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# Psychological Review

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# THE PSYCHOLOGICAL REVIEW.

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## PSYCHOLOGY AND HISTORY.<sup>1</sup>

BY PROFESSOR HUGO MÜNSTERBERG.

*Harvard University.*

A few years ago, at the Philadelphia meeting of our Association, the Presidential Address sketched the wonderful progress of our modern psychology and culminated in the statement: "We are past the time for systems of psychology; now handbooks of psychology are prepared." Psychology, indeed, since its declaration of independence, is eager to find out and to collect the special facts, without allowing the traditional interference of metaphysical philosophy, and that which brings us together in our Association ought to remain our common interest in the discovery of empirical psychical facts. And yet I cannot help thinking that many of us who sincerely agree with that enthusiasm for daily use are ready to confess the wish of thoughtful hours that, while handbooks of psychology appear now in masses, the time may come again for systems of psychology. We strive, I think, from the disconnected facts towards a systematic unity, and know that such unity is never reached by even the most complete collection of facts, but only by a philosophical understanding of the fundamental principles of our work. The discussion of the basal conceptions and categories of psychology, of its presuppositions and its limitations, of its relations to other empirical sciences and to philosophy, seems thus still more important and essential than the results of any observation, and the fact that in recent years inquiries in regard to the psychological standpoint

<sup>1</sup> President's Address, American Psychological Association, New York Meeting, December, 1898.

have come everywhere to the foreground of epistemological research appears to point more strongly towards the real progress of psychology than any discovery between the walls of our laboratories. I welcome, therefore, the more, the honorable opportunity of this hour, as I understand that the Presidential Address should emphasize the general problems of our science.

My address deals with the limits of psychology. I know quite well that such a choice easily suggests the suspicion of heresy; whoever asks eagerly for the limits of a science appears to the first glance in a hostile attitude towards it. To emphasize its limiting boundaries means to restrain its rights and to lessen its freedom. It seems, indeed, almost an anti-psychological undertaking for any one to say to this young science, which is so full of the spirit of enterprise: Keep within the bounds of your domain. But you remember the word of Kant: "It is not augmentation, but deformation of the sciences, if we efface their limits." Kant is speaking of logic, but at present his word seems to be for no field truer than for psychology. Psychology, it seems to me, encouraged by its quick triumphs over its old-fashioned metaphysical rival, to-day moves instinctively towards an expansionistic policy. A psychological imperialism which dictates laws to the whole world of inner experience seems often to be the goal. But sciences are not like the domiciles of nations; their limits cannot be changed by mere agreement. The presuppositions with which a science starts decide for all time as to the possibilities of its outer extension. The botanists may resolve to-morrow that from now on they will study the movements of the stars also; it is their private matter to choose whether they want to be botanists only or also astronomers, but they can never decide that astronomy shall become in future a part of botany, supposing that they do not claim the Milky Way as a big vegetable. Every extension beyond the sharp limits which are determined by the logical presuppositions can thus be only the triumph of confusion, and the ultimate arbitration, which is the function of epistemology, must always decide against it. It is thus love and devotion for psychology which demands that its energies be not wasted by the hopeless task of transgressions into other fields.

Philosophers and psychologists are mostly willing to acknowledge such a discriminative attitude when the relations between psychology and the normative sciences, ethics, logic, æsthetics, are in question. They know that a mere description and causal explanation of ethical, æsthetical and logical mental facts in spite of its legitimate relative value cannot in itself be substituted for the doctrines of obligation. The line of demarcation thus separates with entire logical sharpness the duties from the facts, the duties which have to be appreciated in their validity as ideals for the will, and the facts which have to be analyzed and explained in their physical or psychical existence as objects of perception. But can we overlook the symptoms of growing opposition against the indiscriminative treatment of the world of facts in the empirical sciences? The creed of those who believe in such uniformity is simple enough: the universe is made up of physical and psychical processes, and it is the purpose of science to discover their elements and their laws; we may differentiate and classify the sciences with regard to the different objects which we analyze or with regard to the different processes the laws of which we study, but there cannot exist in the world anything which does not find a suitable place in a system in which all special sciences are departments of physics or of psychology. In a period of naturalistic thinking like that of the Darwinistic age the intellectual conscience may be fascinated and hypnotized by the triumphs of such atomizing and law-seeking thought even to the point of forgetting all doubts and contradictions. But the pendulum of civilization begins to swing in the other direction. The mere decomposition of the world has not satisfied the deep demand for an inner understanding of the world; the discovery of the causal laws has not stilled the thirst for emotional values, and there has come a chill with the feeling that all the technical improvement which surrounds us is a luxury which does not make life either better or worthier of the struggle. The idealistic impulses have come to a new life everywhere in art and science and politics and society and religion; the historical and philosophical thinking has revived and rushes to the foreground. We begin to remember again what naturalism too easily forgets, that the interests of



life have not to do with causes and effects, but with purposes and means, that in life we feel ourselves as units and as free agents, bound by culture and not only by nature, factors in a system of history and not only atoms in a mechanism.

Such a general reaction demands its expression in the world of science too, and there cannot be any surprise if psychology has to stand the first attack. The naturalistic study of the physical facts may not be less antagonistic to such idealistic demands, and yet it is the decomposition of the psychical facts which oppresses us most immediately in our instinctive strife for the rights of the personality. The antithesis becomes thus most pointed in the conflict between psychology and history, and it seems to me that only two possibilities are open. One possibility is that these sciences stay yoked together, the one forcing the other to follow its path. Then, of course, two cases may happen. Either psychology remains as hitherto the stronger one; history must then follow the paths of psychological analysis and must be satisfied with sociological laws; every effort of history which goes beyond that is then unscientific, and the works of our great historians must seek shelter under the roof of art. Or—and this second case has all odds in favor of it—the belief in the unity of personality becomes stronger than the confidence in science, which merely decomposes, and psychology becomes subordinated to the historical view of man. That is possible under a hundred forms, but the final result must be always the same, the ruin of real psychology. I think this undermining of psychology with the tools of history is to-day in eager progress. Here belong, of course, all the most modern attempts to supplement the regular analyzing psychology by a pseudo-psychology which by principle considers the mental life as a unity and asks not about its constitution but about its meaning. Whether authors, half unconsciously, alternate with these two views from chapter to chapter, or whether they demand systematically that both kinds of psychology be acknowledged, makes no essential difference. Both forms are characteristic for a period of transition; both must lead in the end to giving up fully the analyzing view, to shifting the results of such analysis over to physiology, and thus to confining psychology

entirely to the anti-causal categories, that is to denying psychology altogether. Such turnings of the scientific spirit are slow, but if history and psychology remain chained up together the symptoms of the future are too clear: there is no hope for psychology.

But there is a second alternative open. The chain which forces psychology and history to move together may be broken, the one may be acknowledged as fully independent of the other. What appears as a conflict of contradictory statements may then become the mutual supplementation of two partial truths, just as a body may appear very different from the geometrical, from the physical and from the chemical points of view while each one gives us truth. To those who have followed the recent development of epistemological discussion, especially in Germany, it is a well-known fact that this logical separation of history and psychology is, indeed, the demand of some of the best students of logic. They claim that the scientific interest in the facts can and must take two absolutely different directions: we are interested either in the single fact as such or in the laws under which it stands, and thus we have two groups of sciences which have nothing to do with each other, sciences which describe the isolated facts and sciences which seek their laws. A leading logician baptizes the first, therefore, idiographic sciences, the latter nomothetic sciences; idiographic is history; nomothetic are physics and psychology. Psychology gives general facts which are always true, but concerning which it has not to ask whether they are realized anywhere or at any time; history refers to the special single fact only, without any relation to general facts.

I consider this logical separation as a liberating deed, not only because it is the only way for psychology to escape its ruin through the interference of an historically thinking idealism, and also not only because the value and unity and freedom of the personality which history preaches can now be followed up without interference on the part of psychology, but because, independent of any practical results, it seems to me the necessary outcome of epistemological reflection. And yet the arguments which have led to this separation appear to me mistaken



and untenable in every respect. I agree heartily with the decision, but I absolutely reject the motives. No antithesis is possible between sciences which study the isolated facts and sciences which generalize; such a methodological difference does not exist. We shall see that it must be replaced by a difference of another kind, but the end must be the same: psychology and history must never come together again. To criticise the one way of attaining this end and to illuminate the other new way which I propose is the purpose of the following considerations.

We must proceed at first critically; what is the truth of the view which contrasts idiographic and nomothetic sciences? At the first glance the importance of the discrimination seems so evident that it appears hard to understand how it could ever have been overlooked. It seems a matter of course that the empirical sciences can ask either about the general facts of reality, the laws which are true always and everywhere and which do not say what happened on a special place and in a special time, and on the other hand about the single facts which are characterized just by their uniqueness. We may be interested in the physical and chemical laws of fire, but our interest in the one great fire which destroyed Moscow has an absolutely different logical source, and if we extend our historical interest from the physical to the psychical side, and investigate the stream of associations which passed during the days of that fire through the mind of Napoleon, we have again an absolutely different kind of interest from that of the psychologist who studies the laws of association and inhibition, which are true for every mortal. How small from a logical standpoint appears the difference between the search for physical laws and the search for psychological laws compared with the unbridgable chasm between the search for laws and the inquiry for special facts which happened once! And this difference grows if we consider that all our feelings and emotions refer to the special single object, not to any laws, that, above all, the personalities with which we come in contact come in question for us just in their singleness, and that we ourselves feel the value of our life and the meaning of our responsibility in the uniqueness of the acts by which we

mark our individual rôle in the history of mankind. These arguments of recent epistemological discussions will easily find the ear of the multitude. Common sense, which demands for itself the prerogative of being inconsistent, will probably hesitate only at the unavoidable postulate of this standpoint, that also the development of our solar system, of our earth, of our flora and fauna, belongs then to history and not to natural science, as they describe a process which happened once, and not a law.

