nature, '*though these have never been of any use*' (p. 50, italics mine). Here we have a crucial case; if the antithesis of the emotion determines the antithesis of expression, James's theory is, in so far, overthrown; if, on the other hand, the antithesis of 'expression' goes back to activities having their own ends, the ground is at least cleared for the discharge theory.

Beginning with animals, Mr. Darwin illustrates his principle of antithesis from the cat and dog. No one can read his account or examine the pictures without being convinced that the movements *are* antithetical. But there is something intolerable to the psychologist in the supposition that an opposite emotion can somehow select for itself channels of discharge not already used for some specific end, and those channels such as give rise to directly opposed movements. Antithesis is made a causal force. Such an idea is not conceivable without some presiding genius who opens valves and pulls strings. The absence of mediating machinery, of interlinking phenomena, is even more striking in this case than in that of 'analogous feeling.'

If, again, the matter be treated as a case of the connection of movements with reference to certain acts, the mystery vanishes. Mr. Darwin's cases are taken from domestic animals. Now wild animals have, speaking roughly, just two fundamental characteristic attitudes—those connected with getting food, including attack upon enemies, and those of defence, including flight, etc. A domestic animal, by the very fact that it is domestic, has another characteristic attitude, that of reception—the attitude of complete adaptation to something outside itself. This attitude is constituted, of course, by a certain co-ordination of movements; and these are antithetical to those movements involved in the contrary attitude, that of resistance or opposition. A study of the dogs upon pp. 52–55 will show that the attitude of opposition is naturally self-centred and braced, the best position from which to fall, on one side, into an attitude of overt attack, and, upon the other side, into that of resistance to attack. The attitude of 'humility' and 'affection' consists, as Mr. Darwin well says, in continuous, flexuous movements. These movements are precisely those of response and adaptation. The centre of gravity is, as it were, in the master, and the lithe and sinuous movement is the solution of the problem of maintaining balance with respect to every change in this external centre of gravity. It is the attitude of receiving favor and food from another. The dependence is actual, not symbolic. Unless Mr. Darwin were prepared to equip the animal with a full-fledged moral consciousness, the 'humble' attitude of the dog can hardly be other than the habitual attitude of reception, or the 'affectionate attitude other than the recurrence of movements associated with the food-getting. The same general principle will apply to the antithetical cat expressions, save that the dependence in the case of the cat assumes more the form of passive contact and less that of active adjustment. The reminiscence of sexual attitude is possibly also more marked.*

The other cases of antithesis given by Mr. Darwin are the shrug of impotence, and the raising of the hands in great astonishment. I feel certain that the rational hypothesis is to suppose that these are survivals of certain acts, and not symbolic indications of certain emotions. As a contribution to such a working hypothesis, I suggest the possibility that the throwing up of the arms in attention is partly the survival of a movement of warding off the approaching hostile object, and partly a reinforcement of the holding of the chest full of air characteristic of expectancy and of astonishment—a movement whose analogue is found in the raising and drawing back of the arms in yawning.† The shrug of impotence seems to be complex; the union of survivals of three or four distinct acts. The raising of the brows is the act of retrospect, of surveying the ground to see if anything else could have been done; the pursing of the lips, the element of tentative rejection (doubt); the raising of the shoulders, the act of throwing a burden off (cf. 'he shouldered it off on some one else'); the holding out of the hand, palm up, the attitude of asking or taking. To my introspection the *quale* of the emotion agrees entirely; it is a feeling of 'I don't see how I could possibly have done anything else, so far as I am concerned, but I'm willing to hear what you have to offer'—of 'I don't know; you tell.' It thus has the distinctly expressive or social element in it, and marks the passing over of emotional attitude into gesture.

Summing up, we may say that all so-called expressions of

* Being unable to do anything with these cases, I called them to the notice of my friend and colleague, Mr. G. H. Mead. The explanation given, which seems to me indubitable, is his. The relation between the vegetative and the motor functions, given above in discussion of pathological emotion and to be used again below, I also owe to him. While I have employed the point only incidentally, Mr. Mead rightly makes it essential to the explanation of emotion and its attitudes, as distinct from the identification and description which alone I have attempted. I hope, therefore, that his whole theory may soon appear in print.

† Since writing the text, I have repeatedly noticed this attitude of the arms, without the rigidity, assumed by a child of two years while watching the preparation of his food.

emotions are, in reality, the reduction of movements and stimulations originally useful into attitudes. But we note a difference in the form and nature of the reduction, and in the resulting attitudes, which explain the apparent diversity of the four principles of 'serviceable associated habits,' of 'analogous stimuli,' of 'antithesis,' and of 'direct nervous discharge.' A given movement or set of movements may be useful either as preparatory to, as leading up to, another set of acts, or in themselves as accomplished ends. Movements of effort, of bracing, of reaching, etc., evidently come under the former head. Here we have the case of useful associated movements in its strict sense. The culmination of all these preparatory adjustments is the attainment of food or of the sexual embrace. In so far as we have attitudes which reflect these acts, satisfying in themselves, we get cases of so-called analogous stimuli. The antithetical attitudes of joy and grief, and all that is differentiated from them, mark the further development of actual attainment of an end, (or failure to get it) occurring when the activity specially appropriate to the particular end reached (or missed) is reinforced and expanded by a wide range of contributory muscular and visceral changes. The cases of failure bring us to the breakdown of co-ordinations habitually useful, to their alienation, or to reciprocal disturbance of their various factors, and thus to the facts usually subsumed under the idiopathic principle. In this progression we have a continually changing ratio of the vegetative to the motor functions. In the preparatory adjustments the latter has the highest exponent, and the strictly emotional *quale* of feeling is at its minimum. In joy and grief, as in less degree with 'sweetness,' disgust, etc., the organic resonance is at its height, but strictly subservient to the motor performances. In the idiopathic these vegetative functions break loose and run away, and thus, instead of reinforcing the efficiency of behavior, interfere by their absorption of consciousness.

In the following article I shall take up the discharge theory of the nature of emotion, and discuss it in the light of the conclusions now reached.

THE STUDY OF A CASE OF AMNESIA OR 'DOUBLE CONSCIOUSNESS.'

BY PROFESSOR CHARLES L. DANA, M.D.,

Bellevue Hospital Medical College.

The following case of altered personality seems to me to have a special psychological interest, for the reason that it is so classical a type, and also because there are in it no disturbing elements of hysteria, epilepsy, or organic disease.

In 1884 I collected and published all the recorded cases of 'double consciousness' or 'periodical amnesia' then accessible (Wood's Reference Handbook, art. 'Disorders of Consciousness.') They numbered at that time sixteen. Since then several new cases have been reported by Guinon, Janet, James, Weir Mitchell, Baker, and others. The subject has been discussed at length by Ribot (Diseases of Memory) and by Prof. Wm. James (Psychology, vol. II.), also recently by Alfred Binet (Internat. Scient. Series, 1892), and Pierre Janet (Rev. gén. des Sc., Mar. 1893). Most of the reported cases are evidently illustrations of epilepsy, hysteria, or a peculiar form of melancholia. In epileptic cases the amnesia is associated with automatism, and the patient's mental powers are sharply limited and lessened. The same is true, so far as my experience goes, in hysteria, the altered personality being only an illustration of an automatic or trance condition. Huxley's traumatic case is one of this kind, and they are not very rare in neurological experience. I must be permitted also to express grave doubts as to the value of psychological studies made upon the trained hypnotic patients of Salpêtrière. A personal knowledge of some cases of this class and a perusal of the investigations of Mr. Ernest Hart upon the subject justifies this view and a distrust of the multiple personalities of trained Parisian subjects.

I do not wish to disparage the reports of others, but I am sure a candid student of the cases of alleged double personality will find that what may be called pure cases are few. Such are those of Dr. Mitchell, Dr. Donar (Trans. Roy. Soc. Edin., 1822), Dr. J. N. MacCormack (*Medical Record*, May 26, 1883, p. 570), of M. Azam, and the present one.

History of the Case.—The patient, Mr. S., age 24, was an active, intelligent, and healthy young man. Though coming of a somewhat nervous stock, there is no actual psychosis in the family. He had himself always been well. His habits were good. For a year or two before his trouble came on he had been subjected to some nervous strain, but it had not perceptibly affected his health or spirits. About two weeks before his accident he had some financial trouble, and on coming home had a 'nervous chill.' However, he seemed perfectly well next day and continued his usual duties. On Friday evening, November 18th, he retired as usual. Next morning, as he did not appear at breakfast, a member of the family entered his room and found it full of gas, and the patient lying unconscious in bed. The escaping gas was due to a leak in the pipe, as was subsequently found. The stopcock of the gas-burner was turned off, and there was no possible reason for or suspicion of suicide. The patient was as stated unconscious, the face livid, the lips blue, the eyes open, the respirations slow and stertorous, sometimes almost ceasing. The family physician, Dr. Rodenstein, was called, and worked over him for three hours before the breathing became natural and his life seemed out of danger. He became partially conscious by 4 P.M., and to a clergyman who had called he talked rationally but not clearly. Next morning he recognized his sister and father, and said he thought he was losing his mind. In the afternoon he became somewhat delirious. He slept that night, but during the succeeding six days his mind wandered and he was apparently distressed and excited. He was oppressed with the idea that some one wanted to take him away and do him a bodily injury. He talked about a trip he had been expecting to make to Washington, and called for his time-tables. He spoke also about his business and of various plans he had been intending to carry out. On Tuesday, four days after the accident, he was seen trying to read a newspaper upside down. On the eighth day he was taken to Dr. Granger's sanitarium. He went without trouble, though he was still somewhat excited and maniacal. That night he slept and next morning awoke free from any signs of mania. He was quiet and sane in every way. From this time the evidences of his amnesia and changed personality were apparent. He dressed himself neatly and with his usual attention to his toilet, understanding apparently the use of the various articles of dress. He showed by his conversation at once that he did not know who he was or where he was, and that his conscious memory of everything connected with his past life was gone. His vocabulary at first was very limited; he could only use familiar words, and could only understand language of the simplest character, such as that bearing on the things immediately about him. He did not know the names or uses of the things in and about the house, though he at once remembered and never forgot any name told him. Consequently his vocabulary and understanding of conversation rapidly increased. He had a German attendant, and pronounced many of the new words with a German accent. Everything had to be explained to him, such as the qualities and uses of the horse and cow and of the various articles about the house. Yet he would sit at the table and eat his meals with his former neatness, preserving also the courtesies and amenities of a gentleman, but he could not understand why he did certain things until it was explained. He did not recognize his parents or sisters, or *fiancée* though he said that he had always known the latter, and his great desire and longing was to have her with him. He did not remember the slightest detail of his former relations with her and did not know what marriage meant or the significance of the filial relation. Those persons whom he had liked very much before he seemed especially glad to see, though he could not explain why.

He could not read, and did not even know his letters or figures. But he soon learned both to read and write simple sentences involving ordinary words.

His vocabulary was gradually increased, but even two months after his accident he could not read a newspaper understandingly, except simple accounts of every-day happenings. He was naturally slowest in understanding abstract terms. He learned figures and arithmetic very quickly and could soon do ordinary arithmetical computations easily. He had been accustomed to play billiards a little, but played the game badly. He very soon learned to play again, appreciating the value of angles, and before long he became much more skilful than he had been in his former state. He had always been clumsy with his hands and never liked mechanical work or showed the least capacity for it; he never could draw or carve. With a little instruction from another patient he soon became very skilful in carving and worked a monogram in the back of a brush in a most creditable manner. He also made a shuffleboard, doing the work very neatly. He showed in fine a much greater cleverness with the hands and finer development of muscle-sense than he had had before.

He used to play and sing a little. About six weeks after the accident he picked out a tune on the piano which he had known long before, but had not heard or played for a year. He did not know what it was, or associate it with any early memory. He sang some of his old songs and played a little on his banjo. The old musical memories were there, but dissociated from any thoughts of the past. He was very imitative and his memory for everything told him was extraordinarily retentive. He had always been careful and even fastidious about his person, and he continued to be so. His habits of courtesy and affability continued the same.

He had had some religious upheavals in the past. Two or three years before, he was distinctly and positively atheistical; later he was more inclined to theism and agnosticism. In an argument which I undertook with him to test his logical powers and knowledge of abstract ideas he showed a distinctly atheistical state of mind. His views were those held some years previously, not his later ones. In argument he showed considerable dialectic skill and logical power. But he evidently could not understand any conceptions at all abstract. His 'mind-stuff' was made up of conceptions closely related to his recently acquired practical knowledge. He had previously acquired a special repugnance to any form of religion, and he showed this feeling of antagonism in his conversation.

He was even-tempered and obliging. He had never been demonstrably affectionate and was not in his new state, except as regards his *fiancée*, about whom his thoughts and feelings were intensely centred.

If one were to meet him, and discuss ordinary topics, he would show no evidence of being other than a normal man, except that he might betray some ignorance of the nature or uses of certain things. His conversation ran chiefly on the things he did every day and on the new things he every day learned. He was exactly like a person with an active brain set down into a new world, with everything to learn. The moon, the stars, the animals, his friends, all were mysteries which he impatiently hastened to solve. He was somewhat sensitive to his condition and did not like to meet persons whom he had known before. He cherished also a lurking suspicion that some one in some way might want to take his *fiancée* away from him. But he never was in a passion, never became incoherent or delirious, had no delusion or hallucination, and was not in the slightest degree demented.

He spoke of his own mental condition, and seemed to understand that it was not right. He was very anxious to get well.

Physically his health was perfectly good. He had no anæsthesia of the skin, no limitation of visual or aural fields, no stigmata of a trance or hysterical state. He slept well, and so far as I know had no dreams. He had a tendency to coldness and redness of the extremities, and there was evidently lack of vaso-motor tone. At times when a little excited he would move his head constantly from side to side as if working in an uncomfortable collar. This was a violent exaggeration of a habit I observed that he had when in his normal condition.

On three occasions I hypnotized him, using the methods of Braid and Bernheim combined. On the second and third trial I put him in a light degree of hypnotic sleep. During this I told him that after waking at a certain signal he would go through certain acts, such as rubbing his eyes, walking about the table, opening the door and giving a certain greeting to his mother. Also that at a certain hour in the evening he would remember the past. He did everything that I suggested except the last. At the time named in the evening, he simply said without suggestion, "Dr. Dana told me to remember something, but I can't do it."

I saw him once or twice a week at my office. He continued in much the same state day after day. His knowledge increased so that he was able to go about alone to a considerable extent, and I had begun to advise his going to his old place of business and learning something of his old work.

At the suggestion of Prof. Josiah Royce, to whom I gave some account of the case, I told him to get some of his old love-letters and copy them, also to copy some of the prayers that he used to say daily as a boy, and finally to get some of his old business accounts and copy them off; I was in hopes that some of these things might revive old memories by appealing to his affections, his religion, and his business instincts. He did this, but with no apparent success.

On February 15th, Friday evening, exactly three months from the time of his attack, he went to see his *fiancée*. She thought, after the interview, that he was rather worse, less like himself. She cried that night when he left, thinking he would never get well. While riding home with his brother he said he felt as though one half of his head was prickling and numb, then the whole head, then he felt sleepy and was very quiet, but did not fall asleep. When he got home he became drowsy and was carried to bed, where he fell asleep. At about 11 o'clock he woke up and found his memory restored. He remembered distinctly the events of three months ago: his visit to his *fiancée*, his supper at the club afterwards, his journey home, his shutting his bedroom door and getting into bed. His memory stopped there. He did not recall a thing that had occurred between times.

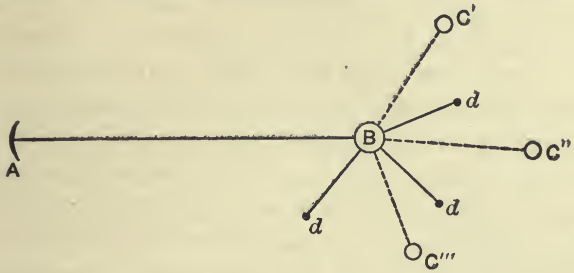
He knew all his family at once and was plainly just the same man as before. But the three months was an entire blank to him. Next day he came to see me, but did not know me (I had never seen him before his accident). Not a thing connected with the three months could be recalled. It was so much taken entirely out of his existence. He at once resumed his old work and habits, and has continued perfectly well up to the present time.

Remarks.—I do not pretend to psychological expertness, and my training has been such that I am obliged to formulate my psychological conceptions in physiological terms. A simple visual sensation is always represented to my mind by a nervous impulse travelling from the retina to a certain part of the occipital cortex. If this sensation is complex and arises to the dignity of the perception of, say, a man, a mass of association-fibres send out their impulses, awakening previously-stored images of man. If it is an old friend, the cortical areas for a still larger space are sent tingling into activity, and memories of old days rush into consciousness, intensifying it, awakening pleasurable feeling and suggesting new or reviving old ideas.

Now it was just this power of bringing into activity these longer association-fibres which was lost in the case of S. To him at first a man was a simple visual sensation, perhaps a little more, but not very much. Those associative tracts which formerly brought quickly into consciousness his previous visual, auditory, and tactile memories of man were paralyzed.

Yet certain shorter fibres were not altogether incapable of activity, and helped by subconscious memories they soon worked quickly and helpfully, enabling him to form again a fresh conception of the creature man. This idea of 'man,' however, was one almost entirely new and narrow. The idea of a father or teacher or old friend he did not in the least understand. The old and more complex memories lay quiescent in the mind; they could not be reached by any strands of connecting nerves that had been formerly so active. Certain memory-feelings, however, of affection, love, friendship, were associated with the new conception. He was glad to see and at once enjoyed the presence of an old friend, whom he had been intimate with, but about whom he could recall nothing. A previous special affection for mother, sisters, and others showed itself in a desire to be with them and enjoyment of their society.

I can only conclude that in this condition there was a paralysis or suspended function of those longer especially-trained associative tracts leading to past memories. I can perhaps make my meaning clearer by a very crude diagram.



A = eye. *B* = cortical centre for vision.

C′C″C‴ = old memories acquired in past experiences.

ddd = new memories in active association with *B* and acquired since accident.

BC′BC″BC‴ = special association-fibres connecting visual centre with memory areas.

Can neuro-pathology throw any more light on this process? There does occur at times a destruction of certain parts of the brain cortex as a result of which all memories of a certain class are abolished, leaving the intellect otherwise clear. A patient of mine after a slight right-sided stroke lost entirely the power of reading. She did not know even the letters that she saw. She could write fluently and coherently, but could not read a word of what she had written. In her case there was a softening of the left angular gyrus extending into the white matter and involving the association-tracts. Yet her word-memories of the past were not destroyed because by making her trace the letters with a pencil she could slowly read through her muscular sense; and on reading a sentence its meaning was clear to her. But she could never be taught to read with her eyes. Now as far as a loss of word-memory is concerned she was exactly like my patient who at first held his newspaper upside down. In her case the sense-centre *B* was partly destroyed, and the association-fibres with it, while the remoter areas which had previously been stored up by her reading were intact. In Mr. S. the centre *B* was present and soon excited to activity, but the remoter memory-areas could not be brought into activity by any form of sensory stimulation or excitation of newly-acquired or subconscious memories.

Besides, in Mr. S. what was true for vision was equally true for auditory memories. He did not at first understand any but the simplest language, nor appreciate the significance of sounds or gestures. Suppose I represent these various centres by *B*, *C*, *D*, *E*. Then if around each one I were to draw a dark circle it would represent the inactive association-tracts, which kept his centres when stimulated from arousing old memories. A further dead line would have to be assumed to exist for his muscular memories, his affections, and his automatic and subconscious activities. This necessitates, perhaps, a clumsy mechanical conception, but so far it appears to me to be absolutely correct from a physiological point of view.

Suddenly all these dead walls fall away, the associations are renewed, old memories and the normal personality are restored. It must be remembered that the dead walls are not absolutely solid, but that association-fibres, never before much used, pass through them, and that by their means new memories were quickly established, connections with older subconscious ones were in a measure made, and complex mental processes were possible.

Such a conception as I present excludes as impossible any theory of a dual brain or of one cerebrum being put to sleep except on the hypothesis that all memories are, when solidly established, located in the one cerebrum. But the facts of pathology disprove this.