I may begin my criticism at the periphery of the subject, moving slowly to the center. I claim first that all natural sciences, of which psychology is one, do not seek laws only but set forth also judgments about the existence of objects. Of course, we can make the arbitrary decision that we acknowledge the natural sciences as such only so far as they give eternal laws without reference to their realization in a special place or in a special time, while any judgment about the existence here or there, now or then, has to be housed under the roof of history. The sciences as they practically are would then be mixtures of historical and naturalistic statements, the historical factor diminishing the more, the more abstract the science, reaching its minimum in pure mechanics. Such decision has only recently found able defense, but do we not destroy, by its acceptance, the whole meaning of natural science? Are the laws for themselves alone still of any scientific interest at all? Why do we care at all for such general laws, as the law of causality, the most general of them, which embraces all the others, is included already in the presuppositions of science, and thus anticipated beforehand? When formal logic or mathematics deals with A and B and C, they state valid relations without asking whether A, B or C is given anywhere or at any time, even without excluding the possibility that their real existence may be impossible. The scientific judgments of physics and psychology, on the other hand, have lost all their meaning if we deprive them of the presupposition that objects which prove the validity of such laws have real existence in the world of experience.

We can construct well-founded physiological laws also for the organism of the centaur and psychological laws for the mind of nixes and water fairies, but both attempts do not be-

long within the system of science. The claim of existentiality is not explicitly expressed in the formulation of scientific knowledge, not because it is unessential, but because it is a matter of course. The larger the circle for which the law is valid the more we find these included judgments of reality deprived of their reference to special local and temporal data, but even in the most general propositions of mechanics, such judgments are tacitly included. The question is not whether the objects with which the laws of mechanics deal have real existence from a philosophical point of view; certainly they have not. The important point is that mechanics by its laws tries at the same time to make us believe that even the atoms have existence. On the other hand, the existential judgment must become the more detailed the more special the law is, that is, the more complicated the conditions of its realization. If the psychologist states the laws of the feelings, he claims that it is not true that only men without feelings exist; he claims that men with feelings have reality too. If he gives us the more special laws of ethical feelings, he claims that experience knows men with ethical emotion. If he goes on with his specialization of the psychical laws, claiming that under special conditions the ethical emotion of obedience to the state comes to inhibit the desire for life, he tells us that this really happened. His psychological law becomes finally only still more detailed if he lays it down that under such and such conditions obedience to the state discharges itself in the drinking of a hemlock potion in spite of antagonistic suggestions of escape from philosophical friends. It is a psychological law and yet it claims at the same time that all this once at least really happened, while the complication of conditions practically excludes the possibility of its happening more than once in the world of our experience.

Of course, it remains a law of general character with regard to the absolute space and the absolute time; when all conditions including our solar system and all the events on the earth are given once more in infinity, then Socrates necessarily must drink once more the poisoned cup. But in the limited space and time of our experience the conditions for the realization of such a psychological law can have been given only once, and that



they really once were given is decidedly claimed and thus silently reported by the law. If our opponents maintain that the naturalistic sciences need as supplement a historical description of one special stage of the world to give a foothold for the working of the eternal laws, we can thus reject this external help for the explanation of the world, as the laws themselves furnish all that we need. The system of the laws is at the same time a full and graduated system of existential propositions with regard to the limited space and time of our experience. If ever and anywhere in the empirical universe a molecule had moved otherwise or another thought had passed through a consciousness, then the system of laws, thought in ideal perfection, would have demanded a change. Our physics and psychology presuppose and assert the real existence of exactly our world. They do not seek the laws with the intention of neglecting and ignoring the special facts.

The separation of the single facts from the general facts is thus untenable, because the explanatory law includes the description; but we can also emphasize the other side of this mutual relation: every description includes explanation, every assertion of a special fact demands reference to the general facts. A description has a logical value only if it points towards a law. We describe a process by the help of conceptions which are worked up from the general facts, common to a group of objects, and these general conceptions are the more valuable for the purposes of description the more their content is a condensed representation of real objective connections. The descriptions in popular language make use of conceptions which are deduced from superficial similarity, but every new insight into the physical and psychological laws gives to the general conceptions a more and more valuable shape. The history of science is the steady development of the means of description; there is no description which by its use of conceptions does not aim at working out the laws. Thus, far from the trivial belief that the law is merely a description of facts, we ought not to forget that the description of facts involves the laws and is only another form of their expression. To describe a physical thing as a group of atoms or an idea as a group of sensations demands the whole knowledge

of the psychological and mechanical laws and condenses in its conceptions the progress of science. To separate the descriptive report from the explaining apperception is thus again impossible.

It could appear that this does not hold for all kinds of description; we communicate with one another in practical life without relying on general conceptions. But our communication then is no description. Any mode of personal expression, gestures or tears, may tell us what is going on in the mind of another without reference to psychological laws. But the fact is that they give no description either; they give a suggestion. The words of practical life, the words of the poet and, as we may add at once, the words of the historian, work like such movements of expression; they make every mental vibration resound in us, because they force us unintentionally or with conscious art to follow the suggestion and to imitate the mental experience. The rhythm and the shade of the words may then be substituted for logical exactitude, and interjections may have deeper influence than complete judgments, but all that is decidedly no description, as a description demands a communication of the elements. Such a suggestion allows us an understanding of the meaning, but gives us no knowledge of the constitution. Where a single object really has to be described, there conceptions and laws are inevitable, and the historian cannot make an exception.

But just this fact, that description and explanation cannot be separated and that the conception includes the law, has opened in recent philosophical discussions a new way of thought which also seems to lead to those claims which we rejected. Granted, it is said, that every description presupposes generalizing abstractions, but such abstraction must then lead us away from the endless manifoldness of the reality. Every scientific description deals with physical or psychological abstractions; does that not mean that we need still another kind of treatment which does justice to the existing richness and fullness of the real single fact? If we give this mission to history we acknowledge that its communications would not be ordinary descriptions, but we should in any case again have the separated camps with the



antithesis : Manifoldness and abstraction, single fact and general fact. But the presupposition is wrong ; the manifoldness of the reality is not endless and the abstracting conceptions are not at all unfit to do justice to the richness of the single fact. The single conception abstracts, but the connection of conceptions in the sentence reconstructs again. On the other hand, whatever is the possible object of perception and discrimination must be the possible object of descriptive determination. Whether the task of a complete conceptional description is difficult or not is no question of principle ; impossible it is not. The ability to perceive differences is even inferior compared with the power to separate the differences conceptionally, and the abstracting description of science must, therefore, frequently increase and not decrease the manifoldness of the object. We know about the objects more than we perceive ; above all, the description can never leave behind it a perceivable remainder which from its too great manifoldness excludes description. The full variety of the single facts thus belongs just as much as the most general law to the physical and psychological sciences ; the antithesis psychology and history as coinciding with the antithesis abstraction and manifoldness of reality is then impossible. That history stands, indeed, nearer to reality than any psychology we shall later fully acknowledge, but, as we shall see, for very different reasons ; history abstracts, we shall see, not less than psychology, and psychology is interested in the variety of the facts just as much as is history.

This brings us to our central arguments : Every science considers the single facts in their relations to other facts, works towards connection, towards generalities. Science means connection and nothing else, and history also aims at general facts, or it cannot hope for a place in the system of science. Does that mean that it is valueless to consider the single fact as it stands for itself, isolated and separated from everything else ? Certainly not ; the isolation is not less valuable than the connection, but it never forms a science ; it is the task of art. The single fact belongs to art and not to history ; history has to do with the general facts. That is the thesis which I must interpret and defend. One point, of course, is clear before the dis-

cussion. If we maintain that history has also to work up its material with respect to the general facts and not with regard to the single facts, then it is evident that there is in the deepest principle of the inquiry no methodological difference between physics and psychology on the one side and history on the other. But we insisted that an important difference does exist. The difference must then be not in the kind of treatment, but in the material itself. To be sure, there cannot be a physical or psychical object in the universe which would not be possible material for psychology or physics; if history deals with a material which is different from the possible objects of those empirical sciences, then it must deal with facts which differ from the physical and psychical objects in their kind of existence; in short, the difference between psychology and history is not a methodological but an ontological one.

We must then ask what kind of existence belongs to the material with which physics and psychology deal and how it is related to reality; above all, how far reality offers still another kind of facts which could be the substance of other sciences. Reality means to us here the immediate experience which we live through. This immediate truth of life may be transformed and remoulded in theories and sciences, and these remodelings of reality may be highly valuable for special purposes of life; we may even reach finally a point of reconstruction from which the subjective experience appears as an illusion and the supplementation stands as the only truth. Yet the importance of such constructions must not make us forget that we have then left reality behind us. Our doubting and remoulding itself belongs to the reality for which its products can never be substituted. And this primary reality can, of course, never be reached when we start from the finished theories of the physical or psychological sciences. Whether we pretend that the world is a content of our consciousness, a system of psychological ideas, or whether we start from the mechanical universe and consider experience as effect of the outer world on the consciousness, or whether we confuse the two and call the world a product of the brain, it is all equally misleading if we seek the reality, as each view presupposes equally the psy-

chological or physical constructions. It is then, of course, for us also impossible to reach the less remoulded primary experience by going backward through the genetic development of the individual or of the race to an earlier simpler stage of experience. Just this genetic tracing backward fully presupposes the categories of the psychological view; we must have already considered our own inner life as a complex combination of elements before it has a meaning to call the mental life of the child or of the animal less complex; the starting point of the genetic development is thus itself an artificial construction which lies further away from the primary experience.

If we thus escape all theories and stand firm against the suggestions which psychology and physics plentifully bring to us, then we find in the reality nothing of ideas or of mechanical substances, neither consciousness nor a connected universe. The reality we experience does not know the antithesis of psychical and physical objects, but in the primary stage merely the antithesis subject and object. We feel our personal reality in our subjective attitudes, in our will acts which we do not perceive but which we live through, and with the same immediacy we acknowledge other personalities as subjects of will. They too are not objects which we merely perceive, but we acknowledge them, by our feeling, as subjects with whom we agree or disagree and whose reality is thus not less certain than our own. Our acts as subjects are directed towards objects which in reality exist only as such objects of will, that is, as values. They are our ends and means, our tools and purposes, and nothing is to us real that is not called to be selected or rejected, to be favored or dismissed. Subjective acts of will and objects of will form the reality, the whole reality, nothing lies outside and nothing is valid beyond this world of will relations, and even if we form judgments about objects which we think as independent of the will, this judgment and this thought itself is an act of will working towards a purpose.

As soon as we begin to bring order into the manifoldness of this real world the subjective acts as well as the objects divide themselves into two groups, those of individual character and those which are common to all, over-individual. This division



means not a result of counting whether several subjects or by chance only one subject have made the decision or appreciated the object: it is a question of intention merely. My act is overindividual if it is willed with the meaning that it belongs to every subject which I acknowledge, and my object is overindividual in so far as I consider it as a possible object of attitude for every subject. My overindividual will-act is that factor of reality which we call duty; every duty lies in us as subjects, as our own deepest will, and yet as more than our individual decision. The overindividual objects are those which we call physical; the individual objects are the psychical ones; we must only not forget that these physical and psychical objects are in reality not in question as independent objects of perception, but are always related to the will; they are not contents of consciousness and mechanical bodies in a continuous space, but suggestions which have a meaning, things which have a use. We find thus four factors of reality, beyond whose validity a constructive metaphysics alone can go. Metaphysics may ask whether the individual and overindividual acts do not blend in an absolute subject and whether the objects are not posited by such a subject of higher order; epistemology must be satisfied with the more modest task of settling how we deal with this reality in our scientific or æsthetic knowledge. Reality itself is, of course, neither art nor science, but life. Art and science must be thus transformations of the material which life offers to us, while these transformations themselves are acts of the subjects and thus belonging to those will-formations which claim for themselves an overindividual character, creating the values of beauty and truth.

The acts which lead from life to art and science are thus for epistemology free acts of that subjectivity which we find in ourselves by immediate feeling, and which we acknowledge in others by an understanding of their propositions and suggestions; they are not functions of the psychophysical organism, not psychophysical processes, as we must have reached already the artificial reconstruction of science before the subject is replaced by that object among other objects, the psychophysical personality. Scientific and æsthetic acts are not the only functions of



the real subject; the ethical and others stand coördinated, but we are concerned here only with the two functions which do not aim to change and to improve the world but to rethink it in beautiful or truthful creations. It seems to me now that the two attitudes are in every respect antagonistic; to express their direction in a short formula, I should say science connects the factors of reality; art, on the other hand, isolates them. The material of science and of art is then the same, though treated by a different method. Both can deal with all the four factors of reality, with individual acts and overindividual acts, with individual objects and overindividual objects. Life does not isolate fully and gives no complete connection; whatever we turn to with our will has features which lead us further and further to ever new interests; life does not let us sink into the one alone—we rush beyond it to new realities. And life does not give connections beyond the immediate needs of practical purposes in the narrow circle of chance experience. Wherever is full isolation of single facts there is beauty, wherever truth there must be full connection.

The assertion that every isolated fact in its singleness means beauty has for us here only the character of a critical argument and is not for itself object of our discussion. It has for us merely the negative purpose of proving that the singleness cannot be characteristic of history. We cannot thus defend here this assertion by detailed discussion; we have only to elucidate its meaning. Certainly the real life, too, brings us pulses of experience in which our will is captivated by the given experience, satisfied with the object in itself or in the acknowledgment of other subjective acts; then we have the beauty of nature, the beauty of forms and of landscapes, of love and of friendship. Of course, it is only an exception when life offers to us in the untransformed reality such complete beauty; it remains the duty of art to change the world till everything is eliminated that leads the subject beyond the single experience, and the will can rest in the single fact. The world of objects is thus transformed in painting and sculpture, the world of subjective acts remoulded in poetry. The sentiment or the conflict which the poet suggests to us, the bust or the landscape which the artist brings before

our eye, is severed from the practical world; as long as anything connects it with the background of the daily world it may be useful or inspiring or instructive, but it is not beautiful. The poet projects his work into an ideal past; the painter cuts an ideal space out of the reality, and the sculptor fills an ideal space, not the space of our surrounding, to take care thus that the acts and objects may not link into our real world, may never become causes for outer effects, motives for actions, or centers for associations which lie beyond the frame.

We ought not to become skeptical in regard to this point on account of the overhasty generalizations in which empirical psychology mostly characterizes the æsthetic act as rich in associations. The epistemological problem we are discussing can not be settled by psychology, but as soon as the facts are expressed in the terms of psychological language they can not possibly assert the opposite of the epistemological truth. But there is no reason for such a conflict, as psychology is undoubtedly in the wrong. The psychological claim is based on the general theory that all pleasant mental states represent an increase of activity, and with it an increase of associations, while all unpleasant states are marked by a decrease of activity and lack of associations. I think that is wrong; there are different kinds of increase and different kinds of decrease in both ideas and actions. The antithesis pleasure and displeasure does not at all coincide with increase and decrease if we do not arbitrarily select such emotions as joy on the one and grief on the other side. Increase of activity characterizes pleasant and unpleasant states, only in the pleasant states it produces action of the extensors, in the unpleasant states action of the flexors. In the same way decreases of activity can have a double type; it can have its ground in the absence of stimulations, and this is, indeed, characteristic of some unpleasant states, but the lack of outer action can have its ground also in the fact that every motor impulse goes to the antagonistic muscles equally. This increase of tonicity without possible action is characteristic for one pleasant state above all, the æsthetic one. The increase and decrease of associations is here, as always, parallel with the motor impulses. Here also increase of associations is essential

for some pleasant states, but for some unpleasant ones too, only like the muscle activity, in antagonistic directions, in the one case turning to the future, in the other case falling back to the past. And the same doubleness is to be noted in the decrease of associations; in some unpleasant states the decrease comes from a mere lack of ideational impulses, in some pleasant states from the fascination which leads every ideational impulse again to the object itself, so that no thought can lead beyond it. This is again true, above all, for the æsthetic state. The beautiful object includes all that it suggests in itself, and where we connect we sin against the spirit of beauty.

By the contrast with art the fullest light falls on the process of science; every step towards science leads in the opposite direction. The incomplete connections of life are severed by art, but made complete by science, while the material is the same. We had four groups of facts in reality, and we must thus have four groups of sciences which bring systematic connections into the four different fields. We have the science of the over-individual objects, that is, physics; secondly, the science of the individual objects, that is, psychology; thirdly, the sciences of the over-individual will-acts, that is, the normative sciences; and, last, not least, the sciences of the individual will-acts, that is, the historical sciences. Physics and psychology have thus to do with objects; history and the normative systems, ethics, logic, æsthetics, deal with will-acts. Psychology and history have thus absolutely different material; the one can never deal with the substance of the other, and thus they are separated by a chasm, but their method is the same. Both connect their material; both consider the single experience under the point of view of the totality, working from the special facts towards the general facts, from the experience towards the system. And yet the difference of material must, in spite of the equality of the methodological process, produce absolutely different kinds of systems of science. We must consider again from the standpoint of real life how the connections of objects is different from the connection of attitudes, and how the purposes of the systematizing reconstruction are different in the two cases.

We and the other subjects have objects which are in reality,



as we have seen, objects of our will. Why have we an interest in considering the objects from a scientific standpoint, that is in systematized connection? If we do so, it must serve, of course, a special purpose in our real life. The purpose is clear. We cannot do the duties of our life, that is, we cannot act on the objects, if we do not know what to expect from them with regard to the reality which we prepare, and we call the reality which we can still prepare the future. We must ask, therefore, what we have to expect for the future from the objects alone, that is from the objects thought as if they were independent from the subjective will reaction. The answer to this question as to our justified expectations is the system of physical and psychological sciences. To reach this end we must think the objects, the individual or over-individual ones, as if they were no longer objects of a will, as if the subject were deprived of its real activity and were a merely passive perceiving subject the objects of which are thus definitely cut away from the will. Our interest was to determine their influence on the future. We thus consider every object as the cause of an expected effect, and call those characteristics of the object which determine our expectation of the effect its elements. Physics and psychology thus look on their objects as complexes of elements. It is the task of science to reconstruct and to transform the objects till each is thought as such a combination of elements that the effects to be expected can be fully determined from the elements. In this service grew up the atom doctrine in physics and the sensation doctrine in psychology. Each object is thus linked into a causal system; each is considered not as that which it really is, but as a complex of constructed factors which are substituted for the purpose of the causal connection, and each is in question in its relation to all the others. The world thus becomes a system of causally linked objects which can be described by their elements, while these elements themselves are chosen from the point of view of explanation by causality. The determination of the effects by means of the elementary causes is expressed by the laws which give the rules for our expectations. We can say thus that physics and psychology may very well consider any special facts, and, as we have seen, they certainly do not ignore the spe-



cial facts at all, but they consider them with regard to the causal law, and the laws as types of causal connections are thus the only general facts towards which the systematized study of objects can lead us.

Quite different is the systematic connection of the subjective will-attitudes; we may abstract here at first from the over-individual attitudes and concentrate our interest on the individual will-acts. In psychology the will-attitude as such, as act of the real subject, cannot have any place whatever; psychology deals with objects; the subjective attitude is never an object; it is never perceived; it is experienced by immediate feeling and must be understood and interpreted, but not described and explained. If psychology wishes to treat of the will, the psychophysical organism must be substituted for the real subject and thus the will be considered as a process in the world of objects. The description of any known will-acts as psychophysical functions, that is, as illustrations of psychological laws, thus as a matter of course belongs to psychology, and if the psychologist should analyze into psychophysical elements and explain as causally determined all will-acts and human functions of the last three thousand years, he would not transcend the limits of psychology. It would be a very useless psychological undertaking, but it would be such and not history. History starts from and deals with the real subjective will-acts which cannot be found in the world of psychophysical objects.

Our personal life in its political, economical, religious, scientific, æsthetic, technical and practical aspects is a manifoldness of will attitudes and acknowledgments. We live in the midst of a variety of political and social, technical and practical institutions, but no institution means anything else than expectations and demands which reach our will, and suggestions towards which we take attitudes. State and church, legal community and social set, what else are they but will attitudes which we acknowledge and which are, therefore, never understood in their real meaning if they are considered as describable objects, but which must be interpreted and appreciated as subjective will relations, striving towards purposes and ends. And to understand all the technical and practical institutions which civilization brings to

us means again not to describe or explain them, but to interpret them as will suggestions to be imitated. The machine and the book, the law and the poem, are not physical and psychical objects for our interests as living men, but suggestions and demands for the understanding of the intentions and attitudes of other subjects which we can enter into only by taking an imitating or rejecting attitude, thus reaching will by will. All our knowing and believing, our enjoying and respecting—as long as we abstract from their over-individual values—all our education and civilization, our politics and our professional work, is such a complex of real affirmations and negations, demands and inhibitions, agreements and disagreements, which have to be understood and felt and interpreted, but which are not touched in their reality if merely their psychophysical substitutions are analyzed and causally explained. To be a Chinese or Mohammedan, a symbolist or a Hegelian or an atomist, means to be the subject of special complexes of will attitudes and nothing else. If, for instance, we substitute the race for the state, then, of course, we have objects before us and no longer subjective attitudes, but then we deal with biological conceptions and no longer with history.