I must assume, therefore, that the association-tracts which ordinarily connect sensory areas with long-stored-up memories are only put in action by a specialized and highly-differentiated power on the part of nerve-cells. And this is certainly the case. The simplest function of a receptive cell when excited is to awaken a simple sensation, next to arouse some other simple sensation-memories, so that a person sees and knows an orange. But to recall past experiences, gustatory, personal, etc., in connection with that object calls for a higher and complex function. Thus the nerve-cells have a special memory-arousing function.

This is often lost temporarily, as in states of abstraction, or trance states, excitement, epilepsy, delirium, insanity, and dementia. But to overthrow this special memory-function of the cell for the whole brain requires a peculiar and exceptional kind of stimulus.

In fine, the hypothesis which I put forward, very tentatively, is that the nerve-cells have special and highly-developed functions which are in relation with special lines of association-fibres, and that this specialized function is suspended in such a case as that which I have described; just as special functions and memories are inhibited in hysteria.

If we come to consider this case from another point of view, viz., the strictly neurological, one could say that at the very first my patient had complete *apraxia*, including mind-blindness and word-blindness, mind-deafness and word-deafness, mind anosmia, ageustia and amimesis, in fact a total loss of memory for the significance and uses of things and of language. He gradually but with varying rapidity recovered from his word blindness and deafness so that he could read and understand spoken words. His apraxia, as regards the ordinary things of his daily life, soon disappeared so that he could dress, eat, and go about much as others. But a residuum of apraxia remained, and this bore upon abstract ideas, the facts of social and domestic relations. He did not understand the relationships of parent and child, husband and wife, the significance of business acts or the current events. In fact none of the memory-pictures stamped upon his brain by his life-experience previous to his injury could be revived.

The fact that the memory-disturbance in Mr. S. was due to some inhibitory action on a special function of the cells is supported by other clinical facts. It is not by any means rare for persons who receive a severe head injury to suffer from a temporary amnesia. In my own experience I have seen such cases. A youth was suddenly knocked down by a blow on the head. He was not made unconscious, but was slightly stunned. He was helped upon his feet and presented no marks of severe physical injury. But he did not in the least remember where he was, what his name was, where he lived, or what he had been doing. He talked coherently and understood questions, but there was a total amnesia of the past (retrograde amnesia of Charcot). His symptoms gradually improved, and in a few weeks he was entirely well.

It is an acknowledged physiological law that by a severe mechanical shock we can suspend the functions of cell-groups partly or entirely. So with regard to the brain it seems to me easy to believe that the memory-centres have special association-tracts by which they are kept in association with the cortical centres for the special sensations. These tracts must be represented by nerve-fibrils of especial tenuity and delicacy (*BC′*, etc.); it may even be that there is a special part of the cell or special cells with special lines of collaterals differentiated for this function and represented by the dotted lines in my diagram. If this associating function or process of 'revival' is indeed psychologically and physiologically a special one, then it has a definite anatomical representation. And one can understand how poisons and shocks suspend its function. The very fact, indeed, of the comparative frequency of milder forms of shock-amnesia confirms the view that there is such a unity as I suggest.

An interesting practical point exists in connection with this case. Rouillard states that carbonic-acid gas is particularly liable to cause defects in memory, and he even adds that children who are brought up in houses with open fires and insufficient draught are apt to have defective memories. (*Gazette des hôp.*, May 7, 1892.) Fallot has reported the case of a person who attempted suicide by charcoal-fumes. After recovery the memory was defective not only for events subsequent to the poisoning, but extending back to three days prior thereto (retrograde amnesia). (*La Semaine medicale*, Mar. 3, 1892.)

EXPERIMENTS IN SPACE PERCEPTION. (II.)

BY DR. JAMES H. HYSLOP,

Columbia College.

There is another experiment which throws farther light upon the question of magnitude and its perception as discussed in the previous article. It is illustrated in Fig. 4. Draw the circles A and B and C and D on separate cards, so that one of them can be held above the other and at a greater distance from the eyes. Also draw C and D farther from the median line or plane MN, so that when placed at a certain distance farther from the eyes than A and B, the planes LY and OY may pass respectively through the centres of C and A and D and B, and terminate in the retinas of the eyes after crossing, E being the point of fixation. This enables the same degree of convergence to combine C and D at the same time that A and B are combined. When this is done the effect is as follows. The central circle, which is the fusion of C and D, appears to be much smaller than that of A and B. Now as the retinal image cast by C and D, they being farther from the eyes, is smaller than that of A and B (the circles being of the same size), we might say that the phenomenon is due wholly to that fact. But as previous experiments have shown, if the centres of C and D are at the points H and I respectively, the central circle *does not* seem any smaller than the central image of A and B. Measurement shows the fact. This cannot be determined easily by introspection, because under the circumstances the fusion of C and D does not take place at the same time as that of A and B. But the comparison by memory, which is that of two successive seconds or moments, makes them appear the same. However, as this is exposed to error, we can rely upon the measurement mentioned above, and that, as we have seen, makes the central circle the same in magnitude for all degrees of convergence. Now if the diminished magnitude of *C* and *D* under convergence, when their

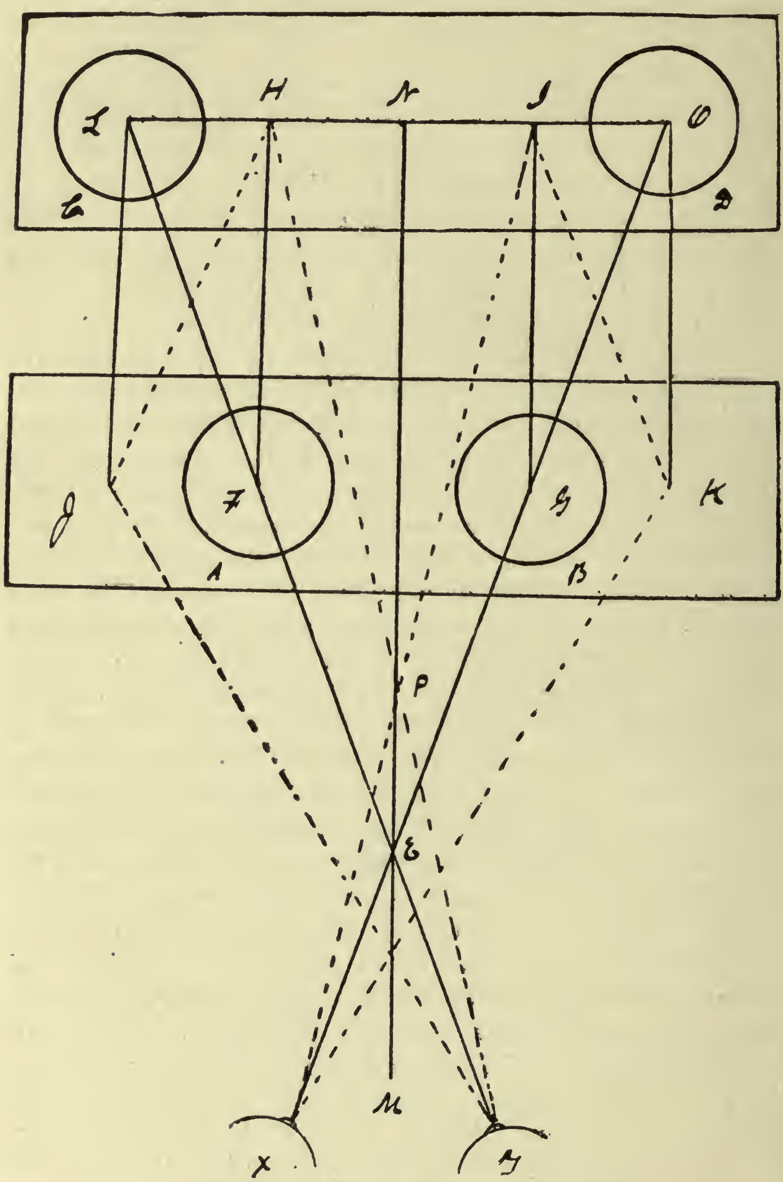


FIG. 4.

centres are at *L* and *O*, were due wholly to the diminution of the retinal image owing to their greater distance from the eyes compared with A and B, then the same diminution ought to take place when they are at the points H and I respectively, because these points are as far from the eyes as L and O, and the retinal images must be the same.

But before referring to the possible explanation of this result we should indicate also a probable reason for the constancy of the apparent magnitude of the central circle in all degrees of convergence, a fact which would appear anomalous if we suspected that the diminution of magnitude in the first place was due to the degree of convergence. But if we note that the angle of convergence FEG for the simultaneous combination of C and D, and A and B is greater than that for the fusion of C and D at the points H and I respectively, and then remember at the same time both that the retinal impressions of C and D are smaller in proportion to their distance from the eyes than those of A and B, and that by hypothesis associative accommodation may coincide with the lessened convergence for H and I, we can imagine that this proportional decrease of convergence and retinal magnitude might exactly compensate for the difference of distance and for the enlarging influence of ciliary expansion or relaxation, associated with diminished adjustment. That is to say, other things being equal, decrease of convergence is, by hypothesis, connected with increase of retinal magnitude due to ciliary relaxation, habit and association having made the two functions tolerably cohesive, and hence with the approach or recession of the circles the changes of convergence might be accompanied by constancy in the apparent magnitude of the circles. But, of course, the difference between the magnitude of the fused images of C and D at L and O, and that of A and B at F and G, can be accounted for by the difference of retinal magnitude, all other things being equal in the case. This is proved by drawing circles on another card at L and O just large enough to cast the same retinal images as A and B. This can be calculated quite accurately. If then we combine them at the same time as we combine A and B, we shall find the apparent magnitude of both central circles *exactly the same.* This would seem to justify the conclusion that, retinal magnitude remaining the same, the effect varies with convergence and combination, and it remains only to determine whether the modification of magnitude is due to convergent or to ciliary influences, or to both of them together.

That merely retinal magnitude, so far as it is determined by the distance of the circles from the eyes, has nothing to do with the effect is amply proved by placing the larger circles, or the card containing them, at *J* and *K*, and the smaller circles at *H* and *I*. If under these conditions we combine, it will be found that the magnitude of the central circles may still be different, but the reverse of what they were in the first instance. The larger circles under fusion appear smaller than the combination of the smaller. The trouble here, however, is that the combination for both sets cannot be effected at the same time, and we have to compare a memory impression in one case with a real impression in the other. But the lapse of time is too brief to attach any weight to this objection, and besides the difference of apparent magnitude is too great in the case to make illusion possible, as every one who can perform the experiment will see. Moreover there are two objective proofs of the fact which make it unnecessary to rely upon subjective impressions as evidence. In the first place, measurement, such as has already been described, shows so marked a difference of magnitude that illusion cannot be supposed without assuming that a man cannot tell the difference between a twenty-five- and a fifty-cent piece. In the second place, we have already learned that the magnitude of the central circle in any case remains constant with the approach or recession of the figures from the eyes. In this way we can make a more or less direct comparison of images at the same time, and the result confirms all that has been described. Hence retinal magnitude may be different, so far as distance is concerned, and the effect the same, or the retinal magnitude of one image smaller than another and yet its apparent visual magnitude greater than the other. We might infer from this that the whole effect of modified magnitude was due to convergence alone, because, in spite of the smaller circles at the greater distance from the eyes, the combination of the larger circles at *J* and *K* appears smaller than that of the others.

The convergence for the larger circles is greater than that for the smaller and the magnitude is correspondingly diminished.

But there is a fatal objection to this hasty inference. We have not eliminated possible ciliary influences and the modification of the lens. By hypothesis ciliary and convergent functions are so cohesive as to act in harmony, and here, noting that the convergence is less for the remoter and smaller circles, ciliary contraction would be less, so that the effect coincides with the possible variation in the associated action of the optical lens to modify the retinal magnitude independently of distance. If this be so, we should find an actual change of retinal magnitude to compensate for the supposed differences due to distance from the eyes, and construction of the circles. But it would be a change which we should have to account for by the associative cohesion of ciliary with convergent influences, and not by reflex accommodation to distance. Hence, though the experiment may show that the effect is independent of ordinary retinal magnitude, it does not seem to eliminate that factor from the problem, because associative ciliary action may influence changes of magnitude which we may think to have eliminated.

But there are facts which strongly contradict the supposition of ciliary influence in this case as before. The first of these is one that we have already mentioned; namely, the fact that no such modification of magnitude accompanies monocular perception when one eye is covered or closed and artificial convergence effected. Under the conditions described, whatever appearances of change take place, they are purely proportional, though we should be obliged to suppose associated ciliary action along with the changes of adjustment, if that action ever takes places at all when convergence is effected. But in this case and in spite of convergence and the supposed ciliary changes the differences of magnitude are not observed. But a second experiment is still more conclusive. It is represented in the circles of Fig. 5. I draw three circles *A*, *B*, and *C*, *A* and *B* being farther apart than *B* and *C*, but in the same plane, so that combination of *A* and *B* will require greater convergence than that of *B* and *C*. Now since these circles are of the same size and lie in the same plane their retinal magnitude will be the same. But if I cross the eyes until *B* and *C* combine, I observe two effects: first, the apparent fusion of *A* and *B*; and second, a greater diminution of magnitude in the apparent fusion of *A* and *B* than that of *B* and *C*. If I combine by negative convergence, enlargement takes place and the fusion of *A* and *B* appears larger than the apparently simultaneous fusion of *B* and *C*. Now we cannot suppose that the differences here remarked are due to differences of retinal magnitude without supposing that two very different degrees of accommodation occur simultaneously with a given degree of convergence. This must be absurd upon any theory of neural mechanics. The same absurdity is manifest if we suppose different degrees of simultaneous convergent tension, and with it different degrees of accommodation. The only other theory of the case that can commend itself is that of a muscle continuum which has no resemblance or analogies whatever with the ordinary theory of muscular tension, based upon the conception of mechanics. Such a view, however, only retains the motor theory in name and not in reality, and we can only wonder at the self-deception and disingenuousness that led to its adoption and the retention of all implications belonging to the muscular sensation-theory. Not that we mean to present any *a priori* objection to the general conception of such a theory, but only that it should be called a doctrine of a muscle continuum while imagining that motor implications were involved and sensory denied. However this may be, it is clear that ciliary influence and its presumably consequent modification of retinal magnitude are not available to account for the alteration of apparent magnitude in the objects in the case mentioned without supposing different degrees of accommodation simultaneously, and that, I judge, no one is willing to do. But if we

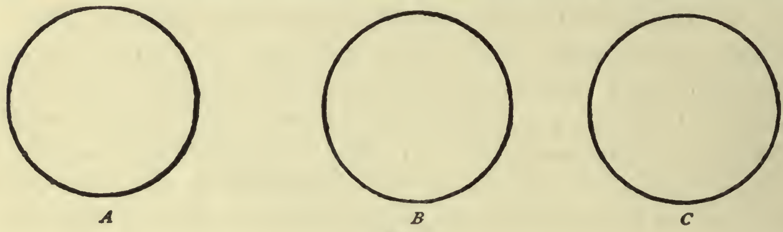


FIG. 5.

thus exclude ciliary action from the effect in Fig. 5 we have no reason to suppose that it will explain the same phenomenon in Fig. 4, and we find that the only definite generalization to be formed in the case is that apparent magnitude is somehow connected with the degree of convergence necessary to produce fusion, whether we choose to regard the influence as motor or sensory. A more complete generalization may be forthcoming in the sequel of the present discussion, after we have examined some of our experiments in the perception of distance.

There is a very pretty, and perhaps conclusive, experiment showing a modification of apparent magnitude when we *know* retinal magnitude has not been altered, whatever connection we assign between accommodation and convergence. It is in after-images, and one effect in it is quite uniform. For instance, I have often tried the experiment with pictures hanging on the wall. The attempt was to cast the after-image upon some other part of the wall nearer or remoter than the picture itself. What I have uniformly noticed is a diminution of magnitude or increase of it according as I look at the nearer or remoter point, after gazing at the picture for a moment. That is, if I throw the after-image on the ceiling, which may be farther off than the picture, as it was in the experiment here described, it appeared larger than the object; if I throw it on the wall near me, fixing my eyes upon the wall, the figure seems smaller than the original. It is to be noted also, that where I can hold the after-image for some time, which happens whenever certain neural conditions favor extreme sensibility, the image seems located at the point of fixation. When the after-image lasts but for a second this translocation is not so evident or clear, as the change of adjustment seems to suppress it. But by quick changes of positive or negative convergence I can even then notice a corresponding difference of locality and magnitude. However, when the image persists for fifteen or twenty seconds, or sometimes for nearly a minute, both the localization and modified visual magnitude are perfectly distinct. Now here is a case where we cannot plead the alteration of retinal magnitude, due to the coexistence of a certain fixed degree of accommodation with a given degree of convergence. Even if this occurs in such cases it cannot alter the retinal magnitude of the impression, and yet we find the apparent magnitude varying with the degree of convergence while retinal magnitude remains absolutely constant. It will be apparent, therefore, that if we thus exclude the influence of ciliary contraction and expansion from the effect, and with it all fixed connection between retinal and apparent magnitude, in this important case, there will be no reason to suppose them in other cases where it is not so easy to eliminate them, and we are left only with the connection between convergence and apparent magnitude, as the only uniformity as yet unanalyzed.

There is one other interesting fact in connection with this experiment which bears upon the perception of solidity, and which should be noted before passing to that question. It is, that in spite of the influence of binocular changes of adjustment to translocate the after-image to the point of fixation, it does not alter its dimensional relations to the plane of vision. Thus, if the picture hang obliquely on the wall, say thirty degrees, more or less, and I look at it while lying on a bed or lounge and then look at the wall vertically near me, besides the diminished magnitude and translocation I notice that the image *does not lie in the plane of the wall, but in the same position relatively to the plane of vision as in its real position.* The top of the picture, which is some thirty degrees from the wall, now seems resting on the wall and the bottom of it seems much nearer. The angle at which it, the after-image, intersects the line of vision remains the same as before, and in the same relative position. At a certain point on the ceiling the image will seem to lie in its plane. This is when the plane of the ceiling and of vision concide. In all other positions the plane of the after-image and the ceiling intersect. I get the same effects from whatever position or attitude I look at the picture, and the same is true of any object from which I can succeed in getting persistent after-images. Here is a case where solidity, or the third dimension, seems independent of motor functions, even though we explain translocation and modified magnitude by them. They may modify localization without modifying the dimensional relations of the object.

But before taking up the subject of distance it will be interesting to use the experiments already described to explain a phenomenon which has generally been attributed to memory and association. We may also find in it a confirmation of the suspicion that ciliary influence or accommodation may not be involved in the modification of retinal and therefore of apparent magnitude, at least to the extent required by the illustrations described. I allude to the phenomenon of the judgment of size at great distances. Physiologists and psychologists were once puzzled by the fact that an object, say a man, a horse, a wagon, or a house always seems as large at the distance of a thousand feet or more as at a distance of five hundred feet, though it was assumed that the retinal magnitude is quite different in each case. Supposing that real magnitude depends on retinal magnitude, this apparent constancy of our impression about size without regard to distance seemed to be quite an anomaly, and hence association with past experience was the resort to explain the phenomenon. But whatever influence may be attributed to association, it certainly cannot explain the constancy of apparent magnitude with the varying degrees of convergence in the experiments above described, while this latter fact throws much light on the normal impressions of mankind as containing natural and functional elements not admitted by the associational theory. It represents exactly the strong feeling we have in ordinary experience that the phenomenon is less analyzable those we find connected with association. But not to urge the force of a feeling merely, this actual constancy of apparent magnitude with the alterations of retinal magnitude and convergence, unless we assume that changes of accommodation overcome or compensate for the former, is evidence of functional arrangements for the judgment of real magnitude. But a fact in connection with the whole phenomenon of such judgments reflects upon the hypothesis of ciliary influence as affecting a modification of retinal magnitude sufficient to compensate for the effect of actual distance in diminishing it or increasing it, as the case may be. It is that beyond the parallel position of the eyes all changes of accommodation, if any, must be so independent of convergence as to cast a doubt upon the fixity of their relation within those limits. If that connection is not a firm and invariable one, there ought to be some variation in the perception of magnitude with any given degree of convergence, which I do not discover. In my own case, however, it is certain that this connection between accommodation and convergence is not a fixed one, while the relation between convergence and apparent magnitude is fixed. Thus for any degree of positive convergence the perception of outline is as clear as in the normal position of the eyes. *But this is true only for the fusion of similar images.* Before the combination of the images they are slightly blurred or indistinct, and if they are different in nature, say a black and a red pencil, they are blurred even in fusion. Again, in negative convergence, as remarked earlier in the discussion, the blurring is constant until the plane on which the figures are placed is located about eighteen or twenty inches from the eyes. For this and all greater distances the images are clear. I notice in all this blurring, which is undoubtedly caused by defective accommodation, that the apparent magnitude is not affected in any way to reflect ciliary influences, and as the accommodation does not assume a definite degree until fusion of similar impressions occurs, while localization and magnitude are proportioned to convergence, there is every reason to discount the associative agency of ciliary influences and to suppose other structural functions, perhaps sensory, to account for the phenomena described, unless motor influences in connection with convergence can be invoked. Further confirmation of the unimportance, if not the absence, of association, and of the independence of convergence and accommodation, is found in the fact alluded to that blurring occurs if I close one eye and then converge while looking at an object, which shows that there is no absolute fixity of relation between the two functions as long as distinctness occurs only when fusion is effected. But if accommodation varied uniformly with convergence there should be equal clearness for all degrees of it, which is not the case except when fusion takes place. Hence I must conclude against the probability that ciliary influence determines either magnitude or localization, while the facts point to other structural functions than the purely motor to account for the effect and to explain the normal judgment of magnitude when the retinal image varies with distance. That is to say, some other function than association and accommodation is necessary to account for the apparent constancy of magnitude in spite of distance and diminished size in the retinal image.

I come next to some experiments on the perception of distance or solidity, or, perhaps better, localization in the third dimension. They will confirm the conclusion just mentioned, and throw some light on the general function of space perception. The first illustration will involve, presumptively at least, nothing but binocular processes. If I draw four circles as represented in Fig. 6, with the centres of the

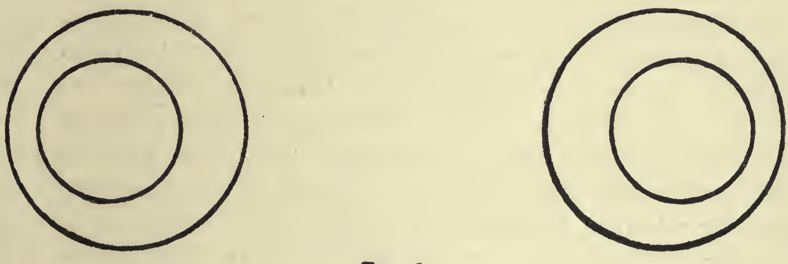


FIG. 6.

two smaller circles farther from the median plane than those of the larger, and then cross the eyes until fusion occurs, I see the usual frustum of a cylindrical cone, with the smaller base nearer the eyes. If I focus the eyes beyond the paper, negative convergence, I get the same effect, except that the relative position of the basis is reversed. The larger end now appears nearer the eyes, and the smaller end farther away. The same alteration of magnitude, in both circles, occurs as it has already been described. But these are not the phenomena to which I call special attention. The fact of apparent solidity is perfectly familiar to all observers. But I have referred to it as preliminary to the mention of certain other incidents not so generally considered, some of which I have not seen in the accounts of any other experimenter. I shall not dwell upon the relation of the circles to the median plane as a determinant of this localization in the frustum, but only upon the facts that the frustum itself seems located at the point or points of real or apparent fusion, and that the length of the frustum varies with the distance of the paper from the eyes, and with the construction of the figures.

In the first place, if I employ positive convergence the localization is at the point of fusion or fixation between the paper and the eyes; if negative convergence it is beyond the paper at the point of fixation. These facts I determine by placing a pencil or some pointed instrument where the figure appears to be. I do not use the point to effect convergence, but after this is done I can point instantly, as in normal vision, to the places where the two bases of the frustum appear, and these are invariably as described. To do this accurately for negative convergence the circles should be drawn upon a piece of glass, and when so done I have no difficulty in indicating the true apparent position. The same is true of all objects or figures combined by binocular adjustment. A screen, for instance, always appears distinctly to be at the point of fixation nearer or beyond the real place according as the convergence is positive or negative, and my eyes can wander all over it without disturbing the illusion any more than most persons can modify the position of objects at will in normal vision. This is so universally true that I could formulate as a law that all fused images are located in the horopter, no matter what their real position in space or their distance from the eyes. At least this is true in my own experience, though subject to qualification from the disturbance of monocular functions, which varies with the nature of the figures or objects combined, being less where the resemblance or identity is great, and greater where differences of surface are marked or distinct.

I have been able to obtain an approximate measure for the length of the frustum of a cone which I see. For negative convergence this is impossible unless the circles are drawn upon glass, and even then there is the difficulty of attaining complete accuracy, because putting the pointer at the right place is not easy for the reason that it is too far away to be sure either of its exact coincidence with the visible line of the figure, or of the absence of double images. But it is quite accurate enough for practical purposes. Then there is also a limit to which even the attempt to measure the frustrum can be made. But nevertheless the measurement can be tolerably accurate for a short distance in negative convergence, and quite perfectly so for the whole of positive convergence. This is effected in the following manner. I take a sharpened instrument, like a pencil, and place it at the point where I see one base of the frustum. This is marked by another person, and I then indicate in the same way the position of the other base. The distance between the two points is then measured. It can be quite accurate, but I have not attempted to get the most accurate results possible, but only such as give something better than my subjective judgment, and the figures will show very suggestive differences under the varying conditions. I hope that those who are devoting themselves to accurate measurements in such cases may take the matter up and investigate it more thoroughly than I have either the time or the instruments to accomplish.

Taking the circles of Fig. 6, which are drawn respectively from fifty- and ten cent silver pieces, and combining them, at a distance one foot the length of the frustum measures ½ an inch, at two feet it is ¾ of an inch, at three feet 1 inch, and at four feet 1½ inches. Beyond that distance the measurement was not attempted because I did not have at hand a pointer long enough to indicate the position of the figure. But experiment shows that the length of the frustum increases with the distance of the circles from the eyes, and the above figures will give some conception of what the general description means. There is only one anomaly to mention, and it is that my subjective judgment of the length of the frustum is uniformly greater than the actual measurement makes it. For instance, at the distance of one foot I should guess the length to be at least an inch, while the measurement puts it at ½ an inch. But the liability to this illusion is very easily explained. In normal vision the judgment of definite distance in the third dimension is never so accurate or reliable as that of plane dimension. I find by actual trial that the error is greater for median than for horizontal distances. Still it is a noteworthy fact that the measured length is much less than the apparent length.

The results of negative convergence were more difficult to obtain and are less accurate, though they show approximate figures. The main difficulty came from the rough means employed to measure the length of the frustum, and were the differences between the lengths for various distances very slight I could place no reliance upon the result. But these are great enough to justify consideration. Another difficulty came from the blurring when the distance from the eyes was less than nine inches. Beyond this the blurring was so slight that the only obstacle to comparative accuracy was the apparent distance of the frustum from the eyes, which was about four feet. However, at a distance of three inches from the eyes the frustum, which was localized at a much greater distance, measured 4 inches; at six inches it measured 7 inches; at nine inches it measured 12 inches; and at twelve inches it measured 18 inches in length. Beyond this distance no measurements were made because of the difficulties attending it.

The length of the frustum varies with the position of the inner circle in the larger circles, being longer as its centre is farther from the centre of the larger. The reason for this is apparent as being proportioned to the amount of convergence to produce fusion. But it is not so easy to account for the increasing length of the frustum in proportion to the distance from the eyes of the plane on which the circles are drawn. If we could say that it was the association of a given degree of convergence with a given retinal magnitude we might rest content; but so far is this from being the case that a variation in the relative position of the centres of the circles always affects the apparent length of the frustum. More especially the difference between the results of negative and those of positive convergence, which is greater than the difference between the separate acts of convergence, at once eliminates the notion that association will account for the effect. For instance, the difference between the point of fixation in negative convergence at three inches distance from the eyes of the figures is about the same as that of positive convergence at three feet distance, and yet the length of the frustum in the former case was 4 inches and in the latter 1 inch, although to get any results at all by negative convergence I had to draw the circles on the glass much nearer the median line than they are on the paper as represented in Fig. 4. Association, therefore, however much it may contribute to the effect,—and this, I suspect, is not much, if anything,—will not explain the phenomenon. But when it comes to offering anything better than association I fear that I cannot indicate anything more than the peculiar visual machinery for judging of magnitude and distance according to a very complex set of relations—the relation between the retinal image fixated and the degree of convergence, on the one hand, and between the same image and all other objects, homonymous and heteronymous, on the other. We merely know as a fact in normal vision that two circles of different sizes and at different distances, though producing exactly the same sized retinal image, the larger being the more remote, will be correctly judged as to real magnitude and distance, often under circumstances where experience and association can hardly account for the whole of the effect. Horopteral relations and their accompanying binocular functions, whatever they are, probably contribute largely to the result. That purely binocular influences may do something will be proved when we come to remark the results of rivalry between them and monocular agencies. But this name is so vague a conception that it can count for nothing except to exclude the efficiency of association to explain the result. Hence I shall not pretend as yet to have reached an adequate explanation of the phenomenon described, but must be content with stating the facts and remarking the difficulties in the way of accepting the associational theory.

There is a very pretty confirmation of this result and conclusion by a variation of the experiment. It might be said that the lengthening and shortening of the figure may be affected less by the changes of convergence than by the actual alteration of the distance of the circles. But in reply it can be shown that the same result can be obtained without altering the distance of the figures, and while they remain in the same plane. If we draw the two sets of circles on two pieces of paper, each near an edge, so that they can be moved toward or from each other, increasing or decreasing their distance from the median line at will, we shall find that the length of the frustum changes with the degree of convergence necessary to produce fusion. Thus as we move the circles farther apart while increasing the convergence to retain fusion, the frustum shortens while its magnitude diminishes. On the other hand, as they approach each other and fusion is sustained the frustum lengthens and the magnitude increases, and all this while the figures occupy the same plane. The only difference is that when the circles retain the same distances from each other but are moved along the median line, that is, to and from the eyes, the magnitude remains the same while the length of the frustum changes. In this case, however, as described both undergo a modification, the contrast between the two different effects showing that one modification does not determine the other. This makes it all the more remarkable, therefore, that when lying in the same plane the third dimension should vary with the degree of convergence except that the modification seems to coincide exactly with the law established in the first case; namely, that localization in the third dimension represents varying relations of distance with the degree of adjustment necessary to produce fusion. It is hard to see that the slight changes of binocular parallax can account for the result, especially as that would seem to diminish with diminished convergence, while the length of the frustum increases under these conditions. But when the circles continue in the same plane binocular parallax increases with their increased distance from each other while the length of the frustum diminishes, and when the parallax decreases with their approach to each other the frustum lengthens. Besides there are two distinct reasons for wholly excluding parallax from the effect. The first is that the figures are in a plane, and the second is that the fused images are exactly alike, the parallax occurring between the monocular and the binocular circles, and the former taking no part in the combination. We seem left, therefore, wholly to the degree of convergence for explaining the phenomenon, retinal conditions apparently being excluded from consideration unless we can take into account something like Hering's theory of space perception. But with this view I am not yet satisfied, and if other experiments seem to exclude the muscular tension of convergence from the effect we can only fall back upon functions which are as yet imperfectly analyzed.

There are several more interesting experiments bearing upon the question whether the perception of distance is a motor phenomenon. If I draw two circles intersecting each other, as in Fig. 7, and combine them either by positive or by negative, I obtain the following result, with the variations of magnitude already described. I see three intersecting circles, with the central one located at the point of fixation, but the two monocular circles do not appear in the same plane. They can be made to alternate with changes of attention, but without change of convergence between remoter and nearer positions than the central circle. Now they can be located beyond, and again within, the point of fixation. But the translocation in this case is monocular instead of binocular; that is, the images translocated are monocular, while the experiment described some years ago in *Mind* (vol. XIII., p. 512) represented the translocation of a binocular image with a variation of attention. In the present case, however, the binocular localization remains fixed, and the monocular images are translocated without change of convergence, though the effect is not due to the shifting of attention from the binocular to the monocular circles, but to a process like the inversion of mathematical perspective in geometrical figures. I can alternate their positions by the effort to think them within or

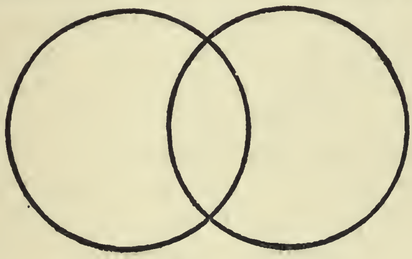


FIG. 7.

beyond the horopter. But this effort has most decided limitations. I cannot locate them thus where I please. They can occupy but either of two positions. The one beyond the horopter is considerably farther from the central circle than the one within it. This coincides singularly enough with the tendency of divergent movements or negative convergence to increase the space representation with the degree of its condition, a fact which would hardly occur if I could determine arbitrarily the distance from the central circle at which the monocular circles should appear. But as this is a fixed position, determined by the relation of the monocular circles to corresponding points, and the function of localization, acting without any apparent change of convergence or separation of images, and as the translocation represents points which do not correspond in motor or muscular terms to the amount of tension or movement which is necessary to account for the distance between them in the median plane, and which must be the slightest in this case, if it exists at all, there seems no reason to suppose that muscular or motor efforts cause the result. There are some features about the phenomenon which look like motor influences, and which I cannot discuss at present. In a more exhaustive description I should mention the particulars, and would feel bound even to indicate and analyze them here but for several facts which depreciate their importance. *Firstly*, there is no proper proportion whatever between their localization and the utmost possible amount of muscular or motor change which might escape visual detection. A fine point in the centre of the two circles remains fused during the translocation, which represents degrees of convergence that would widely separate them. *Secondly*, I am able to translocate one of the circles to a point within, while the other remains beyond, the horopter, though this effect is not easily retained for a long period. *Thirdly*, I have been able with two sets of such circles, one immediately below the other, to get opposite effects at the same time, though only for a second or two, the result being very difficult in this case. But these facts, with the alternation of position of the two circles without the slightest apparent alteration of convergence, at least propose difficulties for the motor-tension theory unless it is made a sensorial continuum, which, as we have seen, is a retention of the muscular theory only in name. However, as already remarked, there are features about this experiment which seem to confirm the theory. I shall not emphasize its negative import, but call attention to the manner in which it refutes the supposition of a fixed and definite connection between accommodation and convergence with a corresponding effect upon retinal and apparent magnitude. In the first place, apparent magnitude is modified with the translocation, while convergence remains unaltered. If this were due to modified accommodation, it is not a fixed degree with any given degree of convergence, and the force of supposing it such, in experiments above described, to account for alterations of magnitude, is wholly lost, and the whole result falls to the responsibilities of convergence if that be accepted as a cause in the case. In the second place, the translocation occurs without any modification of retinal magnitude, though apparent magnitude corresponds to the apparent locus of the circles. The only conclusion from this is that accommodation cannot be resorted to for an explanation of modified magnitudes, while there are also difficulties in supposing that convergence can produce the effect.

But if there are objections to the argument from the experiment just described, there is one more which makes it quite difficult to suppose that localization in the third dimension is due to muscular tension, and I shall describe it briefly. On the walls of one room in my apartment is a wall-paper of yellowish hue and blue squares formed by lines crossing it. If I cross my eyes so as to combine symmetrical portions of the surface, the diminution of magnitude and translocation of the whole surface of the wall is distinctly apparent. But it is interesting to note two anomalies. First, while all the lines or squares which are combined seem to be located in the horopter, if there be a mark or spot in any square that is not also found in the corresponding square combined with it and symmetrically located so as to fall on corresponding points simultaneously with the line of the squares, instead of being seen in the plane of the squares, that is, in the horopter, it is seen beyond it. That is to say, monocularly it seems to be localized where it really is, while binocular influences seem to affect only *similar figures and lines that fall upon corresponding points or involve the attempt at fusion.* Secondly, if a particle of dust be floating between the wall and the eyes (no reflections on my housekeeping) but beyond the horopter, represented in the convergence mentioned, or the plane in which the squares appear to lie, as long as I watch the squares by keeping the attention closely fixed upon them, the particle of floating dust seems to be floating beyond them ; that is, beyond the horopter where it really is. But the moment that I turn the attention to the dust, without in the least altering the adjustment, and although the squares do not change their apparent magnitude nor their locality, the dust seems to be floating nearer the eyes than the squares; that is, within the horopter. This experiment is the same in character as the one referred to above as having been described in *Mind.* I have confirmed it also in another way. If I take two circles, as in Fig. 1, and draw a short vertical line at any point in one of them, preferably in the line of the vertical diameter, and then combine the circles by convergence, this short line does not seem to be located in the horopter, or plane of the central circle. It appears beyond it, although by the process described in Fig. 7 it can be translocated to a point within the horopter. But the one fact to be noted is that muscular tension seems not to affect the locus of any figure which does not at the same time represent sensory fusion.

A very interesting experiment illustrating what has just been described can be performed with an ordinary stereoscopic picture and the stereoscope. Place a mark, scar, or indenture on *one* of the pictures at any point in it, and then combine by means of the stereoscope. While the real perspective is thus developed as usual, the spot put on the picture does not seem to be localized with the points of the picture on which it is placed. It seems much nearer and so to lie in an entirely distinct plane. I have had others perform the experiment and uniformly with the same result as myself. If muscular tension had anything to do with the matter we should naturally suppose that this spot would be equally affected with all others and more especially with those in immediate contiguity with it. But such is not the case, and localization seems wholly to correspond to the fusion of identical points in the impression rather than to muscular tension. In normal conditions these two factors generally, if not always, coincide, and there is no opportunity to discover exceptions. But in the experiment just described the muscular tension cannot be said to coincide both with the localization of the picture and the mark in it which seems nearer, and we are left rather to suppose that localization is rather connected with the tendency or actuality of fusion apart from muscular conditions.

But I shall not go so far as to exclude them from the problem altogether. It is too complicated, and so many facts consist with it that it is best to allow them a contributory effect in some cases, a determining effect in others, while appearing to be wholly absent in still others. But what functions we must appeal to to supplement the defects of the motor theory I am not yet prepared to say definitely. My experiments, so far as their effects justify any generalizations at all, may be summarized in two tentative suppositions, looking to a central explanation of both distance and magnitude, independent both of peripheral conditions and motor impulses. First, apparent magnitude is not determined by retinal magnitude, but by a certain relation between this and the point of fixation or degree of convergence. Second, localization seems to depend much more on fusion than upon motor tension of the eyes. Both these suppositions appear to make the functions of space perception central, sensory or not as you please, but not proportional to peripheral conditions, nor dependent upon what is ordinarily understood by muscular sensations. Whether such an hypothesis can be sustained or not I am not prepared to assert dogmatically, as the question is still an open one with me. But my experience makes it preferable for the present.

AN EXPERIMENTAL STUDY OF MEMORY.

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A complete act of memory requires that impressions shall be retained, recalled, and recognized as familiar and as belonging with certain other impressions. The perfectness of any act of memory depends upon the kind and intensity of the impressions and of the associations between them. Impressions are of six kinds, visual, auditory, motor, tactual, gustatory, olfactory, but the three first named are of most importance in memory. The following experiments were made to determine which of these three kinds of impressions are best retained, and to discover the relation existing between recall and recognition.

Method.—Thirty names of common objects were chosen and arranged in three columns as follows, care being taken not to put together words commonly associated:

I.	II.	III.
box	door	pen
desk	stool	spoon
thumb	slate	pencil
chain	rug	knife
cap	hinge	shears
broom	corn	spool
sock	peach	bottle
bird	shoe	thimble
axe	hat	spectacles
post	watch	book

The test was made upon the pupils of a typical school and college in all grades from the third primary up. The words in Column I. were pronounced to them at the rate of about one every two seconds; those of Column II, having been previously written upon the board, were uncovered one at a time, and rubbed out at about the same rate; while the objects named in Column III were shown at the same rate. In each case after the ten words were given the pupils wrote as many of them as they could remember. Three days later (in a few cases two days), at the same hour, the pupils were asked to write as many words of each column as they could remember. They were then given orally the following lists of words:

IV.	V.	VI.
loud	black	rat
bang	sparkle	spade
whisper	yellow	sheep
boom	red	rake
splash	gloom	nest
hiss	bright	mouse
buzz	green	leaf
whiz	white	hen
tinkle	shadow	cat
ring	pink	coat

They were asked to think of the sound of the first, of the visual appearance suggested by the second list, and of the objects named in the third. How closely they followed these directions it is impossible to say, but their faces indicated that they were trying to do so. There were a few indications in the papers of attempts to guess the words in Columns IV and V that they could not recall, but this was true of only a few.