The manifoldness of will-acts the totality of which forms my real personality thus refers in every act to the will-acts and attitudes of other subjects which I acknowledge or oppose, imitate or overcome. These demands and suggestions of others are not in question in my life as causes or partial causes of my will; they have not to be sought in the interest of a causal connection; they are merely conditions which I as subject of attitude and acts presuppose for my free decision and which are thus logically contained in it; the connection is, therefore, not a causal, but merely a teleological one. The endless world of will-acts which stands thus in teleologically determining relation to our own will-attitudes forms the only material of history.

The material is, of course, unlimited. If every act of ours means an attitude towards acts of others which we must try to understand, it is clear that those others are understood only if their acts again are interpreted as attitudes towards the propositions and demands and suggestions of others, and so on and

on. Every will-act is thus ideally related to an unlimited manifoldness of other acts, just as the movement of every grain of sand is causally related to every molecule in the universe. It is the unique task of history as a science to work out and make complete this teleological system of individual will-relations, thus to bring out the connections between our acts and all the acts which we must acknowledge as somehow teleologically influencing our own. While physics and psychology thus produce a connected system of causes and effects, linking all objects which stand in connection with our objects, history follows up all the subjective acts which stand in will-relation to our subjective attitudes.

Physics and psychology, as we have seen, reach this end through striving towards laws and causality; that, of course, cannot be the way of history. The objects interested us only as factors which influence the future, and the laws by which we have connected them have satisfied this expectant interest. The subjects, on the other hand, do not interest us in first line as causes of effects. Of course, we are able to consider them also as objects which produce effects, and that aspect may become important to us in many practical respects; psychophysics will fully satisfy this kind of interest. And in the same way we may look on the development of peoples with an interest in what we have to expect from them; they are then sociological organisms, the laws of which we study; but such study is not history. The aim of the real historian is not to prophesy the future. Peoples never learn from history, and the forgotten doctrine that we ought to study history to find out what we have to expect from the future stands on the same level with its contemporary, the doctrine that it is the purpose of art to instruct us and to make us better. No, the historian makes us understand the system of will attitudes to which our individual will is related. That, indeed, alone, is our primary interest in the will-acts of other subjects; we want to understand them, not to analyze them into elements; we want to interpret their meanings and not to calculate their future. The objects awake our expectations; the subjects interest our appreciation, and all that we want to know about them is with what other attitudes they agree or disagree.



We thus have the logical aim, to consider them in their relations to all other will attitudes and to work out the system of these connections; that is, to consider the institutions which are the representatives of will suggestions, together with the personalities themselves, as links of this endless chain of will relations.

The purpose of history is not reached until every institution and personality with which we may be in a direct or indirect will relation is understood as a complex of agreements and disagreements, that is, of will attitudes towards other subjects. This regress must be, of course, infinite, just as no physical process can be reached which has not again causes and effects; and this task demands also, like the naturalistic sciences, a continual transformation. Just as the physical object is not really a complex of atoms and the psychological idea not really a complex of sensations, but must be in thought transformed into such to make causal connection possible, so in exactly the same way history must reconstruct the personalities and institutions as complexes of will attitudes, which they really are not, but as which they must be considered to make the unbroken teleological connection possible. And again, like physics and psychology, history too cannot communicate to us the whole of the connected system, but has to work out the general facts which give to every single fact at once its place in the whole system. These general facts in the teleological will system cannot be causal laws, but must be will relations of more and more comprehensive character. Just as in the world of objects the general law covers and determines the causal changes of an unlimited number of objects, so the important will-actions cover and determine in the world of subjects the impulses and suggestions for the decisions and attitudes of an unlimited number. The regularity of the causal law and the importance of the imposing will lift in a corresponding way the general fact over the level of the single facts. It is the work of history to make conspicuous the increasingly important will influences, as it is the work of physics and psychology to work out the laws. If I say I am a German, I want to assert by that statement that I acknowledge by my will a world of laws, institutions, hopes and ideals which are the will



demands of an undetermined multitude of subjects ; this multitude constitutes the historical nation of Germany. But it would be unscientific if I should start to interpret the attitude of every one who is part of that chaotic mass of subjects ; it is the work of science to find those influences which determined the multitude, those will-acts which were imitated and acknowledged by the unimportant subjects. The chaos thus becomes order, and Goethe and Beethoven, Kant and Hegel, Luther and Bismarck, stand as the general facts for the millions and millions of less important subjects who were determined by their suggestions. Any individual's historical place is then characterized by his will attitudes towards the leaders. Just as the naturalist knows a whole hierarchy of sciences which work out increasingly general laws up to mechanics as the most abstract system, so history can consider in different stages the will relations of more and more comprehensive character. The most abstract view is represented by the so-called philosophy of history, which aims at understanding the history of the world as determined by one decision of the will. In this spirit the conception of original sin in the theological systems of the Middle Ages was in the field of historical thinking perhaps not less marvelous than the conception of atomistic mechanism in the realm of natural science. The fact that Adam did not exist in reality is as little an objection to the mediæval construction as the fact that no atom can really exist militates against our atomism ; both reconstructions of reality fill merely ideal places as necessary goals of thought.

On the other hand, in the same way that mechanics does not lower the importance of special natural sciences, no all-embracing theory of the history of man can interfere with the importance of the special historic disciplines down to the biographies of single personalities. But even the biography has to work in the same direction as the most abstract philosophy of history, in the direction of general connection. The real biography written in an historical spirit shows in the individual the attitudes towards the demands and suggestions which make the history of mankind ; the single man becomes thus the crossing point of all the political, technical, religious, æsthetical, intellectual impulses of his time, and he is thus by the will-attitudes which

constitute his personality connected with the whole universe of will-acts. As the astronomer in his calculations describes the one curve of a star as the combination of a large number of impulses by attraction, and thus brings the star in relation to the whole firmament, so the historical biographer reconstructs the one life as a system of single attitudes towards an endless multitude of demands and suggestions. It is a complete transformation in the service of connection. The man's life can be told also otherwise: the life as he feels it as a personal experience; so also do we learn to understand the man; but we have then poetry and not history; it is isolation and not connection. And if we, instead, describe and explain his life as a set of ideas, feelings, emotions and volitions which arose in his psychophysical system from birth to death, then we have again a transformation in the service of connection, but this time for the causal connection of objects, not for the teleological connection of subjects; it is again not history, but psychology.

The separation of the material of the two sciences is thus simple and clear; there can never be a doubt about the line of demarcation, as there is no psychophysical object in the world—from the sensations of a frog up to the ideas of Newton, the emotions of Byron, and the volitions of Cromwell—which is not a suitable object of psychology, and as there is no subjective individual act which cannot be linked into the endless teleological system of history. A division of material, as if a social psychology, for instance, were to deal with the psychical processes of the unknown masses, while history were to deal with the psychical processes of the well-known men, is an absurdity. Not less misleading would be an antithesis between savagery and civilization. From a psychophysical standpoint such a line is secondary; the organism which has outer appendages of his body to make the psychophysical functions more effective has reached merely a higher stage of biological development, but is not different in principle from the lower type in which nature does not provide for detachable acquisitions of the organism. The animal which runs with locomotives, sees with microscopes, hears with telephones, makes gestures of expression through newspapers, attacks through cannons, remembers through libra-

ries, stands above the savage as a dog stands above a jelly-fish, but it is by principle nothing new; it is a more complicated product of nature which, therefore, offers a more difficult problem to the descriptions and explanations of psychology and physiology, but does not become as such material for history. And still another line of separation has to disappear; the fight between the 'materialists' and the 'idealists' of the recent economical schools has nothing to do with the doubleness of psychological naturalism and real historical aspect. If the materialists claim that every occurrence among men is the direct or indirect effect of economical causes, while the idealists consider other causes still which seem to them independent of material conditions, for instance, religious and patriotic emotion or ambition and love, both sides stand fully on the ground of psychology and outside of history. Those emotions of practical idealism are in question only as psychophysical causes and are thus material merely for a causal system. In the system of history exists no causality.

Here is the point where even the historians themselves are inclined to compromises which, at least in principle, must be rejected. Whether or not practically quite interesting reports of periods of civilization can be written by mixing the two attitudes is secondary. Historians, we know, produced in earlier times their deepest effects by mixing history with ethics, but the philosopher at least must be clear that ethics is not history, and he ought to be still less in doubt that a causally explaining social psychology is not history either. As soon as it is acknowledged that we have, on the one side, an interest to consider human life as an object and thus to describe and to explain it, and that we have, on the other side, a logical aim to understand human life as subjective acts which can be only interpreted and linked together by will attitudes, then we must have the energy to keep the two systems separated. Each is logically valuable, each is therefore true, but if confused both become logically useless.

We can say that Socrates remained in the prison because his knee muscles were contracted in a sitting position and not working to effect his escape, and that these muscle-processes took



place because certain psychophysical ideas, emotions and volitions, all composed of elementary sensations, occurred in his brain, and that they, again, were the effects of all the causes which sense stimulations and dispositions, associations and inhibitions, physiological and climatic influences, produced in that organism. And we can say, on the other hand, that Socrates remained in the prison because he decided to be obedient to the laws of Athens unto death. This obedience means, then, not a psychophysical process, but a will attitude which we must understand by feeling it and living through it, an attitude which we cannot analyze, but which we interpret and appreciate. The first is a psychological description; the second is a historical interpretation. Both are true. They are, to be sure, not equally valuable for science, as that particular psychophysical process is not more important for the understanding of the psychological system than millions of other emotions in unknown men, while that will attitude influenced by its demand the acknowledging will of twenty centuries, and is thus most important in the historical system. And yet both are equally true, while they blend into an absurdity if we say that those psychophysical states in the brain of Socrates were the objects which inspired the will of his pupils and were suggestive through two thousand years.

A history which interprets subjectively and understands their purposes out of the deeds of men relinquishes, indeed, its only aim if it coördinates these teleological relations with the causal explanation of human happenings from climatic and geographical, technical and economical, physiological and pathological influences. The subject which is determined by purposes is free; the action which is the effect of causes is unfree. In the unfree world there cannot be any action which must not be understood causally, and we have no right to stop anywhere in our explanation; the unexplained action means only an unsolved problem which is in no way solved if we seek for its subjective meaning instead of its elements and causes. In the world of freedom, on the other hand, it would be meaningless to ask for cause, as the objects then come in question merely as objects for the willing subjects and not as realities for themselves. The



realm of freedom is not made up of oases in the world of necessity; the reality of history is not spread here and there over the field of nature, but lies fully outside of its limits. The antithesis between psychology and history is thus not law and single event, but causality and freedom, and this difference is the logical result of the ontological difference of the material, the one dealing with objects, the other with subjects. Both go methodologically the same way, considering the single facts from the point of view of the general fact, and both transforming the disconnected material until a perfectly connected system is reached. But because objects are understood by describing and explaining them, while subjects are understood by interpreting and appreciating them, the connection of the one system must be causal, that of the other system teleological, and the general fact in the one field must be a law and in the other field the will relation of importance. As every subjective act can be substituted by a psychophysical function of an organism in the world of objects, and as every object can be understood as a value for a will, the whole reality can be brought without any possible remainder under the one aspect as well as under the other. History, in the real historical spirit, then need no longer fear that the progress of psychology can inhibit its functions, and the psychologist need not feel discouraged that his psychological laws of history appear so utterly trivial to the historian. That which is important for psychology, that which is fit for constructing connections between psychological objects, has the privilege of being indifferent for the historian, that is, of being unfit to link subjective will attitudes. Psychology and history cannot help each other and cannot interfere with each other as long as they consistently stick to their own aims. Each of them has thus unlimited opportunities for development. The processions of the great psychologists from Aristotle to Herbart, and that of the great historians from Thucydides to Macaulay, can both have for the future an unlimited number of followers without any quarrel, in spite of the naturalistic claims of our age, which for a while was under the illusion that all is understood when all is explained, and that the historians should better become psychologists.

As soon as the difference of the two standpoints is recognized, light falls on all the special characteristics of the two sciences. Now we understand why history stands so much nearer to real life than psychology. Not, as it was suggested, because history deals with single facts and psychology with general facts, but because psychology deals with objects which are thought as independent of the subject, while in reality and so in history the material is acknowledged only in relation to willing subjects. In real life we are subjects which must be understood but not described; psychology starts thus at once with a material which in its singleness is already farther away from reality than the material with which history deals. Now we understand also why the substance of history has value for us, while the objects of psychology and of all naturalistic sciences are emotionally indifferent. That is not, as it was suggested, because the single facts are important for us and the general facts indifferent; no, it is because the psychological objects, the contents of consciousness, are thought as cut loose from the will and thus no longer possible objects for appreciation, while the historical objects are thought as in their relation to the attitudes of the will. Now we understand also under which principle the historian selects his material. If we accept the view that all single facts belong to history as such, it is arbitrariness to chronicle Napoleon's battles and state acts but not his flirtations and breakfasts, while now we understand how it is that this selection means the most essential part of the historian's work, as it is the way to transform the reality into a system of teleological connections, thus dropping more and more the will-acts which have no teleological importance for will-attitudes of other subjects. Now we understand also why the language of the historian has so much similarity with that of the poet. The historian, we have seen, has aims which are directly antagonistic to those of the poet, as the poet isolates, while the historian, like every scientist, connects his material. But the materials themselves, the subjective acts, are common to the poet and the historian. Where the psychologist encourages the reader to take the attitude of the objectively perceiving observer, the poet and the historian speak of facts which can be understood only by interpreta-

tion and inner imitation; they cannot be described by enumerating their elements; they must be suggested and reach somehow the willing subject which enters into the subjective attitude of the other. Thus the means of both may approximate to each other. The poet and the historian may use the same methods of suggestion to reënforce in the reader the subjectifying attitude which is the presupposition for the understanding of the isolated will-acts in the work of poetry and the connected will-acts in the work of history, while the psychologist has to adapt even his style and his presentation to the service of his objectifying aim.

But we now understand and see in a new light also the relations of the psychological and historical sciences to the normative doctrines, to ethics, logic and æsthetics. As long as history appears merely as a part of psychology or as long as the one is given over to single facts, the other to laws, all the normative sciences stand without any inner relation to any empirical science, those speaking of duties, these of facts. For us the relation takes a very different form. We have seen that all the historical sciences are systems of individual will relations and nothing else. On the other hand, we have found that duty never means anything but our own over-individual will-act. All the normative sciences are thus the systematic connections of our over-individual will-attitudes, our will-attitudes aiming toward morality and truth and beauty and religion. As the over-individual will is, of course, thought as independent of the individual subject, the connection which is sought cannot lead as it did in history from subject to subject; as all subjects are presupposed as agreeing in their over-individual acknowledgment, the connection, the scientific aim can then lie here merely in the systematic connection of our own over-individual purposes and their interpretation. A transformation becomes here, too, necessary in the interest of connection; each single will attitude must be linked into this teleological system and must thus be transformed till it represents a crossing point of all the ethical, æsthetical, religious and logical impulses and demands. The normative sciences and history stand thus in the nearest relation to each other; both are transformations of will-acts in the service of teleolog-



ical connection, only the one reconstructs and systematizes the individual will-acts in us, the other the over-individual will-acts.

The relation between these two groups of sciences, the historical and the normative ones, is thus perfectly parallel to the relation between the psychological sciences and the physical sciences, of which the one systematizes the individual objects and the other the over-individual objects. The proportion—history—stands to the normative doctrines, as psychology stands to physics—is, indeed, true in every respect and in every consequence. We may consider here as our last word only one of them. The historical development of the naturalistic sciences shows the continuous tendency to take more and more of the properties of the physical object into the psychological object, that is, to show that the apparent over-individual qualities of the thing are qualities which depend upon the individual; color and sound, smell and taste, go over from the physical thing into the idea, and thus the whole manifoldness of our experience moves over into the sphere of ideas. In exactly the same way and led by the same methodological motives, history takes more and more of the normative duties over into its own field, and shows how the special duties, the logical beliefs, ethical convictions, æsthetical demands and religious postulates are the results of individual attitudes under the suggestion of the individual groups of will-influences. The absolute duties and beliefs and obligations and truths seem thus lost in our life as the colors and sounds and smells are lost for the physical objects. But the parallelism holds for the end-point of this development too. We must deprive the physical object of its colors and sounds, but we cannot give up the truth that there is a physical object nevertheless, as the quantitative reality to which we project, with objective truth, our sensations and ideas; all the naturalistic sciences would be destroyed if we were to give up this realistic conviction of physics. In the same way we may take into the individual all the single over-individual special duties of special nations and ages and social groups, but the reality of the background of projection we cannot give up. Whatever history teaches, the postulate of the reality of duties, of absolute values, stands firm. The abso-



lute duties may be abstract and deprived of color and sound as is the world of physics, but they stand and must last like the physical universe, and whoever in striving towards truth denies the reality of absolute values and gives up the belief in morality and the belief in logic, thus destroys and undermines his own endeavor to find the truth as logical thinker and to stand for the truth as ethical man.

# A STUDY OF THE RELATIONS BETWEEN CERTAIN ORGANIC PROCESSES AND CONSCIOUSNESS.

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## I. INTRODUCTORY.

Circulation and respiration are the organic processes with which this paper is mainly concerned. An inspection of the literature dealing with the relation of these processes to consciousness reveals a condition of disagreement among investigators, both as regards fact and theory. On the side of fact the discrepancies are gradually giving way before more accurate methods of observation and experiment. On the side of theory, however, the progress toward agreement is, perhaps, less noticeable.

The thesis which we shall defend in the following pages is based primarily upon experiments undertaken by us some two years ago with the purpose of determining whether changes in the character of attention were accompanied by any regular alterations in the organic processes above mentioned. The formulation we have reached should, however, if true, be equally applicable to the observations of other investigators. This we believe to be the case, although the description of these observations is often too meager on the psychological side to permit a satisfactory comparison with our own work. Within the bounds of its pretensions our formula, if correct, will have the value of a centralizing, harmonizing principle for a mass of facts which, from many points of view, appear self-contradictory and unintelligible.

At the risk of devoting a disproportionate part of our space to the matter, we shall begin by attempting a sketch of the more important of the relevant facts hitherto observed. So far

as we are aware, these have never been brought together in the form we adopt, and they furnish the best possible proof of the necessity for some general connective principle.

## II. HISTORICAL OUTLINE.

It will be convenient to state first the factors in the processes under consideration which have been thus far differentiated.<sup>1</sup>

The most striking alterations shown by the respiration consist in changes of rate and depth, or amplitude. The general type of the breathing also displays certain differences under varying conditions. Thus breathing in which costal or thoracic movements predominate may take on a more abdominal or diaphragmatic character. In addition to these changes, however, must be mentioned alterations in the general rhythm and in the duration of the various constituent factors in the total respiratory act. Thus the slight pause which follows expiration may be exaggerated or may practically disappear. Similarly the pause concluding inspiration may be altered in its relation to the total act.<sup>2</sup> Again, as accompaniments of such changes, we may find the usual relation between the phase of inspiration and that of expiration altered; the normal relation being that of a slight excess of expiration over inspiration.<sup>3</sup>

The more important aspects of the circulation, to which reference is made in this connection, may be summarized as follows: (1) The rate and force of the heart-beat. (2) The tension in the walls of the blood vessels—constriction or dilation. (3) The blood-pressure. (4) The amount of blood sent

<sup>1</sup>Excellent descriptions and illustrations of apparatus employed in such investigations will be found in 'La Fatigue Intellectuelle,' Binet and Henri, Paris, 1897. The technique of such apparatus has been carefully studied by Hürthle, Pflüger's Archiv, 53; also by Binet and Courtier, *L'Année Psychologique*, 1895. Cf. also Langendorf, *Physiologische Graphik*.

<sup>2</sup>Certain authorities question the genuineness of these pauses and regard the second as distinctly abnormal. Cf. Landois and Stirling, *Physiology*, p. 200.