Results.—The following table shows the average number of words in each list of ten recalled.

	Primary.		Gram. Sch.		High School		College.		Average.		Av. of Both Sexes.
Sex....................	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	
Number............	15	15	39	47	58	53	50	102	162	217	379
Column I.........	5.46	4.33	6.48	6.17	7.29	6.94	7.20	7.38	6.90	6.79	6.85
" II...... ...	5.66	3.86	6.57	5.72	7.09	7.26	7.28	7.81	6.88	6.95	6.92
" III.........	7.26	6.80	8.28	7.85	8.38	8.83	8.56	8.60	8.18	8.36	8.28
" IV	5.64	5.43	6.25	6.17	7.57	6.56	7.65	7.59	7.25	6.79	6.98
" V.........	6.64	5.00	7.97	7.32	8.26	7.49	8.45	8.12	8.37	7.58	7.91
" VI...	5.71	5.56	7.25	7.22	7.57	7.35	7.90	7.85	7.59	7.40	7.48
Average............	6.06	5.16	7.13	6.74	7.69	7.40	7.86	7.89	7.53	7.31	7.40

Reproduced after 3 days.

I.	No. of words recalled.......	....	....	1.51	1.20	1.23	.97	1.79	.95	1.47	1.10	1.25
	Placed in the right column..	....	....	1.20	.79	1.00	.76	1.29	.70	1.14	.74	.91
II.	Recalled.	....	....	2.89	2.05	2.51	1.53	3.22	1.95	2.75	1.86	2.23
	Correctly placed	....	....	2.58	1.71	2.35	1.36	2.24	1.55	2.38	1.53	1.89
III.	Recalled...... ..	....	....	6.46	6.33	6.67	6.29	5.44	6.58	6.23	6.44	6.35
	Correctly placed	...	....	6.23	6.33	6.66	6.27	5.44	6.47	6.16	6.39	6.29
Recalled...........		....	...	3.62	3.19	3.47	3.06	3.48	3.16	3.45	3.13	3.27
Average correctly placed...		....	....	3.33	2.94	3.30	2.79	3.32	2.57	3.22	2.88	3.03

The averages for the first three lists of words, 6.85, 6.92, and 8.28 respectively, show that objects were remembered better than the written names, and the latter better than the spoken names. Doubtless some when they saw the written words thought of the sounds and perhaps retained them as auditory words, while others thought of the visual appearance of the spoken words and retained them as visual words. Many repeated softly the words both oral and written, thus getting motor sensations; and probably many formed mental pictures of the objects named. But, however differently they may have stored the words in memory, the difference in reproducing them, since they are all of the same character, must have been due mainly to the different ways in which they were impressed. The difference would probably have been greater had the conditions for seeing the written words and the objects shown been as good for all the pupils as they were for the spoken words.

The averages for the last three lists of words, 6.98, 7.91, and 7.48 respectively, show that the visual qualities are remembered better than sounds and also better than objects imaged. In this case the impressions were all auditory, but in so far as the pupils followed directions the method of storing was different in each case. The attempt to store as directed doubtless helped reproduction in some cases and hindered in others, but in general the pupils were helped by storing in visual images, as will be seen by comparing the reproduction of the names stored in their own way with those stored as a visual image.

The results of the reproduction after three days were rather surprising. Probably the most enthusiastic advocate of object teaching would hardly have dared assert that if the names of ten common objects were pronounced to and written by pupils they would after three days remember but one seventh as many of them as they would if they were allowed to look at each of the objects a fraction over a second and write the names, yet the numbers .91 and 6.29 indicate that such would be the result. These figures indicate also that the authors of memory systems in which the kinds of associations formed are the only things considered are at fault. Some ground is found for saying, "Make the impressions vivid and the associations will take care of themselves." It is worthy of note also that there were few mistakes in recalling and recognizing the names of the objects seen. If two papers in which all the names were in the wrong column, probably because the pupils made a mistake in the column, had been omitted in averaging, the number recalled and the number in the right column would have been practically the same. This and the fact that mental images of objects are remembered better than their names is of great pedagogical significance, indicating that if objects are shown children, or when that is impracticable, if they are led to form mental images of them, they can obtain a genuine knowledge of things more readily than they can be crammed with the verbal appearance of knowledge.

The table shows an increase, from the primary department up, in the power of immediate reproduction of words of all kinds, but the difference of only about two words between primary pupils and college students is not very great when we remember that it is not so much a matter of memory as it is of mental grasp, and that the younger pupils required longer time to write the words and hence would be more likely to let some of the words drop out of consciousness. In the reproduction after three days the college students show no superiority over the children. One reason for this may be because the experiment was more of an event to the younger pupils so that the words were more deeply impressed. (In one room they discussed it afterwards, each telling how many words he remembered.) I noticed that the third day when I told them what was wanted, the grammar-school pupils began to write at once, while the college students hesitated, apparently making an effort to recall the words. This suggests, and many other facts make it probable, that children are equal or superior to educated adults in impressibility, in retention, and in spontaneous recollection, while the latter have gained more power of *voluntary* acquisition and recollection; hence culture of the memory is not so much an increase in the power to remember as in the power to determine *what* shall be remembered. It is interesting to note that in memory of spoken words, as compared with written, the younger pupils are superior, evidently because they have not had so much practice in dealing with visual as with spoken words.

This experiment, like others that have been made, indicates that females are superior to males in both immediate and delayed reproduction of words. This difference is most marked in the spoken words of sounds, while in memory of the objects seen the boys are slightly superior.

Individual differences were very marked both as to general memory-power and memory for the different kinds of words. Only two—a ninth-grade boy and a senior girl—reproduced all of the thirty words given at one time. Nearly all remembered objects better than words, and a few who remembered words poorly were as good as or better than the average in recalling the names of the objects. Some remembered written words very well and spoken words very poorly, and others the reverse; but quite a number reproduced them equally well, and sometimes the names of objects seen also. It seemed as if they could grasp just so many words and no more, whatever, the mode of presentation. Very few gave the words in order, and it was quite noticeable that the first and last words were less frequently omitted than any others.

Some months later, in order to supplement the above experiments, the following test was made upon 180 normal-school students, the first three lists of words being used. They wrote as many of the first list as they remembered, but for the second list they were requested to simply make a mark for each word they recalled. When the third list was given they were requested to form a mental picture of the objects named. The averages were as follows:

I. 7.33, II. 7.80, III. 8.01.

To make sure that III was not easier to remember than I the experiment was reversed with thirty-four eighth-grade pupils. The averages were:

III. 7.56, I. 8.56.

Which showed beyond doubt that the names of objects are remembered better where mental images of the objects are formed.

Three days later the normal students were asked to write all of the words they could recall, putting them so far as possible in the right columns. The average numbers recalled were:

I. 2.61, II. 1.61, III. 4.22.

And the numbers correctly placed (recognized) were

I. 1.76, II. .53, III. 3.52.

The act of writing evidently had a very marked effect especially upon recognition, since but little more than one twentieth of the words not written were recalled and recognized. How much of this effect was due to motor sensations and how much to the more prolonged attention necessarily given to the words written it is impossible to say. Movements of the vocal organs were probably also an important factor in the retention of all the words, for inquiry revealed the fact that 77 students repeated all of the words after me; and of those who did not, 32 repeated I, 26 II, and 18 III.

In order to determine to what extent the power to recognize completely or partially might remain when the limit of the power to recall has been reached, the thirty words originally given mixed haphazard with fifteen other names of common objects were read and the pupils told to write those that they thought were in the original lists, putting them in the right column as far as possible. The average numbers written were:

I. 7.43, II. 6.05, III. 8.49.

And the numbers placed in the right columns:

I. 3.75, II. 2.74, III. 6.02.

Adding it would seem that on an average 21.97 of the thirty were at least partially recognized. Since, however, on an average three of the fifteen new words were falsely recognized as being of the thirty, it would seem that six of the words apparently recognized were really guessed, leaving 15.97 as the number actually recognized. Probably the proportion was for the majority greater than this, for 47 students put down none of the new words and 30 but one. The high average of words falsely recognized was produced by a few students who depended entirely upon guessing, 18 of them putting in ten or more of the fifteen new words. The power to recognize appears therefore to be for the average student nearly double that of recall.

In individuals the difference is often much greater. One young lady that I had often noticed as able to recognize the correctness of discussions of topics that she herself could not recall was, in this test, able to recall but two words in the second list and none in the first. When the words were read, however, she correctly recognized seven of the first list and five of the second and wrote none of the fifteen new words. There were other students who recalled a great many words, but placed but very few of them in the right lists.

There were some incidental illustrations of false recognition. About a week previously in experimenting upon mental imagery I had pronounced to the normal students ten common words. Many of these were recalled and placed with the memory list. Again, it appears that when such words as 'spool,' 'thimble,' 'knife,' were pronounced many students at once thought of 'thread,' 'needle,' 'fork,' which are so frequently associated with them. The result was that many gave those words as belonging to the list. This is an excellent illustration of how things suggested to a person by an experience may be honestly reported by him as a part of the experience.

The results of these experiments are of more special interest to those concerned with pedagogical problems. They reveal very clearly the absurdity of the common assumption that any subject that requires memorizing gives valuable memory-training, and suggest for further observation and experiment questions which when answered will enable teachers to intelligently direct the exercise of the memory of pupils in general and to correct special defects in individuals.

DISCUSSION.

THE ORIGIN OF EMOTIONAL EXPRESSION.

Recent discussion has brought out certain great facts about the psycho-physics of emotion. The service of the 'peripheral' theory as announced by Lange and James, and especially as argued by the latter, has been to set this problem in evidence. Undoubtedly the stimulating and highly valuable influence of James's treatment—here as on many other points—has been due to a certain frankness and *naïve* clearness which has concealed in a measure the real complexity of the problem.

The outcome of a discussion in which this 'peripheral' theory has had able but, I think, very inadequate criticism takes form about two or three general principles which I am now aiming to state in their general bearing upon the origin of emotional expression. It has been evident from the first that the 'emotion' that the peripheralists are talking about is a phenomenon of instinct—something that a baby has already got; and that the emotion that the adversaries of the theory are talking about is a phenomenon of ideas—something that the baby has yet to get. If this be true—and no one denies the distinction in fact, apart from the terms which have hopelessly obscured it—it becomes just as evident that the question as to what the components of emotion are is really a genetic question. All the elements of the problem of the genesis of reactions—that of the laws of motor development —must be recognized and woven into an adequate theory. This is what I mean by saying that the 'effect' theory of emotion is *naïve*— just as much so as the old 'cause' theory.

When, then, we come to take a broad survey of motor development, in the race no less than in the child, we are able to signalize certain great principles which we cannot do without: principles which stand out in biology and in psychology as essential to any theory of development. The range of facts fairly shown by recent discussion to be available for the genetic theory of emotion-reactions may be stated under three such principles. These are *Habit*, used broadly to include the effects of repetition and heredity, as the postulate of 'race-experience' makes use of it; *Accommodation*, the law of adaptation in all progressive evolution, no matter how adaptation is secured; and, earliest and most fundamental, *Dynamogenesis*, expressing the fact simply of regular connection between the sensory and motor sides of all living reactions, as to amount of process.

I. As for the fact of dynamogenesis: who doubts its force, either in race-history or in the life of the single organism? I have been so sure of it that I have made it the ever-present fact in the whole analysis of the 'motor consciousness.'* Fouillée writes a whole philosophy to expound it.† And as for the advocates of the theory of emotion now in question, no one has done more to prove this truth of dynamogenesis than Féré,‡ and no one more to illustrate it than James.§

But what bearing has this principle upon the theory of emotion? Much every way. We must bear in mind that this principle has always been acting, and always is acting, in every reaction we make; that our reactions have grown to be what they are in all cases by direct reflection of what we have received or experienced; that just as certain as it is that we are experiencing new things every instant of our lives, just so certain is it that we are expressing these new experiences in every reaction that we make. Every one is familiar with Prof. James's view that we never have the same mental content twice. Of course we do not; there was nothing really new about that. But the correlative fact has not had recognition. If we never experience the same twice, so we never *act* the same twice. The new x of content, added to the old c of content, must call out a new x of action, added to the old a of action. If then our reaction is always $a + x$, just as the content which it follows upon is $c + x$, then no reaction is ever that and that only which is guaranteed by habit, inheritance, and what not, in the past.

It is easy to see, however, that the 'effect' advocate has a way of escape from any such easy trap. He admits it all, and adds a pertinent view. He distinguishes content + its expression from content + *feeling* of its expression; saying that there is no consciousness or feeling of the new element of motor process until it is itself reported as a new element of sensory content. Quite possible; it may be so: if the nervous system has developed that way. But the question whether it has developed that way resolves itself into the more theoretical one, how could it develop that way? That is, assuming that it has, what view must we then hold as to the actual mode which the organism has of acquiring reactions to new elements of content; or,

* See my *Feeling and Will*, Part IV.

† *Psychologie des Idées-Forces.*

‡ *Recherches sur la Sensibilité.*

§ *Principles of Psychology.*

in short, of acquiring any new reactions? This clearly takes us into the domain of another of the principles of development mentioned above, Accommodation. But before we turn to that, certain deductions favorable to the theory in question may be shown by what precedes to be necessary from the third of our principles, Habit.

II. It is now evident that a motor reaction of any kind has always two stimulating antecedents: one the influence fixed by habit, and the other the influence of the new elements of content presented by the environment. But we know that habit tends to make reactions automatic and reflex; and that consciousness tends to evaporate from such reactions. As I put it long ago, "psychologically, it [Habit] means loss of oversight, diffusion of attention, subsiding consciousness." * Hence we must admit that the reactions most dominated by habit—the smoothest, most inherited, most instinctive reactions—have *least consciousness*. And, on the other hand, where habit is least influential, where the content is largely new, where the pleasure or pain of its assimilation is great, where attention and effort are strained, where excitement runs high—in all these cases the stimulating influence is new, one which has not yet been brought under the influence of habit, and so one which adds a new dynamogenic influence to the reaction.

It turns out, however, that just those 'expressive' reactions which are most instinctive and reflex (fear, anger, joy, etc.) really do carry with them most of the consciousness which we call emotion—certainly vivid and disturbed enough. What then shall we say? Either that there are other new elements of content additional to the regular antecedents of the reflex; or that the emotion is not the antecedent of the expression at all, but that the reverse is true—the emotion is consequent upon the expression. We cannot hold to the former alternative. Where are the adequate stimulants in conscious content to the newly-hatched chick's wild fear of the hawk? So we must take the other alternative, and *hand over all this class of reactions to the effect theory,* admitting that the emotion, as far as it has fixed instinctive forms of expression, follows upon the expression. I have no hesitation, therefore, in adopting the 'effect' theory of Lange and James as regards inherited emotional expression excited by constant definite objects of presentation. But this argument for it has not been made before, I think.

This is therefore no exception to our law of ontogenetic expression, i.e., the law that that which is habitual is accompanied by least consciousness. The high consciousness is a reflex effect. But we would

* *Feeling and Will*, p. 49.

expect, on the other hand, that in all the ideal states of mind, in all the new complications of content to which the attention gets adjusted, in all emotional states which do not attach immediately and unreflectively to conscious objects of presentation,—that in all these cases the exciting influence should give elements of expression over and above the reactions due to habit. This is really the outcome—and about the only valid outcome—of the numerous criticisms of James recently made from different points of view.

But it must be remembered that a claim is still open to the 'effect' theorist, as was said above; namely, that even though this be true, still the central process at the base of it may not itself get into consciousness as emotion. It may get in to consciousness only as presentation, attention, etc., the emotion-consciousness not arising until the reaction thus stimulated is *reported back* from the periphery. But, again, this whole question of the behavior of the organism in the presence of the intellectually new as opposed to the habitual is only another stage of the question spoken of above ; i.e., the behavior of the organism in the presence of new sense-stimulations. How has the organism been able to acquire new reactions *of any kind*?—a genetic question and a fundamental one. This leads us again to the principle of accommodation, to which I now turn.

III. The principle of 'accommodation'—the psychological phase of the biological problem of adaptation or development—is the most urgent difficult, and neglected question of the new genetic psychology. How can consciousness ever, under any circumstances, get a new and better-adapted function ? In answer to this question there is only one theory in the field, that of Bain, in his latest formulation of which he shows its conformity to evolution requirements. Spencer's theory is purely biological and seems to be open to some of the modifications (and more) suggested by Bain in the following passage,* which is his latest utterance, I think:

"My leading postulates—Spontaneity, the Continuing of an action that gives pleasure, and the Contiguous growth of an accidental connection—are all involved in Mr. Spencer's explanation of the development of our activity. . . . The spontaneous commencement is expressed by him as a diffused discharge of muscular energy (*Psychology*, vol. i. p. 544). He considers that as nervous structures become more complicated, every special muscular excitement is accompanied by some general muscular excitement. Along with the concentrated discharge to particular muscles, the ganglionic plexuses inevitably carry off a certain diffused discharge to the muscles at large; and this diffused discharge may lead to the happy movement suitable to some emergency.

"This is the doctrine of Spontaneity in a very contracted shape; too contracted in my judgment for the requirements of the case. I have adverted to the inferiority of the diffused wave accompanying a ceniral process, whether active or emotional, such as is here assumed. If another source of chance muscular movements can be assigned,

***Emotions and Will*, 3d. ed. 1888, p. 318 f.

and if that source presents advantages over the diffused discharge, we ought to include it in our hypothesis . . . Mr. Darwin expresses what is tantamount to the spontaneity of movement thus: 'When the sensorium is strongly excited, the muscles of the body are generally thrown into violent action.' 'Involuntary and purposeless contractions of the muscles of the chest and glottis, excited in the above manner, may have first given rise to the emission of vocal sounds' (*Expression*, pp. 82, 83). This is spontaneous commencement under circumstances of strong excitement; but I have endeavored to show that excitement is unnecessary, and that spontaneity is a fact of the ordinary working of the organs.

"The second indispensable requisite to voluntary acquisition, as well as to the consolidation of instinctive powers, is some force that clenches and confirms some successful chance coincidence. Mr. Spencer's view of this operation is given thus: 'After success will immediately come pleasurable sensations with an accompanying large draught of nervous energy towards the organs employed.' 'The lines of communication through which the diffused discharge happened in this case to pass have opened a new way to certain wide channels of escape; and consequently they have suddenly become lines through which a larger quantity of molecular motion is drawn, and lines which are so rendered more permeable than before.'

"Here is assumed the Law of Pleasure and Pain. Pleasure is accompanied by heightened nervous energy, which nervous energy finds its way to the lines of communication that have been opened up by the lucky coincidence. There is assumed as a consequence the third of the above postulates—the contiguous adhesion between the two states, the state of feeling and the appropriate muscular state. The physical expression given by Mr. Spencer to this result is, I have no doubt, correct—'the opening up of lines of discharge that draw off large amounts of molecular motion.'"

Bain's three postulates touch the inevitable requirements of a theory, i.e., first to get movements (his 'spontaneity' as a substitute for Spencer's 'diffused discharge' and Darwin's 'purposeless contractions;') second, to get selections made from these movements (his 'accidental success' of certain movements); and third, 'some force that clenches and confirms some successful chance coincidence' ('pleasure and pain,' identified with Spencer's 'heightened nervous energy which finds its way to the lines of communication that have been opened up by the lucky coincidence').

I do not intend to go into a criticism of this scheme in detail, especially as I intend soon to publish a book containing a detailed theory of accommodation.* But it is evident that the truth—if it be true—of 'spontaneity' in developed organisms does not invalidate or even supersede Spencer's 'diffused discharge'; for the phylogenetic explanation of spontaneity—the question how did spontaneity arise—must rest on some such hypothesis as Spencer's. But the question comes: given movements—by either of these principles, both, or neither—how are some of them selected and preserved? Here again the answer comes from both authors: the fitness of those selected, by the application of natural selection to movements overproduced in quantity. This we may admit as most likely. But now—and here we reach our

* A sketch of some of its features may be read in my article on *Imitation* in *Mind*, Jan. 1894.

topic again, emotion—how are these successful, good, advantageous movements kept up? 'Pleasure and pain' is at once on everybody's lips, Bain's, Spencer's, *et al.* But how? Evidently by association, we are told. The lucky movement gives pleasure; it is done again to secure the pleasure again. But we may say: for an association one term must be given; either the pleasure to bring up the movement, or the movement to bring up the pleasure. We must have the presence of the one in some kind of 'organic memory' in order to get the presence of the other. How does the organism get started toward either? Here Mr. Spencer's theory, on the organic side, gives us an answer; and Bain, as it seems to me, adopts it as a supplement, in the quotation made above from his third edition, directly from Spencer. "Here is assumed," says Bain, "the 'law of pleasure and pain.' Pleasure is accompanied by *heightened nervous energy*, which nervous energy finds its way to the lines of communication that have been opened up by the lucky coincidence."