<sup>3</sup>The amount of air breathed under various conditions, the amount of oxygen used and the amount of carbon dioxide exhaled have all been studied with much care, but we shall make no reference to these features, as the results attained do not appear to bear in any essentials upon the considerations with which we are here immediately concerned. See Speck, *Physiologie des Menschlichen Athmens*, Leipzig, 1892.

to the brain. (5) The amount of blood sent to the viscera.<sup>1</sup> (6) The amount sent to the periphery. (7) The interrelations between (4), (5) and (6). (8) The features of the cardiac, arterial or capillary pulse: its height or amplitude, shape, etc., with special reference to its anacrotic or catacrotic characteristics. It will of course be understood that in this analysis of salient features in the circulatory process, no implication of complete independence of the elements so distinguished is for an instant contemplated. As a matter of fact, some of the processes do at times vary irrespective of the action of others, but these interrelations will be canvassed more closely later on.<sup>2</sup>

We may summarize the results of the various investigations as follows: The dominant tendency of sensations of every kind is, according to the latest and most careful observations, to produce a vaso-constriction in the periphery and an afflux of blood to the brain. The amount and regularity of these alterations probably depend on the intensity and duration of the stimulus. Those investigators who find regular differences in the organic processes as accompaniments of the algedonic tone of the sensation, would necessarily make an exception in favor of such sensations as are distinctly agreeable and productive in their experiences of peripheral dilations. The effects of sensation upon the heart-beat, the form of the pulse curve and the respiration vary too widely with varying conditions to permit any generalized statement. But wherever a sensation breaks in upon a state of relative quiet and repose, so that a mild emotion or shock is produced, it generally produces acceleration of heart-beat and respiration, the latter being a trifle spasmodic and often deeper.

Mental activity of the type illustrated by application to mathematical computation, memorizing or recalling past experiences is, when contrasted with conditions of greater repose, accompanied by afflux of blood to the brain. Under the conditions of the ordinary laboratory experiment, such psychological pro-

<sup>1</sup> Points (5) and (7) concern processes too inadequately investigated to permit very definite formulations.

<sup>2</sup> As in the case of the respiration and for similar reasons, we make no reference to the observations upon the chemical changes attendant on alterations in these various phases of the circulation. Cf. Hermann's *Handbuch der Physiologie*.



cesses are sometimes productive of peripheral constrictions and sometimes show peripheral dilations. Their effects upon the form of the pulse curve are equally equivocal, but they result with greater regularity in increasing the rate of both heart-beat and respiration, the latter as a rule becoming more superficial.

Emotions of every sort seem more effective than other psychological processes in producing increase of blood in the brain. Vaso-constriction seems to be by far the most frequent volume change in the periphery, although a modification, similar to that mentioned under the head of sensation, has to be made in favor of those investigators who find opposite physiological expressions for agreeable and disagreeable experiences. The same restriction has to be placed on the statement that acceleration of heart and respiration and increased depth of the latter are accompaniments of all emotions. It is not possible at present to speak definitely of the changes in the form of the pulse curve.<sup>1</sup>

Without injustice to earlier investigators, it may be said that our serious knowledge of the connections between consciousness and the organic processes with which we are here concerned, begins with the classic and revolutionary observations, both clinical and experimental, of the great Italian physiologist, Angelo Mosso.

The results of his investigations may be briefly summarized as follows, bearing in mind that the psychological conditions involved are those of sensation, emotion and application to mental calculations. We may notice first the facts concerning changes in volume:

Psychic activity of every kind produces an increased flow of blood to the brain and a decreased flow to the periphery.<sup>2</sup> The changes which follow emotional excitements are much more marked than any produced by intellectual processes, and the responses to emotional stimuli are more noticeable in the brain than in the periphery.<sup>3</sup> Sleep is accompanied by a withdrawal of blood from the brain.<sup>4</sup> Deep in-

<sup>1</sup> We shall occupy the remainder of this section with a more detailed statement of the investigations upon which this summary is based.

<sup>2</sup> *Die Temperatur d. Gehirns*, p. 109. Cf. also Patrizi, *Riv. musicale ital.*, 1896. Some interesting exceptions to this general rule have been observed. They consist of cases in which the activity of attention distinctly antedated the circulatory change. Cf. *Die Ermüdung*, p. 195.

<sup>3</sup> *Kreislauf d. Blutes im Mensch. Gehirn*, p. 72 ff.

<sup>4</sup> *Kreislauf d. Blutes im Mensch. Gehirn*, p. 74 ff. Stimulations which are so feeble to produce awakening nevertheless result in circulatory alterations of

spirations produce a decrease of blood in the brain, whereas deep expirations cause an increase. The same thing is noticed in the upper limbs, but in the lower limbs the relations are exactly reversed, a fact which apparently depends upon the inverse pressure relations of the thoracic and abdominal cavities, the walls of the one moving out as the walls of the other move in.<sup>1</sup> Superficial breathing produces practically no effect upon the cerebral circulation. The plethysmographic changes are not distributed simultaneously over the whole of the body. Local changes of both blood pressure and volume occur.<sup>2</sup> The changes in the volume of the brain generally precede the changes in the periphery and are, therefore, not to be regarded as on every occasion the mere consequences of such peripheral alterations.<sup>3</sup> The change in the brain is also often observed to outlast the change in the periphery. Moreover, the amount of such changes in the volume of blood in the brain is much less than the contemporary changes often occurring in the arm, for example, and of course much less, therefore, than those of both arms together.<sup>4</sup> Added to this is the fact that a decrease in the volume of a limb is not invariably accompanied by an equivalent increase of the brain volume.<sup>5</sup> The disparity is at times extreme.

Upon the much-mooted point as to whether or no the blood supply of the brain is controlled directly by mechanisms of its own, or indirectly through changes in pressure inaugurated elsewhere in the body, Mosso inclines to a positive opinion in favor of some neural process intrinsic to the brain itself, chiefly on the ground of the apparently primary and independent variations in the cerebral circulation.<sup>6</sup>

the brain and periphery. Several observers have confirmed these observations upon the effects of unconscious stimuli. Cf. Binet and Henri, *ibid.*, p. 80; also Howell's *Physiology of Sleep*, *Jour. Experimental Med.*, 1897.

<sup>1</sup> Kreislauf d. Blutes, etc., p. 133. For following statements see pages 126 and 106. Cf. Marey, *Circulation du Sang*, for account of changes due to costal or abdominal forms of breathing.

<sup>2</sup> Cf. Bayliss and Starling, *Jour. of Physiology*, 1894, p. 159.

<sup>3</sup> Die Temperatur d. Gehirns, p. 152. Mosso's observations on changes in the blood pressure do not lend themselves readily to a generalized statement. The observations of other investigators to be mentioned later cover the ground more fully, and we therefore make no attempt to epitomize Mosso's work on this point.

<sup>4</sup> Mays questions the accuracy of these observations. He states, moreover, that he has only succeeded in obtaining noticeable alterations in cerebral circulation in response to emotions, other psychic processes being ineffective. *Virchow's Archiv*, 1882.

<sup>5</sup> Die Temperatur d. Gehirns, p. 147.

<sup>6</sup> The researches of Roy and Sherrington point to the direct effects, mechanical or chemical, of the metabolisms of the brain as sources, on some occa-

This is a convenient place to mention Mosso's striking view, confirmed in part by other observers, that the cerebral circulation is after all not the matter of primary import in determining the phenomena of psychic activities. It might naturally be expected that if mere increase of blood to the brain were the essential precondition of intense psychoses, any medium which would produce such increase of blood would be followed by increased mental activity. This is not always the case; witness the effects of amyl nitrite. The activity of the attention and of consciousness in general is rather to be connected with the functioning of certain nerves, which control those activities of the brain cells that are accompanied by psychic events. The appropriate analogy is that of the glands. Just as we find in these organs that mere afflux of blood is insufficient, aside from the action of certain nerves, to produce inception of their secretory functions, so the brain cells require blood for the exercise of their peculiar activities, but the mere presence of blood is not alone an adequate stimulus to such functioning.<sup>1</sup>

Turning now to Mosso's observations on the pulse and the heart-beat, we find the following general principles. With the exception of the rhythm and the height, or amplitude, the peculiarities of the pulse curve are entirely independent of the heart and find their explanation in the changing conditions of the various blood vessels concerned.<sup>2</sup> The relations between the energy and frequency of the heart-beat are not as yet definitely formulated. The heart-beat is ordinarily slower during sleep than at other times. The pulse becomes anacrotic (Fig. 1, A, after Mosso) after physical exercise, after heating the vessels and after the inception of complete physical and mental quiet; also as the result of hunger. On the other hand, intellectual activity is accompanied by a catacrotic pulse (Fig. 1, B), which is also the form observed after a meal. The anacrotic pulse is not peculiar to the

sions at least, of circulatory control. *Journal of Physiology*, XI., p. 85. Cf. also Wertheimer, *Archives de Physiologie*, 1893, p. 297. Gley has shown that in mental activity the flow of blood to the brain is, as Mosso thought, due to other causes than the heart. *Étude expérimentale sur l'état du pouls*, etc. Paris, 1881.

<sup>1</sup>Die Ermüdung, p. 195 ff. Moreover, it does not appear that the brain acts more promptly or more accurately (within the limits of ordinary non-pathological changes of volume) when it is flushed with blood. Patrizi has found the reaction time at the height of undulations in volume very slightly better than that at the lowest point of such oscillations. Cf. Patrizi, *Archiv d. Psychiatria*, 1896.

<sup>2</sup>Kreislauf d. Blutes, p. 49.



brain, but is found under certain conditions in other parts of the body.<sup>1</sup> The changes in the form of the brain pulse are noticeable only when vigorous mental activity is contrasted with complete rest. Intense intellectual processes are accompanied by increased force of the heart-beat, probably because of the contraction of peripheral vessels necessitating a greater power to propel the blood through them.<sup>2</sup> Changes in the heart-beat are not always results of changes in the respiration, for they occur independently of such changes.<sup>3</sup>

Apart from the changes already mentioned in connection with the circulation, Mosso's statements about the respiratory accompaniments of psychic processes are somewhat unsatisfactory. In his book on fatigue he says, that observations upon himself show that reverie is accompanied by faster breathing than voluntarily directed trains of thought.<sup>4</sup> When one does not attend closely, the diaphragm tends to become quiet and the thorax makes larger but irregular movements. In sleep the diaphragm is probably passive, but periodic changes occur under the effects of drowsiness. In an earlier work, however, he says it is impossible to make any satisfactory classification of breathing types as connected with mental activity, and some of his diagrams certainly conform but poorly to his formulation above quoted.<sup>5</sup>

M. Féré, in his treatise on sensation and movement, gives the first definite statement of antithetical physiological processes as the accompaniments of agreeable and disagreeable experiences respectively.<sup>6</sup> He differs from Mosso in finding certain sensory stimulations of emotional tone, which cause dilations of the peripheral vessels, instead of contractions. As is well known, he connects agreeable experiences of vari-

<sup>1</sup>These statements may be verified by reference to *Kreislauf d. Blutes*, etc., pp. 52-58, 114.