Let us say, then, that something equivalent to 'heightened nervous energy' alone can explain the repetition of useful and pleasurable reactions. After sufficient criticism and definition—which I now reserve —we may call this for convenience the principle of 'Motor Excess,' and say that pleasure and pain can be agents of accommodation and development only if the one, pleasure, carry with it the phenomenon of 'motor excess,' and the other, pain, the reverse—probably some form of inhibition or of antagonistic contraction. On this basis Darwin's well-known 'laws' get their application.*

What has this to do with emotion? Again, much every way. The heightened nervous energy may not be—without further argument now out of place—assumed to be the 'excitement of emotion'; and we may be dealing only with the pleasure-pain process: but even so, our analogy is worth something. Let us ask this question: where in the entire series of events—stimulus, central process, reaction—has the pain come in, before or after the first adopted movement, i.e., the pain that has an inhibiting influence on this movement? The whole phraseology of Spencer and Bain would serve to make us think that it came in *after the movement only*, as part of the effect of the movement, so that, by the memory of the pain thus got, the movement is inhibited. The pain got from the movement serves in memory to warn us not to repeat *the movement.*† But here I take issue blankly, contending that

* Cf. Mr. Dewey's article in this number of the REVIEW, of which I have seen proof-sheets.

† In support of this see Spencer, *Prin. of Psych.*, vol. I. §§ 227 f., §§ 232, 237. Bain's view is seen in the quotation given above.

it comes in *by and in the stimulus* and *before* its discharge in movement, warning us not to move *so as to repeat that stimulus.* It is by this effect that the first adaptive movement is secured.

Let us take for scrutiny the customary illustration—the one which James uses in explaining the 'Meynert scheme' of nervous action. A child thrusts his finger in a candle-flame, and is burned: he thrusts no more, but shrinks. Here the doctrine of Spencer, Bain, and many others, seems to make the function of the pain the inhibition of the thrusting movement, as itself undesirable. But surely the case is very different. The inhibiting effect and the pain are brought about by the burn, and the recurrence of *that*—that is the thing to be prevented. The thrusting movement is a mere incident. Suppose the candle is brought up against the child instead of the reverse: it then shrinks from it just the same. The movement of the former case is inhibited, to be sure; but only because that is the way the developed organism *has learned to escape damaging stimulations* in general. But how it got this way of escaping them, that I contend is just what we are trying to explain—the fact of habitual expression. The real question is: how did the organism learn to withdraw? And the answer must be: the pain must have originally preceded the adaptive movement—as a signal of an injury. And this original differential motor effect of stimulations can only have been acquired by natural selection.

We cannot simply leave the organisms to the risks of getting repetitions of stimulus by accident; for that means that the organism waits the second time for the lucky chance, just as it did the first time: and that is to say that the pleasure of the first experience left no effect which by its mere presence *increased* the chance of luck.

So it follows that, as we had to hand over to the effect theory all the instinctive expressions, as being so reflex that there is no consciousness of them until their organic resonance is borne back to the centres, so now we see that *in its origin* each and every one of them was directly expressive of a state of consciousness, under the law of accommodation by pleasure and pain. These expressions have grown up, by such principles as Darwin's, as accretions to Habit; but only because they at first followed definite pleasure-pain processes.

This principle applies also to all the organic, visceral, conæsthetic sensations so vital to many emotions. For we are of course driven to ask how it comes that habitual reactions, by becoming more reflex and hence less conscious, come to give, nevertheless, by their return wave upon consciousness, such overpowering floods of organic feeling. I think it is due to the fact that it is by muscular movement that these violent often long-continued protective or offensive reactions are carried out. This exhaustive muscular process taxes for its maintenance all the organic processes—heart, lungs, etc.; so that a great mass of organic sensations are thrown into consciousness, and by unbroken association get to stand themselves, in union with muscular sensations, for the damaging or beneficial *kinds of stimulation* that at first excited pleasure or pain. And as far as they were themselves exhausting and devitalizing, they were directly painful. It is common doctrine that in our more violent organic reactions in emotion, the organism is recapitulating in amount the wear and tear of the long process of offence or defence that animal forms were accustomed to go through when they met the objects which now excite these emotions and sensations in us.

My charge therefore is this: the effect theory cannot be true from the point of view of the development or phylogeny of the pleasure-pain consciousness. And the argument is this: If (1) our theory makes use of pleasure and pain as an agent of development, it must make this pleasure and pain antecedent in the beginning to the reactions which stand for the adaptations secured by the pleasure and pain. The Spencer-Bain theory makes memories of pleasure or pain antecedent to the repetition or inhibition of movements, but it recognizes no pleasure or pain *quâ* stimulus to the original adaptive movements. Otherwise we have a dualism on the account of development, i.e., pleasure-pain securing adaptations, and pleasure-pain emotions resulting from adaptations. No doubt both of these *are true as facts in developed organisms:* but we are now talking about *origins.* One of them must be original.* As I have said before succinctly: "The analogies from sensuous feeling [sense-pleasures] are all in favor of the central origin of emotion [idea-pleasures]. No one holds that sensations are felt only as far as they have motor expression. The kinæsthetic theory accordingly forfeits unity in its account of [the development of] sensibility." † If (2) the effect theory do not make use of pleasure and pain as agents of development, then it owes us a theory of development both of sensibility and motor acquirement, for it throws away the Spencer-Bain theory. Such a theory would have to rest, as far as I see, upon 'lucky' chance alone, going back to Bain's early view—before he supplemented it with Spencer's 'diffused nervous discharge'—and developing all movement, voluntary as well as reflex, by simple chance repetition with association. This further, as I have urged, makes an illicit use of the principle of association.

* See the reference to Marshall's 'dualism' below. James attempts to bring the sense-pleasures and pains under his theory, in a recent discussion (see my 'postscript' to this article below).

† *Feeling and Will*, p. 256. Cf. the detailed criticism of Worcester, in *Monist*, Jan. 1893, p. 285.

This latter is the view advocated often by biologists; even those who, as in a recent case,* appeal to psychology for analogies of development. I have before admitted the possibility of such a theoretical view, as regards some slight organic development; but I think with Spencer, Bain, Sully, etc., that it fails to account for any very complex motor acquisitions. It emasculates higher psychological theory by throwing over the teleological function of pleasure and pain—just the one thing which comes into clear consciousness in this matter of development. On such points I think psychologists ought to give some healthy instruction to the biologists.

The place of sense-pleasures and pains, therefore, in my genetic theory, throws light at once upon the theory of emotional expression. Such pleasures and pains are not only indices of organic weal or woe; they are also dynamogenic agents of accommodation, by a direct influence on muscular movement.† And the very same considerations apply also to ideal pleasures and pains, those, e.g., which cluster about phases of attention, ideation, and object-consciousness generally. Intellect could not have developed in the first place, nor have become the magnificent engine of organic accommodation, through volition, which it is, if intellectual, æsthetic, and ethical pleasures were only the resonance of instinct-reflexes. Yet even here many of the qualitative marks, the excitement, the main psychosis apart from the pleasures and pains of new apprehensions, knowledges, curiosities, are just as surely, and for the same genetic reasons, the resonance of instinct-reflexes as are the gross fixed expressions of anger, fear, etc., in animals.

The immediate locus of the hedonic element in most highly-toned psychoses is therefore sensory and central (i.e., not a matter of reaction or 'resonance'), and can only be 'kinæsthetic' and motor in two cases: first when by habit the reaction has become reflex and the emotional storm bursts into consciousness with its organic associations by a return wave from the motor and organic reactions; or when the original pleasure or pain was itself an index of a muscular condition or function.

And we may go a step further and point out that even when the

* Orr, *Theory of Heredity and Development*, pp. 95–101, who bases his theory of development upon the psychological principles 'repetition' and 'association,' and takes no note of the value of pleasure and pain, or their nervous equivalent, in the process.

† I have elsewhere insisted (*Feeling and Will*, chaps. v and xi) that the traditional 'welfare' theory of pleasure and pain must be modified to apply to 'prospective' experience—as an agent of accommodation—if it is to be available in organic development. This contention, like all other genetic considerations, has had little recognition. Dr. Nichols is, in my opinion, perhaps the only recent writer on this subject who sees the importance of origins.

pleasure-pain element is thus reflex, an element in some sort of utility-reaction established by habit, it then nevertheless still plays the original rôle also, i.e., it becomes the index of the relative advantage to the organism of that same utility-reaction in the newer conditions of life, and so tends to secure yet another secondary reaction. In this way while the pleasure-pain process may by constant association get to be part of a sensation, or a whole sensation with a nerve-apparatus of its own, it then also serves, as all other sensations do, to start its own motor expression in some such antithesis of out-and-in movements as that suggested independently and on different grounds by Münsterberg and by myself. This latter reaction is then 'toned' centrally, as the original utility-reaction was, and contributes a new hedonic element to consciousness. We thus have a certain genetic justification for the distinction between 'agreeableness' and pleasure on one hand, and 'disagreeableness' (*Unlust*) and 'pain' (*Schmerz*) on the other.*

Genetic conditions therefore—to sum up—require that there should be three elements in all emotion: (1) an habitual and in the main inherited element, due to a 'return wave' from various instinctive expressions; (2) a present 'accommodation' element of pleasure and pain produced in consciousness by new sensory, intellectual, and ideal processes; and (3) a 'return wave' element from (2) and from muscular and organic processes in vital connection or association with (1) and (2).

The peripheral or 'effect' theory accounts for the presence of (1), and for (3); it does not account for the origin of (1), nor for (2).

The necessity for some such genetic reconstruction of the doctrine of emotion—to straighten out the tangled lines of fact—may be seen by the examination of a recent book in which many of the salient facts are stated with commendable clearness, but which in my view yet fails signally to unite them.† Mr. Marshall, by dubbing emotions instinct-feelings, goes so far—as James had also—to do justice to the fact of Habit in fixing emotional expression; but then he goes on to deny the adequacy of the effect theory of these instinct-feelings. He seems to suppose that there is a mental accompaniment of marked quality attaching to every instinct apart from its return wave of expression. But a genetic view of Habit would have saved him this; for everybody admits that the greater the fixity of habit the less the consciousness, and instinct is usually quoted as the best instance of this very truth.

* This point receives fuller notice in its proper theoretical position in my forthcoming work on 'Mental Development.'

† Marshall, *Pleasure, Pain, and Æsthetics.*

But Mr. Marshall excepts from the definition of emotion, purely on genetic grounds, two great classes of reactions which nevertheless have emotion accompaniments, i.e., imitative reactions, and what I may, with his approval, call 'self-exhibiting' reactions. These are not adequately fixed in instinct combinations, because their range of content and adaptation is too great to allow them to be shut down to definite channels. True again and good, although I by no means accept this classification of such reactions. But the fact of them illustrates the great genetic principle of accommodation; and if Mr. Marshall could bring himself to take a more genetic view, he would see that all the reactions which are now instincts were once in exactly the same state, but have become consolidated in definite ways upon definite objects. It would then be clear that all emotion is, *in its origin* and process of making, largely a central phenomenon of pleasure and pain, but that all emotion *in its development* is becoming a peripheral and organic phenomenon of 'resonance' or reaction, according as, by the law of Habit, consciousness falls away from the business of the centres and attaches more and more to the business of the periphery.

So Mr. Marshall is then driven to a dualistic view of the affective life in its totality. He agrees with every one in making 'pleasure-pain' and emotion both, as it were, thermometers (or why not *algedometers?*) of development, the indications in consciousness of some sort of good or ill fortune. But he is forced to find them to be different thermometers for registering the same scale of temperatures—to carry out an inadequate figure. He himself has brought the same objection to the 'well-being' theory of 'pleasure-pain,' i.e., it should give two lines of development; * and then commits the very same genetic error as respects pleasure-pain in contrast with emotion. He says (p. 93 f.): "The two sets of phenomena are allied in that both are primitive; in both cases we are able to trace their genesis back to the earliest developments of consciousness; both guide towards the advantageous and away from the disadvantageous."

In arguing this dualism by an analysis of the developed emotional consciousness, Mr. Marshall makes out his case again, I think, and adds one or two new and important *aperçus*, such as the difference between pleasure-pain-reactions and emotion-reactions, and such as the claim that pain expression can be inhibited without inhibiting the pain, while the same is not true of emotion. But when he says that "in both cases we are able to trace their genesis back to the earliest

* A criticism which is wide of the mark, since all the evidence goes to show that pleasure and pain represent complementary organic processes. Meynert's reaching and withdrawing, etc.; the above-freezing and below-freezing on the thermometer.

developments of consciousness," it only remains to ask, why does he not do it? That is just the genetic task which I am undertaking now, and which Mr. Marshall dismisses in such words as these (p. 85): "The value of such argument, doubtful . . . even as far as we have gone, becomes more so the farther we proceed, because of the uncertainty as to the history of our racial development."

To show it would require not only some such hypothesis as Spencer's 'heightened nervous energy,' as the basis of Bain's pleasure-pain factor, but also another kind of heightened nervous energy—for what else could it be?—to represent emotion. Is it not evidently better to say that one sort of heightened nervous energy does for both, and that the conscious difference is due to the different sensory elements which come in together with the pleasure-pain? In sense-states we have pleasure-pain—*Gefühlston*—plus sense-quality (visual, auditory, etc.); in emotion-states we have pleasure-pain—*Vorstellungston*—plus sense-quality (muscular, organic, etc.). The difference, then, is one of developmental level. This seems to me to be fully covered by my hypothesis stated above that pleasure-pain represents the new accommodations, both at the beginning and at every stage of development, and that emotional expression represents the habits organized on the basis of the perception and recognition of objects. The possible dualism then is that between pleasure-pain and sensation.

Postscript.—Prof. James's remarkable clearing-up article on 'The Physical Basis of Emotion' in the last number of this Review calls for an additional word of comment. This paper of his practically settles the controversy over his theory, I think. It shows that we have all misunderstood his book and also, I may say frankly, that he is to blame for the misunderstanding. In my opinion, he now states a theory so different from that in his book that it is fair to say either that criticism has driven him out of his old position or that what he has himself called 'slap-dash' treatment—I call it above (written before his paper appeared) '*naïve*' treatment—misled us all. At any rate, no one should now read, much less teach, his book without practically substituting this article for his chapter on 'Emotion.'

In his new statement, Prof. James claims three elements in emotion: (1) the *direct* reverberation or reaction element, a setback from muscles, organs, etc., in consequence and *contrast* to the incoming stimulus which brings such reactions about. This element is so excessively emphasized in his book that most of his critics have supposed he meant this alone. But to refute all such he now, perfectly legitimately, I think, brings out the second factor in emotion, i.e. (2) the *associated* mass of content—ideas, etc.—which hangs together, however remotely, with the direct reverberation, and so secures all the power of association as an explaining agent. This he urges with great strength in this article, smiting most of his critics hip and thigh. This principle is fairly included by inference, I think, in his book—although so feebly and dimly that the blame is really his that so much good philosophical print has been spent in making the objections to him which he now fully and clearly sweeps away. I must add that I would not have supposed that he himself had thought out these associative extensions to his theory when he wrote the 'Emotion' chapter; for he must have seen that to present them would strengthen his book to an enormous degree. But granted the success of the 'association' element which Worcester and others so plainly overlook, Prof. James now brings in his third element in emotion, i.e., (3) all pleasure and pain tone in consciousness due to 'incoming currents.'

Now to say that the *Gefülston* of sensation, admitting that it is involved in the sensation process itself and is not due to a reaction or reverberation, "falls comfortably under my [his] theory"—this is to blow the frog of his original theory up big enough to rival the ox. Why! who is there to oppose a theory which covers everything so 'comfortably'? I know of no one, unless it be some radical Herbartian who holds that central *Hemmungsprocessen* can go on in the brain entirely apart from sensory conditions and 'incoming currents.' If Prof. James has meant all along what he now says, then I for one have shown in what I have written in the foregoing pages about pleasures and pains of 'accommodation,' both sensory and intellectual, that I agree with him; but it was with a very different understanding of his views that I wrote the pages above which include the passage quoted from my book (*Feeling and Will*, p. 256).

That I am now right in saying that in his original chapter he takes no account of any elements but those of resonance, muscular and organic, is shown by the following quotations. Under the caption 'Coarser Emotions' we read (vol. II. p. 458): "Each emotion is the resultant of a sum of elements, and each element is caused by a physiological process of a sort already well known. The elements are *all organic changes*, and each of them is the *reflex effect* of the exciting object" (italics mine). And under the caption 'Subtler Emotions' (II. p. 471) "In all cases of intellectual or moral rapture we find that unless there be coupled a reverberation of some kind with the mere thought of the object or cognition of its quality . . . our state of mind can hardly be called emotional at all. It is in fact a mere intellectual perception of how certain things are to be called. Such a judicial state of mind is to be classed among awarenesses of truth ; it is a *cognitive* act" (italics his).

Moreover, Prof. James now sees that he agrees with his critics except on one point, which I think it is the main merit of the whole discussion to have brought to the front. He says : * "It may be after all that the difference between the theory and the views of its critics is insignificant." Why? Because—and the following passage shows that it is not James's theory which has become 'orthodox,' as he hopes, but James himself—"The only feelings which I myself cannot more or less well localize in the body are very mild and, so to speak, platonic affairs. I allow them hypothetically to exist, however, in the form of the subtler emotions, and in the intrinsic agreeableness and disagreeableness of particular sensations, images, and thought-processes where no obvious organic excitement is aroused." It is true that he made such an admission in his book regarding 'subtler emotions'; but it seemed clearly contradicted by the context, and I was one of those associated with Lehmann and Irons who said that such an admission would 'give away' the whole theory. Nor do I think we were guilty of an *ignoratio elenchi*, as he now says, although we certainly would be to repeat the charge now.

The final point which James's article sets in the focus may be put in his words : "Must we admit, in the complex emotional state of mind, an element . . . distinct both from the intrinsic feeling-tone of the object and from that of the reactions aroused—an element of which the 'liking' and 'repugnance' mentioned above are the types, but for which other names may in other cases be found?" "Are *these* a third sort of affection, *not* due to afferent currents, and interpolated between feelings and reactions which are so due? Or are they a name for what . . . resolves itself into more delicate reactions still? I incline to the latter view."

I also incline to the latter view and hope soon, in my new book, to give some genetic reasons for so doing. So I am only too happy to say that I am now with Prof. James all along the line, and I hope he may see in the genetic positions stated above some further grounds for his views. But we may still ask—those of us who *now* agree with him, for we are probably many—who has been converted to orthodoxy?

J. Mark Baldwin.

Princeton University.

* This Review, I. July 1894, p. 524.

PSYCHOLOGICAL LITERATURE.

Le sentiment et la pensée et leurs principaux aspects physiologiques. Essai de psychologie expérimentale et comparée. ANDRÉ GODFERNAUX. Paris, Alcan, 1894. Pp. xix + 224.

This very readable and suggestive book bears a major thesis and several minor theses. The major thesis is the well-known one of 'parallelism' or 'double-aspect,' according to which consciousness is the accompaniment of a continuous chain of 'motor phenomena,' between the links of which the causal relation is exclusively to be sought. The minor theses, as I read the author's meaning, are, first, that the motor phenomena in question are actual muscular contractions and tendencies to contraction ; second, that these contractions and tendencies are primitively realized in consciousness as feelings or affective states, rather than as percepts or ideas ; third, that they form emotional conditions and moods of feeling so long as they remain unsystematized and diffused, and that at such times thought proper or ideation becomes either inhibited or incoherent ; fourth, that they contract adhesions with the elements of thought proper, and then, when they run in well-organized channels, cease to be felt affectively, whilst the ideas or thoughts with which they are connected occupy the surface of consciousness ; and finally, fifth, that in this subconscious state the motor tendencies nevertheless form the associative links between the ideas, so that sentiment must be said to determine ideation and not ideation sentiment, and the affective life, in a word, with its deeps and shallows, appears as the shifting sea-bed over which all our thought passively floats.

This, the reader sees, is a very fine programme of psychologic theory for a young author to start upon life with, and many future years of industry may well be spent by him in working out and strengthening its parts. At present he modestly admits that its proofs are imperfect in many places ; and I must confess that, as I read it, some of its essential elements seem far from being clearly conceived. In the parallelism theory itself, for instance, previous authors have usually imagined the 'motor phenomenon' to which the conscious one corresponds to be the nerve-current or discharge from one region of the cortex to another. This kind of motor phenomenon might conceivably be, at times, a direct link between ideational centres, so that ideas might well follow upon each other as its consequence, without any specifically muscular tendency or contraction being interposed. M. Godfernaux simply passes this hypothesis over in silence, and one misses in his frequent references to his own theory any definite schematization of the way in which the ideational beads get strung upon that thread of muscular 'tendencies' which he relies on for the associative work. Another point which I find obscure is this author's conception of the 'systematization' of his motor tendencies. In the purely ideal world we have systems realized before us at every moment of our lives. Ruling purposes ministered to by successively-subordinated means, organized wholes with parts involved, general heads with subdivisions under each, whole trees of thought, in fact, with their branches and twigs complete, are among the most constant objects of our contemplation. Here is 'synthesis,' realized and understood as fast as represented, and only made difficult of comprehension by that false theory of association which insists that all ideas are 'loose and separate,' and that each knows its own elementary object, but nothing in the world besides. Once admit an 'idea' to be capable of knowing a whole system of things at once, while the particular things that make up the system are determined by the concomitant brain-processes, and there is no farther paradox. M. Godfernaux, however, conceives the ideas in the Humian way, and rightly saying (pp. 160, 161) that the English school cannot explain how, when so few of them can be at once in consciousness, our mental syntheses and systems can imply so many, thinks he sees his way out of the difficulty by invoking muscular tendencies. These tendencies, he says substantially, are themselves branched like trees. There is always a fundamental or synthetic one, diffused and vague, which decomposes itself into others more concrete and determinate, of a similar sort, the whole forming a co-ordinated system of channels of discharge, adapted more or less to the environment, and bearing on their extremities, so to speak, ideas as twigs bear leaves. Such an image may pass as a figure of speech. But how does a system of motor tendencies, existent only in succession, and neither one cognizant of any other, form a 'profounder' explanation of our power of mental synthesis than does the 'association of ideas,' even when the latter is atomistically understood? The motor tendencies can *realize* the whole synthesis from moment to moment no more than the loose and separate ideas can; but this realization of the whole synthesis at once, this modification of one element by another of the system, this cognizance of past and future along with present, is just the conscious phenomenon to be explained. I must say that M. Godfernaux's notion of muscular synthesis, so far from shedding clearness on the subject, is itself in need of clearing up.

But in these general matters a second edition may bring improvement to the book, so I prefer not to criticise, but to notice some of the really luminous suggestions of detail which it contains. Its author begins by studying the alterations of the balance between intellect and feeling that occur in various insanities. His studies of mania and melanchology here are more ingenious (and certainly in some points truer) than anything that I have read. Mania is an incoherence due to the lack of any systematic purpose in the thinking. Systematic purposes are one by one introduced into the mind by the process of education. A mother, for example, teaches her child to repeat a fable of La Fontaine by giving, if she be a competent teacher, a unity of interest to the successive words, which thus grow into a system, disconnected from the thousand irrelevant perceptions, auditory, visual, tactile, etc., by which they were accompanied in the learning. This system is essentially one of tendencies to vocal reaction, regulated at first by other tendencies, those of attention to the mother's appeals, but afterwards coherent on their own account. Any tendency which later may prompt the child to think of the fable will bring the latter up synthetically, or as a whole, and the irrelevant perceptions will not revive along with it. Observations of exalted memory in hypnotism, etc., show, however, that the irrelevant perceptions can persist subconsciously, and may consequently be liable to recall when the subject's interest in the fable as a synthetic object no longer exists as an inhibiting force. Now in mania all persistent purposes and interests on the part of the subject have temporarily lapsed, and what is the consequence? Inhibitions cease as well as tendencies to constructive synthesis; all possible ideas acquire an equal value for associative purposes and an equal susceptibility to revival; and thus we find explained, along with the 'objectivity' of the maniac, and his absence of definite interests and pursuits, his incoherence, and the great copiousness of his mind in single ideas and unexpected turns. As in mania we have a morbid exaggeration of what may be called elementary objective thought, following on the suppression of emotion; in melancholy and ecstasy, in turn, we have morbid exaggerations of emotion and a concomitant suppression of thought. One subjective interest, that of the patient's personal welfare, becomes supreme, and inhibits all the usual perceptions and associations by the fixity with which the ideas connected with this interest agglutinate themselves together. In the extreme degree of this expulsion of thought from the mind by feeling, we find what is called stupor, a sort of direct perception of the self and naught besides, with no play of ideas whatever. In all this it is immaterial whether the invasive flood of feeling be of a depressive or of a joyous kind. Melancholy and ecstasy agree in both inhibiting thought's flow.

In normal life M. Godfernaux shows how we oscillate between moods of 'exaltation' (which is a mania-like condition), of 'optimism' (which is an ecstatic condition with 'rarefaction' of ideas), and of depression, in either its melancholy or its hypochondriac form. He enforces in a somewhat new way the doctrine that impulsive tendencies with a definite motor outlet allow a minimum of consciousness, and that conscious feeling and thought are both of them results of inhibition of motor-discharge. Throughout the volume he brings out the close implication with each other of the motor and the conscious life, and makes one feel vividly once more how large a field offers itself here for psychology to make new paths in. And although his own clearing of some of the paths cannot be regarded as definitively complete and satisfactory, it must be said that his book is the work of a genuinely original analyst and observer, from whose continued contribution to psychology we have evidently much to hope in the future. W. J.

Materials for the Study of Variation treated with especial regard to Discontinuity in the Origin of Species. WILLIAM BATESON. London and New York, Macmillan & Co., 1894. Pp. xv + 597.

This important work will probably set up in biology as vigorous a wave of observation and discussion as that which Weismann's works have occasioned. Darwinism in the stricter sense (to which the word Wallaceism might perhaps better be applied) is synonymous with the doctrine that the discrete differences between existing species are due to the summation, pursued through successive generations, of numerous small variations in the same direction, of which variations the intermediary degrees have become extinct, leaving 'chasms,' larger or smaller, between the extremes. The discontinuously-varying species thus formed nevertheless inhabit environments of which the influential factors, such as warmth, altitude, depth of water, salinity, etc., vary continuously; and it is hard to understand why, if so many intermediary steps once existed, they should so generally have perished in the struggle for life. Did they ever exist? May not some of the original variations have been more abrupt and discontinuous than Wallace and Darwin have supposed? This is Mr. Bateson's question, and most of his book consists of a catalogue of such cases as he has been able to find in every branch of the animal kingdom, of discontinuous variation. The cases are numbered from 1 to 886; and when I say that many of them are typical and carry numerous references under them to their similars reported elsewhere in the literature, some idea of Mr. Bateson's industry, patience, and minuteness may be conceived. The details are not for the general reader, nor will critical comments on matter so morphological be expected in a psychological review. But the general thesis of discontinuity in variation is as important in psychogeny as in any other branch of the theory of evolution, so a brief word about Mr. Bateson's results cannot be out of place here.

He of course does not pretend to deny continuous variation, or the accumulation of small steps by selection. What he denies, or doubts, is that such variation and selection can by themselves be responsible for the entire diversity of the animal kingdom as now found. More often than is supposed, he thinks, the new variety must have been a sudden 'sport.' He mistrusts the explanation of so many of the small details of structure by survival of the fittest. Species are only approximately adapted to their circumstances, and live as much in spite, as by virtue of what they are. Dull color may protect a moth, but the particular benefit of one pattern of dulness over the closely-similar pattern of the next species is almost never clear to the inquirer. Colorings are due to the correlated variation of enormous numbers of scales, hairs, or feathers. Have these varied piecemeal and been selected by successive steps, or have they varied simultaneously as a system? The simultaneous variation of all the similar elements of a system of tissues must be admitted as a possibility; and the result of Mr. Bateson's collation of facts is to make it a probability. For instance, in the matter of color he gives a heap of cases where the differently-colored varieties of a species tend to fall into a few discrete groups, and rightly says that this suggests much rather the presence of definite steps of chemical instability in the coloring matter than an environmental destruction of once-existent intermediary tints whose harmfulness we cannot guess. Similarly of the variations of size in an organ. The forceps of earwigs and the horns of beetles are measured, percented, and plotted on curves by our author to show that certain species are 'dimorphic,' and have two sizes of greatest frequency round which the other sizes cluster. These two discontinuous sizes seem therefore to represent conditions of least morphological resistance or strain under the specific conditions. The current explanation of such facts by reversion is, as Mr. Bateson points out, a conjecture without proof.—From such 'substantive' variations, as he calls them, as these, he passes to what he names 'meristic' variations, or variations affecting serially-repeated segments or appendages of the body, and shows not only that new segments may suddenly appear with all their parts complete, but that the same parts of segments may similarly vary throughout a whole series. The bulk of the book is taken up with these meristic variations. All the divisions of the animal, and some of the vegetal, kingdom are passed in review successively for illustrations. The outcome is the exhibition of an immense amount of abrupt variation, none of which I will retail to the reader. Some of it is of the sort on which new species are founded, as where vertebræ, teeth, tarsal joints, dermal plates, annular segments, etc., appear in unusual number; but much of it consists of what can only be called monstrosity, as extra legs, horns, claws, fingers, or, speaking generally, of organs of a useful type growing where they should not be. It must be said on the whole that the bearing of the cases which Mr. Bateson has collected with such admirable diligence is more directly physiological than it is phylogenetic. So many of the odd things he shows us are mere extra copies of organs already adaptively evolved, that radical selectionists will say they throw no light on the original process of the evolving, but only prove the existence, when an organ has once been evolved, of a tendency to what Prof. Owen might have called its irrelative repetition. It seems probable that a hard fight may be waged on this point. Meanwhile Mr. Bateson certainly has, especially by his elaborate discussion of extra limbs and parts of limbs, given a considerable push to the more mechanical manner of looking upon organic growth. The 'major' symmetry which, from the first bisection of the ovum, dominates the forms of so much of the animal kingdom, suggests strongly the influence of mechanical forces, especially when we consider the not infrequent cases of duplication of median organs (as the uterus) and the fusion of symmetrical ones (as the eyes). But Mr. Bateson proves also the existence of a tendency to 'minor' symmetry in the limbs and other appendages. In vertebrates, e.g., the phenomenon of extra digits is not rare. In only one case, however, that of a monkey, out of the countless numbers studied by Mr. B. in museums and printed records, do the redundant parts add themselves to the normal ones in successive parallel order. In all the others they seem added in mirror-order, with an axis of symmetry somewhere in the midst—in fact the name of 'double-hand' has been given to some of these cases. The addition thus appears as a unit, with a certain tendency to relate its parts; and a force must be admitted which may at all events lead to the production of total organs at once, whatever effect such force may be found to have in the modification of species by descent.

Amongst the by-products of Mr. Bateson's investigations we find his disbelief in the current doctrine that domestic animals, taken generally, are more variable than wild ones, and his almost equal disbelief in the rule that a sporadic variation must be quickly swamped. It seems to be a matter of the constitution of the particular animal considered.

As regards psychology, it is clear that the triumph of views like Mr. Bateson's will strengthen the hands of the anti-associationists, and in general of those who have contended for an autogenous origin of certain human faculties, of certain instincts and tastes, for example, or of conscience, the higher reason, and the religious sense. The book is a masterly production, and unquestionably inaugurates a new department of research. W. J.

Cock-Lane and Common Sense. ANDREW LANG. London and New York, Longmans, 1894.

Die Entdeckung der Seele durch die Geheimwissenschaften. CARL DU PREL. Leipzig, Günther, 1894. Pp. 258.

Mr. Lang has the memory of a bookworm and the pen of a *fin-de-siècle* journalist. The result here is a very curious compound of erudition and flippancy, in which the author drags us up and down all the ages of history and to and from all the ends of the earth, in order to make us feel the improbability that clairvoyant trances, 'levitations,' knockings, scratchings, and other noises, stone-throwings, movements of furniture, ghostly apparitions and the like, which *semper et ubique* have been alleged forms of experience, should be due to nothing but an original folk-lore tradition perpetuated and copied by sporadically-recurring fraud. From these persistent and apparently natural types of phenomenon he distinguishes genuine folk-lore beliefs like that in brownies, fairies, and the witches' sabbath, which are much less omnipresent in human history. He makes very merry over the unexacting rules by which 'Science' has hitherto held herself bound in giving explanation of these narratives, and finally he himself—declines to conclude! In all this his state of mind is the pattern and exemplar of what at all times has been that of the 'man of the world.' To be sure Mr. Lang, when his learning is considered, is a very rare man of the world. But he has the worldly lack of reverence even for 'Science,' as well as the worldly bias for fair play and relish for what he calls 'sportsmanlike' treatment of a subject. He has the worldly suspicion that 'where there is smoke there is fire,' but also the worldly dislike to push a thing too far, the worldly reluctance to stand committed and responsible, and the worldly love of keeping some thrilling mystery perpetually open to play with. So his book baffles the reader as the subject has baffled the author; and the most one can say of it is that it is the typical expression of a state of mind that is now common enough. As a skirmisher in the cause of 'psychical research' it will probably be effective; but it should have had an index, to make it useful to the more serious student of the sort of material which it contains.

If Mr. Lang feels baffled by his facts, not so does Baron du Prel. This writer has a *Schlagfertigkeit* at explanation quite equal to the great range of his learning, but the present work is a poor one by which to judge him on the theoretic side. The reasonings on which his theory of the 'transcendental Subject' is grounded are more fully given in his other works. The present one rather takes the theory ready-made, and in a number of chapters gives illustrations of its way of working in such things as emotional and æsthetic expression, somnambulism, thought-transference, clairvoyance, premonition, automatic writing, and speech in foreign tongues. The book is in fact a collection of distinct essays with the transcendental Subject as their nucleus. Our conscious intelligence or Ego, according to Du Prel, is only a partial manifestation of our soul, dependent on the brain and the senses. It has its roots in an extension of the same soul, which in addition to possessing the non-sensuous powers of cognition manifested in trance-states, etc., is the architect of the body and guider of its organic processes, and consequently the original moulder of the brain and senses themselves. This transcendental Subject is an individual entity, and so far Dr. Du Prel is not a Monist; though if we ascend to the substance of the world he admits that the various transcendental subjects may be englobed in the ultimate unity. In all this his hypothesis is more positive and elaborate than Mr. Myers's doctrine of the subliminal consciousness, and less elaborate than the 'theosophic' theory of personality. In the ordinary dream-phenomenon of conversing with an external interlocutor whom on waking we recognize to be our own creation, he finds an analogue of the relations of the normal or sensuous consciousness to the transcendental Self. After the great awakening we may find our sense-life similarly reabsorbed into the wider transcendental unity. That the dream-life plays a great part in the establishment of our author's ideas, those acquainted with his Philosophy of Mysticism will remember. In the present book he explains 'premonitions' (as, for example, the giving-up of one's passage in a steamer on account of a sense of impending evil) as due to emotional vestiges in the waking consciousness of clairvoyant prophetic dreams whose sensible details have been forgotten. The slenderness of the clues which Baron du Prel is not afraid to follow is shown in the first essay, 'On the psychic activity of the Artist,' of which the thesis, briefly given, is that the power that produces works of genius is the same supersensuous

Subject that makes the artist's own organism. The proof of this is that while talent copies nature, genius does not copy but produces works co-ordinate with nature, lending soul and life to the bodily things it represents, as in the personifications of nature in lyric poetry ; and, as in the dramatic and pictorial expression of the emotions, giving body and object to the thoughts of the soul. The same soul that drew the gestures in Leonardo's Last Supper, etc., prompted those gestures in Leonardo's person, and organized Leonardo's nervous system for their execution.

The range of our author's anecdotes is very great, and his choice of them absolutely uncritical. He appears to hold for true anything which any one may ever have reported, the publications of the Psychical Research Society being almost the only source not drawn upon in his pages. Add to this his unchartered freedom of theorizing, and the result is of course completely 'unsatisfactory,' although the book remains 'suggestive' enough. But in the present era of anarchy in these outlawed matters no one can be punished for any special sort of unsatisfactoriness in which he may prefer to indulge, so I say no more. Nevertheless between Mr. Lang's facility in leaving things unsettled, and Baron du Prel's facility in concluding them, it seems as if a better path might be found. Might not the earnest temper of science be combined somewhere with Du Prel's learning and the power of doubt of Lang? So far Mr. Myers's papers on the 'Subliminal Self' seem to have kept nearest to this ideal ; and both Lang's and Du Prel's books set off by contrast the superiority of his work. W. J.

THE NERVOUS SYSTEM.

Die Nervenzelle bei der Geburt und beim Tode an Alterschwäche. C. F. HODGE. Anatomischer Anzeiger, Bd. IX. No. 23. Journ. of Physiol., vol. XVII. Nos. 1 and 2.

These studies have been made on men and bees. That the active tissues of the body represented by the glands, nerves, muscles, and blood should exhibit changes in their structure due to old age was long ago probable, from what was known of cell-activity. One after another such changes have been demonstrated and the present paper shows the differences in certain parts of the nervous system of very old bees as compared with those that have just hatched from the pupa ; and between the cells of a child at birth and those of an old man dying at the age of 92 years. The differences are illustrated by figures. When in the latter case the spinal ganglion-cells of the man were compared with those from the child, taken as a standard, it was found that in the senile cells the volume of the nucleus was reduced to 64 per cent, that it was irregular in outline and shape, that the nucleolus was visible in but one tenth as many cells as in the child, and that while in the latter the cells were not at all pigmented, in the old man all of them had pigment, and in two thirds of them it was abundant. In the cerebellum it appeared that some of the Purkinje's cells were shrunken and that some perhaps had entirely disappeared. In the cerebrum no differences were determined, but in this locality the inquiry was not extensive. In the young bees the nerve-cells are smaller than in the aged ones (antennary ganglion), they have a large nucleus surrounded by a thin layer of cytoplasm, and are absolutely more numerous in the young than in the very old, in the proportion of 2.9 cells to 1. The senile cells have shrunken nuclei and the cytoplasm reduced to shreds, separated by large vacuoles. The gross changes in these cells for both men and bees are similar to changes found in the fatigued nerve-cells (bees and cats), but the absence of increased granulation and a deeper staining of the nuclei indicates that the chemical constitution of the cell has altered in the process of growing old. There are grounds for a close analogy between fatigue and senescence, and the differences in the nerve-elements doubtless depend on the fact that while fatigue is accompanied by the consumption of the stored materials in the cytoplasm, old age is characterized by a diminution not only of the stored materials themselves but in the power of restoration.

A Microscopical Study of the Nerve-cell during Electrical Stimulation. C. F. HODGE. Journal of Morphology, vol. IX. No. 3, 1894.

The changes in the nerve-cell which Hodge has been able to observe after electrical stimulation of the sensory nerves in the frog and cat; as the result of diurnal fatigue in birds and bees, and as an expression of old age in bees and man, have led him to attempt the direct observation of nerve-cells while they were being stimulated. The cells employed were those of the spinal and sympathetic ganglia of the frog. The method of observation consisted in removing symmetrical ganglia from the same animal and placing each ganglion on the stage of a microscope where its further changes could be followed. Both were enclosed in chambers and irrigated by a nutrient fluid. On one slide, however, wires had been laid, so that the cells there located could receive electrical stimulation. The two specimens were then examined from time to time and the nuclei in which the principal changes occurred were drawn and measured at regular intervals. The manipulation of such experiments, which were carried on anywhere from fifteen minutes to six days, was extremely difficult, and hence many variations were observed, doubtless due to lack of identity in the experimental conditions. If the stimulus is not strong enough to either paralyze or kill the cell in a short time, those changes which have been described as characteristic of fatigue were seen to occur in the nucleus and could be followed step by step. The result of direct observation gives, therefore, a full confirmation of the previous conclusions. At the same time a number of new and interesting facts were incidentally observed. The reactions of the cells were in general similar at all seasons of the year. In some cases (2) neither nuclei nor nucleoli of the nerve-cells could be seen, this peculiarity occurring both in winter and summer frogs. On stimulation the nuclei became faintly visible, but no nucleoli were observed. In some instances the cytoplasm became lighter and clearer and oil-droplets tended to disappear as the result of stimulation, but in the living cell vacuolation was not evident.