<sup>2</sup>*Die Ermüdung*, p. 184.

<sup>3</sup>*Die Temperatur d. Gehirns*, p. 150. There are, however, certain well-recognized changes in the heart-beat, as well as in the blood pressure and volume, which are due to respiration. The beats corresponding to inspiration are somewhat quicker than the others, and the amplitude of the pulse seems somewhat smaller. Binet and Henri (*ibid.*, p. 50) have called attention to similar rhythmic changes occurring at intervals of three or four respiratory movements.

<sup>4</sup>*Die Ermüdung*, p. 182 ff. The description of the conditions under which these observations were made is too inadequate to permit satisfactory comparison with the work of other investigators.

<sup>5</sup>Cf. *Kreislauf d. Blutes*, etc., p. 70 ff. The irregularities in the breathing which he meets with in mental calculations are probably due to the distinctly abnormal conditions of his subjects.

<sup>6</sup>*Sensation et Mouvement*, Paris, 1887. He uses the words 'exciting' and 'depressing' instead of agreeable and disagreeable, but his meaning seems to be essentially as indicated.



ous kinds with such dilations of the peripheral blood vessels and with heightened tone of the voluntary muscles. Disagreeable experiences he finds accompanied by the opposite conditions of peripheral constriction and lowered muscular tone.<sup>1</sup> He finds momentary intellectual activity accompanied by momentary increase of power in the voluntary muscles.<sup>2</sup> His monograph is too inadequate in its statement of details to warrant critical comparison with the more complete investigations now at hand. It has distinct historical importance, however, because its statements, like those of the next author we shall mention, have been somewhat dogmatically incorporated in recent psychological treatises.

A. Lehmann, in his scholarly treatment of feeling, reports with much fullness his observations on the physiological accompaniments of agreeable and disagreeable experiences.<sup>3</sup> These agree essentially with those of Féré, but emphasize the following facts.<sup>4</sup> Pleasurable experiences are probably accompanied by increased amplitude of heart movements, disagreeable experiences by decreased amplitude of these movements and ordinarily by dilation of the deep-lying blood vessels. He also emphasizes more explicitly than Féré the deeper and more superficial phases of the breathing which he finds characterizing the two antitheses of feeling.<sup>5</sup>

Probably the most careful, systematic and important experiments, after those of Mosso, are those conducted by Binet in conjunction with Henri, Courtier and Vaschide. However much one may take issue with their usually conservative generalizations—and this is certainly the least convincing part of their work—one cannot abstain from the expression of admiration for the shrewd ingenuity and foresight with which they have executed their tasks.<sup>6</sup>

<sup>1</sup>Cf. *ibid.*, p. 110 ff. and p. 7.

<sup>2</sup>The experiments of Patrizi (quoted by Binet et Henri, *loc. cit.*, p. 194) showed that mental work carried on for an hour weakened the power of the voluntary muscles, unless there was some emotional excitement involved, in which case increased power was observed. In the last case, however, after a time the muscular strength fell below the normal.

<sup>3</sup>*Hauptgesetze d. Mensch. Gefühlslebens*, translated by Bendixen, Leipzig, 1892.

<sup>4</sup>*Ibid.*, p. 82 ff.

<sup>5</sup>Lehmann's results, which were obtained by experiments upon five persons, certainly require confirmation. Like Féré's, they differ in the manner pointed out above from the results of Mosso, and they are distinctly at variance with many results obtained by recent investigators, not to mention our own.

<sup>6</sup>It will be convenient to refer in connection with these authors to a certain amount of the recent monograph literature, much of which is canvassed by them.

They find that vaso-constriction of the peripheral blood vessels is the usual result of psychic activity of any sort, especially when this follows relatively greater quiet.<sup>1</sup> There does not seem to be any constant and demonstrable relation between agreeable and disagreeable experiences on the psychic side and vaso-dilation and constriction on the physiological side. Pain has been observed to cause a dilation under certain peculiar conditions, and moderate pleasure has on several occasions been accompanied by constrictions.<sup>2</sup> The true psychic opposites from this point of view seem to be repose and activity with varying degrees of physiological excitation as their counterparts.<sup>3</sup> When attention is vigorously fixed on a calculation, for example, there is sometimes a dilation, whose nature is not known, and sometimes a constriction of greater or less duration.<sup>4</sup>

<sup>1</sup>This agrees with Mosso. See above.

<sup>2</sup>Binet et Courtier, *L'Année Psychologique*, 1897, p. 87 ff., also p. 126; Binet et Henri, *ibid.*, p. 92.

Cf. also Patrizi, *ibid. Rivista di Freniatria*, etc., 1897. We know these articles only from reports.

Shields finds vaso-constriction of periphery with both agreeable and disagreeable odors—*Journal of Experimental Medicine*, 1896.

Dumas, *Revue Philosophique*, 1896, also 1897. Dumas' investigations with morbid and insane cases confirm in general the antithetical relations formulated by Lehmann and others regarding joy and sadness and the physiological expressions of dilation, constriction, etc.; adding some interesting observations on the changing number of blood corpuscles under these conditions. He meets curious exceptions, however.

Sewall and Sanford, studying changes of volume in the forearm under various forms of electrical, mechanical and thermal stimulation, found that strong stimulations generally produced constriction, whereas weak stimulations gave slight dilations after transitory constrictions—*Journal of Physiology*, XI., p. 179 ff.

<sup>3</sup>The very interesting experiments of Howell on sleep, already referred to, furnish beautiful supplements to Mosso's observations, and show that in normal sleep there is first a rapid decrease of blood in the brain, owing to fall in arterial pressure (chiefly in the periphery, it appears) with flooding of the peripheral vessels. This is followed by a period of relative quiet, and then the pressure gradually rises, the peripheral vessels undergo constriction and finally awakening occurs. He regards the fatigue of the vaso-motor mechanism as the immediate cause of sleep. He is inclined to disagree with Mosso concerning an independent vaso-motor mechanism of the brain.

<sup>4</sup>MacDougall, in his article on the Physical Characteristics of Attention (*PSYCHOL. REVIEW*, 3-158), practically agrees with the French observers, of whom we are writing, as regards the volume changes here referred to. Under the head of 'pulse and volume changes' appearing when attention is focussed on a continuous sensory stimulus, he omits any definite reference to the second part of his paragraph title, so we cannot record his results. With what he calls

Short, intense mental application—*e. g.*, the solution of a mathematical problem extending over three or four minutes—almost invariably increases the rate of the heart-beat.<sup>1</sup> After the cessation of the work, this acceleration may continue, or there may be a reaction toward a rate slower than the original one. The effects of long mental effort are less well known, but there seems to be an increased tendency toward slowing of the heart-beat, when compared with the natural tendency to retardation during the later hours of the day and in general under increasing fatigue.<sup>2</sup> Emotions of every kind, practically without exception, produce increase in the rate of the heart-beat. A few rare cases of pain and sadness have been observed to produce a gentle slowing of the rate. In general the changes observed, not only on the heart, but also on the other organic processes under consideration, show much more dependence upon the intensity of the emotion or affective condition than upon the quality as agreeable or disagreeable.<sup>3</sup>

'perceptual attention' he obtains first constrictions and then periodic fluctuations of dilations and constrictions. These undulations appear under several of the conditions he has studied, *e. g.*, attention to calculations, recalling of past experiences, and continued sense impressions.

<sup>1</sup>This has been noticed by a number of investigators, Cf. *La Fatigue Intellectuelle*, p. 41 ff. Mentz (*Phil. Studien*, XI., p. 567 ff.) reports a quickening of the pulse which seems closely related in its rate to the difficulty of the task undertaken.

Moderate physical exercise generally increases the rate and force of the heart-beat, but where excessive a contrary effect may be produced. The changes under physical exercise are, perhaps, due more immediately to alterations of blood pressure, which Marey has shown may by merely mechanical means affect the rate of the heart. In mental activity the pressure seems to rise, and as this alone should, on the grounds of merely mechanical explanations, decrease the rate, it is probable we have here a direct nervous control. Cf. *La Fatigue Intell.*, p. 37 ff. and 58. Mentz finds (Cf. *ibid.*, pp. 83, 95, 101) that auditory stimuli, whether noises or tones, produce at first slowing of the pulse and then a gradual quickening. Changes in intensity show the same result, the quickening beginning at the point where the stimulus becomes disagreeable. With involuntary attention acoustic stimuli were found to produce slowing, with voluntary attention quickening of the pulse. MacDougall (*ibid.*, pp. 163, 169) obtained, with voluntary perceptual attention, increase of heart rate, with continuous sensory stimuli (tracing of figures on the face) slowing. The cases of retardation in the heart-beat reported by both these investigators seem to lend themselves with difficulty to harmonious incorporation in the observations and hypotheses of Binet and his fellow workers. They agree with our own observations, however.

<sup>2</sup>The capillary pulse may almost disappear under these conditions. It has been suggested that this is due to increased pressure. If true, this fact would seem to present a rather troublesome anomaly for Howell's interesting theory of sleep, elsewhere referred to. Cf. Binet et Henri, *loc. cit.*, p. 96 ff.

<sup>3</sup>Binet et Courtier, *L'Année Psychologique*, 1897, pp. 104, 125-126.



Mental work when intense shows a tendency to alter the form of the capillary pulse curve by decreasing its amplitude and with some persons rendering its angles blunter, while the dicrotic may move upward or shrink. With others there is instead of this a distinct emphasizing of the dicrotic and no noticeable decrease in angularity.<sup>1</sup> Conditions of quiet and repose are ordinarily accompanied by a pulse of large amplitude with clear dicrotic. It is possible that a classification of the emotions may prove to be feasible on the basis of the changes of the pulse, but this is hypothetical.

The blood pressure probably rises under every mental excitation, whether the occasion be attention to a sensation, to a calculation or to an emotion.<sup>2</sup> Nor does the nature of the emotion apparently affect the fact of this rise, although it may show a difference in the degree of the latter.

The rate of respiration is increased by mental work of every kind, both the expiration and the expiratory pause being shortened, emotions producing an increased amplitude in the respiratory curve, mental calculations and the like producing more superficial breathing, with decrease of amplitude in the curve. Occasionally sadness produces a slowing of respiration.<sup>3</sup> The effects of emotional states are, however, relatively irregular.