During stimulation some of the granules in the nucleolus were extruded into the nucleus, and very slight changes in the constitution of the nutrient fluid used for irrigation produced startling results. The addition of .1 per cent of potassium tartrate to the solution of sodium chloride and calcium phosphate caused movements in the nucleolus; movements which were apparently amœboid. Under this treatment the nucleolus of the control cells only changed slightly in size, whereas the fragments of the stimulated nucleolus had all disappeared in thirty minutes. These facts are sufficient to show that the physiology of the nerve-cell is a suitable field for further study.

Beiträge zur Kenntniss des Reichthums der Grosshirnrinde des Menschen an Markhaltigen Nervenfasern. THEODOR KAES. Archiv f. Psychiatrie, Bd. XXV. Heft 3.

Ueber die Markhaltigen Nervenfasern in der Grosshirnrinde des Menschen. THEODOR KAES. Neurologisches Centralblatt, No. 11, 1894.

The former of these papers is the more elaborate and the latter contains a corroboration of the observations there described. The investigation concerns the differences in the thickness of the entire cortex and its several layers in a youth of 18 as compared with a man of 38 years, attention being given to the several strata of tangential fibres and especially to those in the outer half of the cortex. The material consisted of both hemispheres of the youth and the right hemisphere of the man. A large number of samples from every portion were taken, and the sections were stained by Wolter's method, which renders the medullated fibres black on a yellow ground. A naked-eye comparison of the sections thus prepared showed that nearly twice as many yellow, and one half as many yellow-gray and gray sections belonged to the youth, as to the man. Thus in a general way the cortex of the youth was found less blackened and consequently less well medullated than that of the man. The total thickness of the cortex was greatest in the man. The measurements taken at the summits of the gyri and the sides and bottom of the sulci show that the most marked increase in thickness occurred at the summit of the gyri. The convex surface of the brain underwent least increase, while the ventral and median surfaces showed the greatest change, that in the latter being most marked. The several fibre-layers do not exactly follow these changes in the total thickness; the outer layers of tangential fibres being most increased in thickness on the convex surface of the hemisphere where the cortex has gained the least in total depth. The most marked development of fibres in the man was found in the motor regions on the convex surface and in the temporal and occipital lobes about the centres for hearing and sight respectively. Particularly poor in fibres are the interior portion of the frontal lobes and the insula. Interesting is the observation that neighboring samples of the cortex may be quite differently developed. These facts, taken with observations by Vulpius along the same line, show a long-continued growth in the human cortex, a growth which quite escapes detection in the gross weighing of the encephalon and its parts, and yet one which certainly is in progress during the process of formal education, and can perhaps be influenced for good or ill by training. In itself the increasing medullation of the fibres is taken to mean a better organization of cortex by an increase in the functional connection between the cells, there slowly developing.

H. H. D.

The Microscopical Examination of the Human Brain. EDWIN GOODALL. London, Baillière, Tindall & Cox, 1894. Pp. 186.

This little work is intended to give us a complete review of the methods of microscopical examination of the brain.

G. treats successfully the fresh method, the injection of cerebral blood-vessels, the hardening methods, fixation methods, imbedding, infiltration and section cutting, the staining methods, hardening combined with staining, the clearing agents, and mounting media. He then adds an outline of the general plan of procedure in microscopical examination of the brain. The appendix contains the methods for museum specimens, and a valuable schedule of the equipment of a laboratory such as is needed in hospitals. A scheme for the *post-mortem* record is added, but no description of any special method to be followed in dissection.

Like most of the books of this kind it offers nothing from the point of view of a rational chemical or empirical explanation of the course of reasoning that led to the adoption of the rules given. We have little more than a collection of prescriptions before us, with condensed in struction for their use.

The compilation of methods is very complete. The little work will not fail to have an important place in every neurological laboratory. It is to be hoped that the author will furnish soon a second part, containing the methods for examining spinal cord and nerves.

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IDIOCY AND IMBECILITY.

L'idiotie et l'imbécilité au point de vue nosographique. PAUL SOLLIER. Archives de Neurologie, vol. XXVII. 33–38.

Recherches cliniques et thérapeutiques sur l'épilepsie, l'hysterie et l'idiotie, etc. Compte rendu de 1892, vol. XIII. BOURNEVILLE. Paris, Bureaux du Progrès Medical, 1893. Pp. cxii + 364.

L'idiotie, hérédité et dégéneréscence mentale, psychologie et education de l'idiot. JULES VOISIN. Paris, F. Alcan, 1893. Pp. 295.

Zur Ätiologie der Idiotie. HERMANN PIPER. Berlin, Fischer, 1893. Pp. 207.

Of late years the literature of idiocy has concerned itself chiefly with questions of classification, the differences of the different authors arising as usual from their various points of view. The first and perhaps the most important question at issue is whether idiocy is a condition distinct from imbecility or whether they are both merely degrees of one and the same affection. The latter is the view held by most writers in the past and at the present day, and it is to combat this that M. Sollier presents the paper mentioned above, supporting with clinical and pathological evidence a position he has already taken on psychological grounds in his well-known book 'Psychologie de l'idiot et de l'imbécile.' * In that work M. Sollier argued that idiocy and imbecility are, psychologically and socially, two distinct states, being united by a single common factor, viz., that their intelligence is inferior to the normal. Further, that if the idiot is to be considered as an

* Bibl. de philosophie contemporaine. Paris, Alcan, 1891.

inferior individual, incapable of living independently in society, and if the epithet 'extra-social' applies to him for that reason, then that of 'anti-social' should be given to the imbecile, an individual who is not only incapable and useless but even a menace and dangerous to society. In the paper before us, as has been said, M. Sollier approaches the question from its medical side and, to be brief, lays down the proposition that idiocy is not a disease in itself but a symptom; a position that would be accepted, perhaps, by many of his opponents without demurring, but with the addition that if idiocy be only a symptom, then imbecility is but a lighter degree of that same symptom: but here M. Sollier vigorously objects, holding that imbecility is a distinct disease, thus establishing at once a radical difference between the two states.

In support of his position he urges that all idiots present cerebral lesions, while imbeciles do not. One could wish that M. Sollier's evidence were more exact and conclusive, especially regarding the point last made; as it is, his argument while clever is not convincing.

Among the chief opponents of the last writer must be reckoned Bourneville, who, however, in the excellent record before us of the year's work in the Bicêtre does not touch on theoretical questions, but confines himself to clinical and pathological descriptions of the cases under his charge, a record that might well serve as a model to other alienists in similar positions.

Approaching the question of classification of different forms or degrees of idiocy, we find here also a wide divergence among authorities according to their basis of classification, e.g. etiology, symptomatology, or pathological anatomy. Voisin, like most authors, making no radical distinction between idiocy and imbecility, presents four categories depending on the origin or degree of development of the mental weakness:

1. Idiocy, complete and absolute, may be congenital or acquired. This form is incurable.

2. Idiocy, incomplete, congenital, or acquired; capable of amelioration.

3. Imbecility, congenital or acquired; characterized by the rudimentary presence of all the intellectual faculties; by the degeneration and instability of these faculties.

4. Mental weakness (*débilité mentale*), characterized by feebleness or lack of equilibrium of the faculties. Voisin further finds two degrees in his first class, viz., those idiots who possess the instinct of '*conservation*,' and those in whom that instinct does not exist.

This classification is open to perhaps fewer objections than any we have yet seen, and is one of many good points in a book that as a whole deserves the warmest praise as a clear and succinct description of the field. The author's account of the special and general sensibility of idiots may be mentioned as particularly suggestive, as well as the treatise at the close of the book on the best methods of education for this class of unfortunates.

Of quite another type is Herr Piper's report of his researches in the etiology of idiocy. Fortunate in his material, he has made an important contribution to the statistics of the subject. We may note in passing his classification, not altogether happy, depending upon the presence or absence of convulsions, and distinguishing further a congenital and an acquired form of idiocy. Of 416 cases under his charge 310 were congenital and 106 acquired, while 70% are described as having convulsions. Of all his patients 32% were first-born children. As to the causal circumstances in congenital idiocy : in 23% there was a history of pulmonary tuberculosis in the parents ; in 14% of mental disease of one kind or another in parents or immediate relatives ; while in 17% no cause could be advanced. In 10% of the cases the father was a drunkard. Consanguinity of parents could only be shown in 3%.

In acquired idiocy the infectious fevers are by far the most fruitful cause. The proportion of idiotic boys to girls was nearly two to one (276 : 140), a difference that could not be due to accidental causes alone. Open to error as all such asylum records are, Herr Piper has nevertheless presented probably the most important single statistical study in this subject of the last decade.

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THE PERCEPTION OF TIME.

Beiträge zur Psychologie d. Zeitsinns. ERNST MEUMANN. Philos. Stud., VIII. 431–509, 1892 ; IX. 264–306, 1893. To be continued.

Untersuchungen zur Psychologie und Æsthetik des Rythmus. ERNST MEUMANN. Philos. Stud., X. 249–322, 393–430, 1894. To be continued.

These two articles, already filling nearly 250 pages and running into three years of publication, are neither of them yet completed. The first instalment (79 pages) is a review of contemporary workers in the field chosen : Torkelson, Münsterberg, Schumann, Nichols. M. commends Torkelson for raising the still open question in time-experimentation : How shall we compare judgments taken from different stages of improvement and practice ? M. finds Münsterberg's measurements careless and shallow. Rejecting the latter's 'muscle' hypothesis, M. advocates, and I think rightly, that every psychic process is a competent basis of time-measurement. Some processes serve better than others—sound better than muscle, and muscle better than eye—"Münsterberg's doctrine being but an exaggeration of this fact." The remainder of Part I is devoted mainly to Schumann. Schumann's apparatus is mechanically perfect, but too difficult of manipulation to admit of adequate range of experimentation. Schumann's work must have contained errors from improper use of telephone, mercury contact, etc., and with his method of computing results M. finds much fault. Hence Schumann's sole products, as judged by M., are "obscurities, indecisive conjectures, and everywhere gaps patched up with great trouble and gratuitous assumptions." Relative to Schumann's main theory, that waiting and surprise are the crucial elements in time-judgment, M. declares these latter to be merely disturbance-phenomena which accompany uncertain judgment, and which vanish as accuracy is reached. The temper of M.'s criticisms is exampled by the following: "Schumann either must hold to the absurdity that we compare surprise and waiting, or he must admit that with judgments based upon (*stützt*) waiting and surprise there can be no talk of comparison. Upon these logical grounds, therefore, the whole theory (of Schumann) falls immediately to complete nonsense." My own work on Time—that presented for my doctorate—M. disposes of in a single page, wherefore I esteem his good sense, I now holding it to be of no value, and wishing it had never been written.

The above, then, is 'The Present State of the Time Problem,' as reported by M. Nothing but misconception, carelessness, and nonsense! It is difficult to do justice to such an author. Throughout we are grateful for his extremely valuable opinion. The temper displayed toward Münsterberg and Schumann is, however, deeply to be regretted, since it robs his own words of judicial weight. We are inclined to believe that M. would have made his papers stand among the best in psychological literature, had he burdened himself less with unworthy feelings, and given his great intelligence more appreciative scope.

We now come to the second instalment, containing an account of M.'s own experiments done in Prof. Wundt's laboratory at Leipzig. These 43 pages are the most valuable in the parts yet printed. M. first investigated as to whether an insistently-repeated perception of small difference results in inability to perceive any difference, or inability to perceive no difference—a point too nice, it would seem, for M. to have made entirely clear. In 1891 M. began to study the curve of sensibility for empty intervals (.05–8. s). The experiments were abandoned because of three sources of error: inclination to give set answers; power of subject to make small differences seem 'longer' or 'shorter' at will; tendency of subject to imagine beats or rhythms in series of sounds, which tendency made the intervals seem of variable lengths. These errors led M. to believe that we judge short intervals (below 5 s) in an entirely different manner from long ones; that with the former, the succession of strokes is the object of attention, while with the latter the attention is directed to the conscious processes lying between the strokes. M. holds that the time-content comes to cognition in quite another form in the one case than in the other; he says we judge a rhythm as a whole, and in consequence, time-memory of the preceding or repeated interval plays no part in judging short intervals. These are the most important suggestions contributed in M.'s papers, and are likely to prove significant in future experimentation upon the time-problem. After the above M. turns to the study of rhythms. He gave his subjects series of 50 sharp sounds followed by 50 lighter ones. The intervals bounded by the heavier strokes seemed shorter. M. explains by saying that heavy strokes fuse, and make the series seem less continuous and therefore shorter than do dull or soft strokes. As an explanation this is surely surprising. M. found that a strong stroke, introduced into a series of weaker ones, made the interval preceding it appear shorter than the others, and the succeeding one longer. In the main experiments Wundt's new time-apparatus was set to give comparison of two intervals of equal lengths (about $\frac{1}{3}$ s) limited by strokes of different intensities or, in other words, to give rhythms of equal intervals but different accents. Studies were made with the accents in every possible combination for the limiting strokes. M. claims that throughout, the intervals bounded by the intenser strokes seemed the shorter. This opinion seems to be a little forced in such cases as those where a single accent falls on the second stroke (1 2′ 3), with the result that the interval between 2 and 3 seemed the shorter. M. is obliged to reason here that the unaccented 3, coming after the accented 2, seems abnormally weak, and so gives the second interval the appearance of being bounded by weak strokes. We fear that this is the old phantom-psychology—that the act of comparison, set up for explanation, is more of a mystery than the thing to be explained. M. modifies his experiments by placing hammers at unlike distances from the two ears. The gist of the whole work is that change of intensity has influence upon the time-judgment as well as does the length of the time-interval itself. The point raised is a good one, but we feel that M. has not entirely disposed of it.

The two instalments at present given us of the paper on Rhythm are chiefly historical, and are valuable for their bibliography. In Chap. I. 'General Theories of Rhythm' are reviewed (Moritz, Scherer, Benecke, Darwin, Spencer, Schlegel, Schopenhauer, von Hartmann, Köslin, Lotze, Fechner, Herbart, Zimmermann, Mach, Horwicz, Wundt); Chap. II, General Theories from Musicians (Hauptmann, Westphal, Riemann); Chap. III, Rhythm of Spoken Verse (Minor, Paull, Brücke); Chap. IV, Beginnings of Experimental Investigations of Rhythm (Brücke, Hensen, Pipping, Boeke, Wagner and Vietor, Rousselot, Ebbinghaus, Müller and Schumann). In this last chapter are valuable descriptions of modern apparatus for investigating vocal sounds and speech.

M.'s literary style is one that does him the greatest injustice. What is said in 250 pages could be better stated in 50. The different topics are mixed up and strung along with such obscurity and confusion that only with the greatest patience can one discover the author's intended meaning. The work is scholarly, and on the whole the best that has yet appeared in this field. No one can read it without profit or without appreciating the tremendous zeal and patience with which it has been produced. Yet in these days of expectant search for inmost psychological truths one must be disappointed to find that Meumann's theory of time-psychology nowhere gets beyond the notion, that perception of time-content is an ultimate and irreducible fact; or at best gets no further than a disturbing suspicion that perhaps it is a process in some way based upon attention. We repeat that the experimental results are the most valuable part of the author's contributions, as perhaps was to be expected, coming as they do from a laboratory to which, at present, experimental psychology owes a greater total obligation than to any other.

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EXPERIMENTAL.

Influence de l'age sur la mémoire immédiate. B. BOURDON. Rev. Philos. xix. 148–167. 1894.

Bourdon reports a series of experiments made upon one hundred and four Lycée pupils between the ages of eight and twenty. Series formed respectively of digits, letters, monosyllables, dissyllables and trisyllables are distinctly pronounced and the pupils required immediately to repeat them. As is usual in such experiments, precautions of various kinds are employed to prevent disturbances from rhythm, rhyme, trains of habitually associated ideas, etc., the aim being to secure for memorizing presentative elements which possess as nearly as possible equivalent tendencies to fresh associations.

Bourdon finds (1) that between the ages eight and fourteen the 'immediate' memory grows rapidly in power. From fourteen to twenty the growth is almost imperceptible. (2) Not more than six or eight digits can be memorized in this way. For words the limit is five or six. (3) Under the age of fourteen, digits are most easily memorized. After that age, no essential difference appears in the various series. (4) The results show an unquestionable (?) connection between the general intelligence, as estimated by the teachers, and the power of memory. (5) Memory of order as distinct from mere memory of the individual presentation is clearly shown. (6) Series in which repetitions occur seem more difficult to memorize than others.

Not to mention similarities to other investigations of recent date, Bourdon's work, both as regards purpose and method, is strikingly akin to the more extensive and, in most respects, more carefully executed work of T. L. Bolton,* yet there is not even a suggestion that the problem in hand has ever been attacked. Bolton agrees with point (2) and of course with (5). He disagrees with (3) and states (1) in a much more conservative way, besides reaching a number of other conclusions. It is thoroughly regrettable—to put it no more strongly—that experimentation of this kind should be undertaken with so little reference to what has already been accomplished.

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The Relation of the Interference to the Practice Effect of an Association. JOHN A. BERGSTRÖM. Am. Journ. of Psychology, vi. 433–442. 1894.

Mr. Bergström continues his study of the interference of different lines of association. He formulates his specific problem as the "relation of the interference to the practice effect of an association." Suppose that a given series of stimuli, as 1, 2, 3 ... 8 is associated first with one set of psychic processes, *a*, *b*, *c* ... *h*, and later with another set, *i*, *j*, *k* ... *p*; and then let Series I. (the association of 1, 2, 3, etc., with *a*, *b*, *c*, etc.) be repeated : what, if any, has been the disturbing effect of the intervening series ?

Mr. Bergström's experiments were directed to the determination of one of the three possible hypotheses : (1) that the practice and interference effects simply cancel each other; (2) that both influences remain in some constant relation ; (3) that this relation is an occasional and variable one. The experiments are on the general plan of those already

* *Memory in Children*, Amer. Jour. Psych., IV. 362.

reported;* the material consists of a pack of eighty cards, containing outline-pictures, sorted into ten piles in such a way that ten arrangements are possible, and of packs of comparison-cards containing different pictures or printed words.

Mr. Bergström's conclusion, in opposition to that of Münsterberg † and of Müller and Schumann, ‡ in experiments to which reference is made, is "that under the simple conditions of this experiment, the interference-effect of an association bears a constant relation to the practice-effect, and is in fact equivalent to it."

For details of the work Mr. Bergström's paper must be consulted, for it is too skilfully condensed to lend itself readily to further abbreviation.

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Les actions d'arrêt dans les phénomènes de la parole. A. BINET and V. HENRI. Rev. Philos. XXXVII. 608–620, 1894.

Messrs. Binet and Henri describe some experiments on the time-measurement of speech, which have an important bearing on the subject of inhibition. They find that the time required to utter a syllable depends, in addition to its phonetic value, (1) on its position in the sentence (at the beginning, middle, or end), and (2) on its inflection, as marking the rhythm or meaning of the sentence. In pronouncing the numerals from one to ten, as the rate of speed was increased, the intervals between the numbers were diminished by about one half, and the numbers themselves by about one third, excepting the last, which remained the same length. When the numbers were grouped, the last number of a group was always appreciably longer. It would be interesting to repeat these experiments for English and German observers, and also to investigate the difference between vowel and consonant endings.—In another series the subject uttered the numerals rapidly, and stopped at a signal; this was compared with a series in which a sound was prolonged till a given signal, when another was uttered in its place. In the latter case the time needed was substantially the same as the reaction-time for speech (220–260σ for the numerals), while the time for *arresting* a sound was considerably longer (averaging 340σ), and depended on the phase of utterance, or of interval between syllables, in which the signal was given.

An investigation of the effect of speech on respiration indicated a slight rise in the curve at the outset, while the utterance itself was accompanied by exhalation. We would suggest that the former effect

* Am. Journ. Psych., V. 3. † Beiträge, Heft IV.

‡ Zeitschr. f. Psych. u. Phys. d. Sinnesorgane, Bd. VI. 2 & 3, p. 173.

may be due to the muscles of the diaphragm contracting in order to expel the air forcibly, as speech requires, rather than to any actual inhalation.

H. C. WARREN.

PRINCETON.

Beiträge zur Theorie der psychischen Analyse. A. MEINONG. Zeitsch. für Psychol., VI. 340–385, 417–455.

These subtle and elaborate studies, occasioned by an article of H. Cornelius (Viert. f. wiss. Phil., XVI and XVII), treat of a succession of more or less connected questions respecting the presuppositions, difficulties, range and essential nature of the common analytic process by which some element in a complex mental presentation is 'brought out' or 'emerges.' There are two all-important presuppositions of ordinary thought in the matter which need to be tested. First, as to the presupposition of *active* analysis: is it really analysis, a disclosure of parts that were present before, or does the analytic process *change* the content and *create* the new-found elements? The uncritical person holds the former, but he is thinking of the permanence of the stimuli. Memory, recognition, and, in the clearest cases, comparison of the sensation before and after analysis, testify also (so far as they can) to the pre-existence of the disclosed element. The question is decided when we inquire *how* a change of content could be brought about. It could only be because the stimuli of the elements successively disengaged by analysis are at first interfering with each other and producing fused effects; then the analytic process in turn isolates each of the different stimuli by paralyzing the others, and thus leaves it free to produce its proper result. But in the typical case of perception this seems impossible; for how could the analytic activity, while the sense-organs were intact and the paths of conduction open, exclude now this stimulus, now that, from entrance to the central organ? Besides, a fused content would present no distinct point of attack for the analytic operation. Further still, the probable *continuity* of the change brings special difficulties of its own. Thus we are thrown back on the other alternative, that the extricated elements were really there before and that analysis merely brings them under the light of the judging faculty, into the sphere of knowledge. Second, as to the presupposition of *passive* analysis, that is, of cases where some element 'stands out' of itself and is distinguished from the remainder. Is an element in a complex mental whole modified in content by conjunction with other elements? For example, where the conjunction establishes (*fundiert*) a new content, are the original components altered? No, the superinduced content leaves the first combining elements standing.

Taking the case of *timbre* and partial tones as typical, this may be set up as a general rule. Both, then, of the presuppositions of current reflective practice in this matter are justified.

The author goes on to maintain that analysis does not mean the taking of a thought to pieces; against Stumpf, that it is not 'the perception of plurality'; that it is not even knowledge or judgment, though it leads to them. The sphere of mental presentation is wider than that of judgment ; judgment occupies the centre of consciousness, bare presentation the periphery. Every individual has but a limited judgment-capacity. Now what tends to bring a presentation within the judgment-circle ; what are the factors of analytic attention ? First, certain traits of the content : its intensity, certain kinds of quality, medium degrees of simplicity or complexity. Then traits not of the content : perception rather than imagination, Höffding's 'quality of familiarity,' intensity of the act of presentation,—and a factor entirely beyond the realm of *das Vorstellen*, what may be summed up as 'interest.' In virtue of these factors variously conjoined, each presentation has a certain general tendency to be drawn into the judgment-sphere which may be called its 'weight.' Weight is relative and competitive, not absolute. The content within the judgment-sphere when it is *discontinuous* and *articulated* may resolve itself into a plurality of lesser judgment-tracts, each marked by a special relation of its parts—another case of a superinduced content. In general psychic analysis may be defined as the contraction of the judgment-sphere by active augmentation of weight ; or, since the process involves a contraction of the whole span of consciousness, it may be called *concentration*. Into the author's long-drawn discussion of the relation of analysis to the inner articulation of the judgment-sphere, into his appendix on the analysis of sequences, and the many minor but always elaborate ingenuities of the paper, we cannot follow him here.

The articles are marked by fairness, patience, and power of reasoning. But they furnish an almost classically finished instance of the way in which the preoccupation with categories and classifications and blank forms of argument—an over-elaborated machinery of method—may dull an investigator's mother-wit and coarsen into clumsiness his natural tact of treatment. The cogs and wheels, the grinding and pulverizing appliances set at work, revolving with the slow unavoidable creaking of the author's style, impress us only until we open the machine and find that there was nothing inside to be ground. The stuff to be dealt with had gradually sifted out by unobserved cracks as the jarring motion began. Herr Meinong's handling of the momentous question whether analysis finds or makes what it discovers, compared for instance with Professor James's searching treatment of the same subject, is curiously unreal. His assumption of uniformity in the phenomena, his suggestions about the 'stimuli' and the difficulty of supposing them to be 'paralyzed' by the analytic activity, betray a strangely deficient sense of the complex delicacy and fluctuation of cerebral and mental life. His uncritical appeal to memory and comparison shows that elaborateness does not always mean care. And his conception of judgment and mere presentation as two irreducibly different grades of consciousness,—analysis consisting in dragging presentations before the judgment-seat,—congenial though it is to certain current German formulas, lies none the less exposed to the unanswered objections of those who regard 'judgment' as analyzable into terms of presentation.

D. S. MILLER.

BRYN MAWR COLLEGE.

THE PERSONAL AND SOCIAL SENSE.

The Meanings of Self: the Reality of Self. F. H. BRADLEY. Chaps. IX–X of the work 'Appearance and Reality.' London, Swan Sonnenschein & Co; New York, Macmillan & Co., 1893.

Mr. Bradley distinguishes eight meanings of 'Self.' He criticises them all and finds the following outcome. Nowhere is there any content of consciousness which is consistently and always called 'Self.' There is the anthropological self, a cross-section of consciousness, Hume's bundle of present states—which changes of course. There is the organized self of thought which proceeds upon ever new materials of organization. There is the quasi-permament self of memory and personal identity: but what is it that is permanent? There is the sentient self which finds itself subject to the contrasts, fluxes, relativities of feeling, and so on. The actual process of reflection on self is depicted by Mr. Bradley in an analysis which is wonderfully acute and obviously true; a landmark, I think, in the history of that enigma, the so-called 'rational subject.' He depicts a perpetual ego-play of content-elements over against one another in their relation of subject and object. At one time a certain arc in the trajectory of consciousness assumes the rôle of self over against another arc which it takes for its object. Then, at another time, the ego-section slides further around, so to speak. But however long you chase it, it is always part of the trajectory, part of the content—the ego is; and the object is another part. And the unity which contains the whole play, this is the only unity there is. It is a unity of feeling. Always, there is a *fundus* of feeling. This ego-play I find to be very truly described: try as one will to reflect on self, he finds a content—that which is at that moment claiming to be the subject—setting itself over against another content and calling it 'me': and just as soon as one tries to find out what this subject-content is, he is able in a measure to do so; which means that that content has now taken the place of the object-content and so is no longer I, but has become me. And all the time there is a 'feeling' of the whole play, and of the background, as itself upholding the I and linking it into some kind of unity with the me.

The same analysis holds, says Bradley, also for the 'active' self—the self of volition and desire. It seems possible to turn upon any element in the self that desires, and desire *it* to be otherwise; that is, to treat it as a not-self upon which the action of the self desiring is to terminate. This leads to a subtle deduction of the sense of self-activity, which is shown to be due to change in content. For example, the I which desires finds in its object new elements of content fit to be included in the me, and by its expansion to include these elements it sets itself over against its former I-elements, thus converting them into objective me-elements. This expansion and shifting of content-elements through which certain constant I-elements are present—this is felt as self-activity. Even when the elements reached out after as fit for I-elements are not explicit,—i.e., when there is no explicit desire,—even then self-activity is felt. This is due, Bradley thinks, to the implicit presence of these elements already in the original I-content, but in such a way that the entire content as a group is inhibited by the explicit elements. The release of this inhibition is then felt as self-activity.

This deduction, it is clear, is capable of either a Herbartian or a Wundtian construction (see notice of Mackensie's paper), and it assumes, with both Herbart and Wundt, conscious self-activity beneath the threshold of explicit desire. With this assumption I do not agree. There is really no warrant for any such kind of self-activity. Consciousness bears witness, on the contrary, to a very clear aloofness of the I-content from both the members of the change of content taking place in a 'me' which is not the object of desire. Note the case of involuntary attention with its distractions, and the changes wrought in the me content by hypnotic suggestion: these have no feeling of self-activity.* Nor has the progress of a purely objective 'train of ideas.' And even in the instance of blind unratified impulse, there is a feeling of 'run-away' in the machinery, of lack of self-implication, which is due not to the implicit presence of the elements which are explicitly present in desire, but to the weakness of another content which is ex-

* Cf. my volume on *Feeling and Will*, chap. XII. §§ 3–6.

plicitly desired. This latter content is inhibited and overcome, and the undesired takes place because of the *reverse outcome* of the same process as that of explicit desire. Mr. Bradley holds the necessity for some content-element ideally held for realization ; but, in saying that after all it may be implicit, he seems to give up his analysis for the sake of accounting for a myth. The idea said to be implicit is really a part already of the old felt content—otherwise there is mere change, not activity—and this felt content maintains itself successfully against the ideal content. Hence the sense of incompleteness, disappointment, relative irresponsibility in such activities, i.e., as saying 'I will not consent,' and consenting. Put in symbols, there seems to be little difference here between Mr. Bradley's view and mine. But he, in fact, finds self-activity felt towards what is not desired ; I rather find activity, largely not that of self, felt toward that which inhibits what is desired. In the concrete cases which psychology actually knows it makes a difference.

This analysis of self-activity—or any other which proceeds upon what Mr. Bradley calls 'the end in the beginning'—shows itself important in relation to the doctrine of imitative development worked out by recent writers. The object of desire, explicit or through habit implicit, is set up for realization. This is what I have called a 'copy for imitation' in my theory, such a copy as an imitative view of volition requires.* It seems then that this citadel of *actus purus*, this fount of originality and unrelated self-determination, is also capable of a natural construction. The pedagogical applications are very important. For 'self-activity' is talked of so freely nowadays as the goal of education—and so it is—that it is well to show that it is after all through imitation that the training process must proceed even in order to make our scholars inventive.

The other chapter of Bradley's—'The Reality of Self'—proceeds to show that in such a shifting self, constructed out of changing content, we have no right to find reality. It is appearance only. This involves the further doctrines of reality, appearance, change, etc., and is too far-reaching for further notice here.

Mr. Bradley's View of the Self. J. S. MACKENSIE. Mind, N. S. III, July 1894, pp. 304–335.

Mr. Mackensie gives an account of the chapter on the Self of Mr. Bradley's book, and criticises it on the score of certain omissions. He classifies Bradley's meanings of 'self' under four heads—the

* See also Royce's paper noticed further on.

'biological,' the 'psychological,' the 'sentient,' and the 'pathological' self—and claims that two other forms of 'self' must be added, called by him the 'epistemological' and the 'ontological' or 'ideal.' The epistemological or transcendental self is the form of the thought-process, the focus at which the variety of experience is brought to unity in thought. It is the Ego of the cogito and is not a matter of content; thus escaping Bradley's reduction of the various selves to particular constructions of content. In psychological terms, I suppose, this self is the function of apperception considered as unifying principle of thought. The other 'self' added by Mackensie is the 'ontological': again the formal principle of unity, but now considered as the unity of reality or completed system—the ideal unity of 'the completely intelligible for the completely intelligent.' Both these points are familiar to readers of Caird.

As to the matters of fact involved, I think Mr. Bradley is not well criticised. The question arises, how does 'form' come to consciousness? If not as content, we have to say, then not at all. But if not at all, then it must be itself a matter of thought-construction. For how can we say 'experience when thought has the form of unity' except by the use of judgment which must go back again to conscious-content for its matter. So the 'transcendental ego' becomes either the Kantian noumenon, or reduces itself to the 'sentient' self of Bradley, i.e., as I would put it, it is a matter of sentient or felt content over and above the presented content of which it is felt to be the form. In this shape it loses much of its mystery and is amenable to the same natural-history treatment as other facts of consciousness. And the 'ontological' or 'ideal' self is subject to the same kind of criticism. If there be no real *ego* discovered in the *cogito*, apart from the felt form of the *cognitum*, then we have no basis for an ideal *ego* discovered in an ideal *cogito* apart from *what we feel* the form of the ideal *cognitum* would be if we were able to apprehend it. Then presupposing absolute reality, with Bradley, the ideal ego will be an absolute sentient ego—an ego which feels its own perfect content.

I do not know whether Mr. Bradley would accept this bald argument to a conclusion near his own. It certainly is much briefer than his. And I am sure that Mr. Mackensie and his master would say, "not a word about 'reason'—which is a 'higher level' than intellect." But of the points still left in current idealism for Mr. Bradley's probing-knife of psychological analysis, this is the most inviting. I believe that reason is feeling, and its ideals are feeling—the onrush of habit and emotion in their own teleological movement beyond the constructions of intellect which they presuppose. This is its nature and history.

And it is Bradley's splendid service to have shown that reality is as much reality when felt as when judged—possibly more, if the pros and cons of the relation of feeling and thought to each other be duly weighed.

The External World and the Social Consciousness. JOSIAH ROYCE. Philos. Review, III. 513–545, Sept. 1894.

The thesis maintained by Prof. Royce in this interesting paper is this: "Social community is the differentia of our external world. . . . A child never gets his belief in our present objective world until he has first got his social consciousness." The arguments presented by the author in support of this view are of two kinds. He first shows that the ordinary so-called tests or criteria of externality are not valid or sufficient inasmuch as they omit the quality of *definiteness.* All things believed to be external are definite in place, dimensions, number, and movement. But what we really mean by definiteness is, when analyzed, *communicableness* to others; what I cannot express to my fellow and ratify together with him—that is not external, but internal. The notion of externality therefore proceeds upon the sense of social relationship or community. Apart from the question of proof, attention may be called to Prof. Royce's acute note on Renouvier's thesis, 'Whatever is must be determinate,' and to the use he makes of the sense of indefinite movement in after-images quoted from Fleischl. In what is said in this part of the paper we have, I think, a very original and interesting contribution to the theory of externality. It lacks, however, detailed criticism of the criteria usually named, i.e., resistance, regularity, involuntariness, etc., of the external world. I myself, for example, do not feel driven out of my view of the 'coefficient of external reality' * earlier worked out, even though the whole account of the social consciousness given by Prof. Royce be true. This appears in the general point of criticism made below.

In the second part of his paper, the author gives a summary of a theory of the rise of the social consciousness based upon the doctrine of imitation, i.e., a theory with which a recent paper by the present reviewer is in substantial agreement, as is intimated by Prof. Royce in an all-too-kindly reference.† The essence of the theory is that the child gets his material for the personality-sense from persons around him by imitation. So that his growing sense of self is constantly behind

* Baldwin, *Feeling and Will,* chap. VII. §§ 4, 5.

† *Mind,* Jan. 1894. On this topic an article by Prof. Royce on 'The Imitative Functions' in the *Century Magazine* for Mar. '94 should be read. It is to be followed by a short paper by the present writer in the same magazine.

his growing sense of others. This conclusion affords the additional argument that it is through this relationship that the antithesis between self and the external is discovered and the community made possible in which the external world finds its differentia.

The one criticism which I would venture to make upon this paper —as attractive in style as thoughtful in content—is that it neglects the phylogenetic point of view, the considerations from race-history. I think the element of social suggestion may be admitted to the full as Prof. Royce argues for it, and yet the conclusion not follow that the child never *would* get the notion of externality without it. No more would I say that the child never *would* get a notion of self without the imitative copying of others which we agree in emphasizing so strongly. Would not the hereditary impulses of thought and nervous action give an isolated babe a pretty good apology for an external world and a self? To say, 'yes, but not the same he has now,' is only to say that the new element is an addition. Certainly it is; but is there no essential moment in externality which is likely to be either there or not there to a child?

I think there is: something in the structure of the developed nervous system. The seeing of space itself seems to mean externality in presented objects: not not-self-ness, of course, but blank, *definite*, awayness—*da*-ness, so to speak. It is the property seen in the nervous projection of stimulations to the periphery. Little chickens seem to have a very respectably *definite* sense of *da*-ness, and this without comparing notes with one another or with the hen! Now this sense of projection may be the essence of external existence *vs.* internal—although the antithesis comes only later and largely by social development—and it may be that the elements even of personal suggestion which the child imitates already have it.* Indeed I think it can be shown that they have. It is on this basis that I give, in my 'coefficient of external reality,' the element which constitutes *this kind of objectivity*, and make the 'objective' stage first even in the child's knowledge of other persons.

An interesting speculation would arise if Prof. Royce should work out the social criterion in the phylogenetic sphere; by applying it, for example, to the quasi-social community of the different senses together—a test of externality strongly insisted upon sometimes. If so, I should ask him how it has come about that a single sense often so

* Cf. my paper on 'Personality Suggestion' in this REVIEW, I. p. 274, May 1894. I am pointing out in my book that there is a period of 'organic' bashfulness in the child's first year—showing a specialized nervous reaction in the presence of *persons*.

strenuously lies to us about externality, in the face of all sense and social testimony, that we have to lie to ourselves, almost, to keep back our belief in it. If it be because this function, say, of this sense is a part of habitual convention and former beliefs which are themselves guaranteed, then that illustrates what I should say was the case with each organism as a whole with reference to other organisms.

I cannot close this notice without mentioning the grace and impressiveness with which this paper was delivered as a lecture before the Princeton Philosophical Club. J. M. B.

The Oxford Meeting of the British Association. Reported in Nature, Aug. 9, 16, 23, 30, and Sept. 6, 1894. *Inaugural Address.* THE MARQUIS OF SALISBURY.

Attention may properly be called in this place to the reports in *Nature* of the recent meeting of the British Association. The address of the President and of the Presidents of the several sections are included in full, and abstracts are given of many other papers. There is no section for Psychology, but papers of considerable psychological importance are often presented before the section for Anthropology and elsewhere. It is impossible even to enumerate the many papers having some reference to psychology, but one of these, the inaugural address of the President, may be selected for notice.

Lord Salisbury departs from the usual precedent and surveys not our science but our ignorance. He selects three subjects for this purpose—the chemical elements, the ether, and the doctrine of natural selection. The chemical elements and especially the ether are difficulties in the way of a mechanical description of the physical world which are suggestive to the psychologist. In the case of natural selection Lord Salisbury seems to think the time limit set by physicists is one of the most serious difficulties. But if Lord Kelvin allow a hundred million years for the existence of organic life on the earth, and Prof. Tait ten million, the probable error is so considerable that the biologist may claim a thousand million years if he need them. As a matter of fact it is geology rather than biology that must seek reconciliation. A hundred million years is a long while, and under favorable conditions evolution may proceed rapidly—witness the mental and social development of man during the past three thousand years. But the point of special interest to the present writer in the address is the contradiction assumed by Lord Salisbury and by so many naturalists of natural selection and design. Prof. Weismann says we must accept natural selection because it is inconceivable that there should be any other explanation without assuming the principle of design. Lord Salisbury accepts this dictum and concludes that as natural selection cannot be demonstrated we must accept the principle of design. But surely natural selection and design are not exclusive. Whether or not one species has developed from another through variations (small or large, due or not due to physical environment) and survival of the fittest is a matter which must be decided by observation and experiment. We may judge from the evidence that the doctrine of variation and survival is a correct or an incorrect account of what has taken place. In either case one may or may not believe in a principle of design. J. McK. C.

NEW BOOKS.

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The Æsthetic Element in Morality. F. C. SHARP. New York, Macmillan & Co., 1893. Pp. 131.

NOTES.

Professor HERMANN VON HELMHOLTZ died in Berlin on Sept. 8.

Professor VEITCH, of Glasgow, died in that city in September.

Mr. D. G. RITCHIE, of Oxford, has been appointed to the chair in Philosophy at St. Andrews vacated by Professor JONES.

Mr. W. J. SHAW, B.A., M.A. (Toronto and Princeton), has been appointed Instructor in Philosophy in Wesleyan University.

Mr. S. F. McLENNAN, B.A. (Toronto), has been appointed Assistant in Psychology in the University of Chicago.

There will be issued yearly, in connection with THE PSYCHOLOGICAL REVIEW, a Bibliography of Psychological Literature, compiled by Dr. Livingston Farrand, Columbia College, and Mr. Howard C. Warren, Princeton University. The bibliography will include, so far as possible, all books, monographs, and articles in psychology, and those publications in philosophy, biology, anthropology, neurology, etc., which are important for psychology. Authors will contribute to the completeness and accuracy of the bibliography by sending to Dr. Farrand or Mr. Warren lists of their publications, with details of title, author, publisher and place of publication (or name of review or archives), and number of pages. The bibliography for 1894 will be issued early in 1895.

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