<sup>1</sup> Gley (*ibid.*) finds mental work increases the amplitude of the carotid pulse and accentuates the dicrotic. Binet et Henri (*ibid.*), p. 98 ff., also 113 and 120 ff. Binet et Courtier (*ibid.*), pp. 30-65. Physical exercise of a violent type seems to lessen the amplitude of the pulse and to diminish the clearness of the dicrotic.

<sup>2</sup> Kiesow's observation (*Philos. Studien*, 1895) that changes in blood pressure are noticeable only in connection with affective psychic conditions is criticised on the ground of inadequate technique.

<sup>3</sup> Binet et Henri, *ibid.*, p. 156 ff. Binet et Courtier, *ibid.*, p. 65.

Delabarre (*Revue Philosophique*, Vol. XXXIII., p. 639 ff.) found that persons who naturally breathe rapidly show relatively little effect on their respiration when their attention is engaged; but persons who ordinarily breathe slowly display a distinct tendency to acceleration of respiration when exercising their attention, the acceleration seeming to bear a general relation to the measure in which the attention is exercised.

MacDougall (*ibid.*) found that with perceptual and sensory attention there are generally increased rapidity and superficiality of respiration. The long inspiration and short expiration of sleep and relaxed inattention give way to lengthening of the time occupied by the second factor and shortening of the time of the first. The effects on the respiratory pause are ambiguous. Calculation produces the same general changes found by the French investigators. Recall of past events, when tinged with emotional excitation, is productive of great irregularity of amplitude and form, although the increase of rate is still observed.



Like ourselves, these investigators have been relatively less interested in the effects of psychological conditions upon the voluntary muscles. Mental activity certainly affects the muscles, but the effects differ, depending on the length of time devoted to the psychic process and on the presence or absence of an emotional tone in the experience.<sup>1</sup>

### III. THEORETICAL CONSIDERATIONS.<sup>2</sup>

It will be observed that the investigations which we have been reviewing deal with two separate problems. The first problem concerns the differences in circulation, respiration and muscular tone which characterize the antithetical affective conditions denominated respectively agreeable and disagreeable. The second has to do with the differences manifested by these physiological activities under the various typical psychological conditions, *e. g.*, sensation, intellection, etc., and more especially with the differences distinguishing the affective from the non-affective processes. It is unnecessary to emphasize the lack of any established general principle of correlation for the results of these various investigations, much less to dwell upon the disagreements in regard to details.

Mentz (*ibid.*). In general the breathing tends to follow the rate of the pulse reported above. The strong tendency of heart and respiration to change rate together and in the same direction has been noticed repeatedly. The intimate nature of the connection is unknown.

Lehmann's interesting observations (*Philos. Studien*, Vol. IX., p. 66) on the fluctuations of attention and the different phases of respiration seem to show that the moment of completed inspiration, when the volume and pressure of the blood in the brain are at a maximum, is most favorable for mental activity. Needless to say, this does not altogether agree with Patrizi's observations.

Whipple has found (*Amer. Jour. of Psychology*, 1898, p. 560) that forced respiration seems to assist muscular activities, but to interfere with psychic processes and the functions of the higher centers.

<sup>1</sup>Jastrow (*Amer. Jour. of Psychol.*, IV., 398; V., 223) and Tucker (*Amer. Journal of Psychol.*, VIII., 394) have studied the nature and direction of certain unconscious movements of the voluntary muscles connected with different psychological processes. Heinrich (*Zeits. für Psychol. und Physiol. d. Sinnesorgane*, IX., 342; XI., 410) and Mentz (*ibid.*) have found that with mental calculations the pupils dilate, the lenses become flat and the eyes assume nearly parallel axes.

<sup>2</sup>Logically and chronologically, Section IV. of the paper dealing with our own experiments should precede Section III, for the views defended here are based on the observations therein reported. The present order is adopted in the interests of brevity and clearness in presentation.

If we knew the precise significance in terms of organic metabolisms of such processes as constriction and dilation, we might hope to build up a theory on the basis of such knowledge. Unfortunately, this is not the case. Furthermore, we have seen that various observers have failed to note any uniform connection of these processes with pleasure and pain, in which conditions the 'vitality' theories would find the reflection of organic weal and woe. We find ourselves thrown back then upon some general view of the organism as a whole, if we desire a principle of interpretation for the phenomena concerned.

Such a view is offered us by the ordinary evolutionary doctrine, which finds the essential problem of the organism in the adapting of itself to an environment. This adaptation must involve on its physiological side metabolisms of various kinds, in which katabolisms and anabolisms must sustain certain fairly definite relations of dynamic equilibrium, provided the life process is to be subserved.

If we make reference to any one region or to any one process, the exact relations of these antithetic metabolisms must be constantly changing. Slight excesses of wastage at one point and one period will be offset by repair at a later period. The variations from equilibrium must in conditions of health be relatively insignificant, in order to permit of elastic response to the demands of the total environment. Moreover, these adaptive processes must be constantly in progress and must accompany the psychological conditions called intellectual quite as truly as those called affective. Having regard, therefore, to the very various circumstances in which the organism is called upon to respond to changing stimulations, it seems at least possible that the regularity with which these metabolic processes progress, rather than the presence or absence of any one feature in the process, should be the most characteristic expression of the total organic condition. Certainly the presumption that a psychological process like pleasure in its multiform phases should, regardless of its concomitant mental conditions, be accompanied invariably by a single physiological process like dilation, implies a simplicity of structure and function in the psycho-physical organism and a constancy of organic and environmental conditions which probably do not exist.

In view of such considerations our experimental observations lead us to believe that the changes in circulation and respiration which accompany alterations of consciousness can be formulated in terms of attention as follows :

When the attentive process runs smoothly and uninterruptedly, these bodily activities progress with rhythmic regularity.<sup>1</sup> Relatively tense, strained attention is generally characterized by more vigorous bodily accompaniments than is low-level, gentle and relatively relaxed attention (of drowsiness, for instance) ; but both agree, so long as their progress is free and unimpeded, in relative regularity of bodily functions. Breaks, shocks and mal-coördinations of attention are accompanied by sudden, spasmodic changes and irregularities in bodily processes, the amount and violence of such changes being roughly proportional to the intensity of the experience.<sup>2</sup>

In order to make clear the application of this hypothesis to the facts at issue, we must analyze briefly the mental conditions concerned with reference to the attention. We shall need to consider the following: (1) Emotions of various kinds. (2) Sensations both feeble and intense, both expected and unexpected, both transitory and continuous, both agreeable and disagreeable. (3) Intellective processes involving memorizing, recalling and reasoning in the narrow sense, *e. g.*, mathematical calculations ; also reverie.

Now, emotions represent psychological conditions of great instability. Especially is this true when the emotion is profound. The necessity is suddenly thrown upon the organism of react-

<sup>1</sup> To prevent tedious repetition we shall hereafter, except when otherwise stated, use the phrases 'bodily activities,' 'bodily processes,' 'functions,' etc., to mean respiration and circulation.

<sup>2</sup> It will be remembered that the fundamental antithesis found by Binet and his co-workers is that of mental activity in general as against mental passivity. In apparently abandoning any one physiological change like vaso-constriction or increased rate of respiration as a criterion of the psychological condition, we do not mean to imply, even tacitly, that no single change of such character is an essentially constant companion of any one psychological process, like the emotion of anger, for instance. We simply emphasize the apparent absence of any such change as an invariable index of more than one or two conditions, whereas the changes as we formulate them appear to be constant for all conditions. The experimental portion of the paper will bring this point out more fully.



ing to a situation with which it is at the moment able to cope only imperfectly, if at all. The condition is one in which normal, uninterrupted, coördinated movements are for a time checked and thrown out of gear.<sup>1</sup> Equally spasmodic and interrupted is the activity of attention. It may on the other hand be asserted, in opposition to this view, that never is attention so monopolized and completely absorbed by a situation as in the case of a deep emotion. But this is to overlook the cataclysmic change at the outset of the emotion, as well as the violently recurrent rhythms with which the situation is surveyed.<sup>2</sup> In no strict sense does the attention ever delay long with absolutely one phase of an idea,<sup>3</sup> and by so much as the profound emotions are more intense than the ordinary experiences of life, by so much are the shifts in attention more violent than usual.

Moreover, affective conditions of every kind show their affinity with the emotions by a similar instability of attention. This instability is far less with agreeable experiences than with disagreeable ones. This may be connected with the fact, made much of by some psychologists, that pleasure represents a tendency to persist and pain a tendency to change.<sup>4</sup> It calls to mind also the theories of pleasure as associated with normal and moderate activities, and pain as associated with excessive activities.<sup>5</sup> But whenever the experiences are very intense we meet,

<sup>1</sup>Cf. Dewey, 'Theory of the Emotions,' *PSYCHOLOGICAL REVIEW*, Vols. I. and II.

<sup>2</sup>It may at first sight appear that, however fairly this description applies to the more tempestuous emotions, like anger, it is seriously defective when applied to some of the semi-morbid phases of grief and depression. Waiving the justice of the criticism of this characterization of the play of attention in the depressive emotions (we think the apparent difference of opinion rests on a confusion of attention to a topic of thought with attention to a single image), we may simply reiterate, that our observations indicate that the bodily changes run parallel, as regards their regularity or irregularity, with the mode in which attention proceeds. In general the emotions show a much disturbed condition in this particular. But for us this is more or less of an accident, and our contention would be in no wise affected if emotions showed a precisely contrary condition, provided attention also changed its characteristics.

<sup>3</sup>Cf. James, *Prin. of Psy.*, Vol. I., p. 421. Ribot's *Psychol. of Attention* presents a thoroughgoing account of attention as 'monoideism.' The effects of neural fatigue would forbid any long-continued dwelling upon literally one idea. Cf. Hylan, 'Attention,' *Monograph, PSYCHOL. REVIEW*, 1897.

<sup>4</sup>Stephen, *Science of Ethics*. Horwicz, *Psychol. Analysen*.

<sup>5</sup>Spencer, *Prin. of Psychology*.

at the moment of their initiation at least, with relative instability of attention, though the continuance of this instability is incomparably more marked with the unpleasant states than with the pleasant ones.<sup>1</sup> This is tantamount, of course, to saying that the whole distinction is relative.

The different conditions of attention under which sensations may be experienced vary rather more widely than those of emotions, so that if we had regard only to the fact of the presence of a sensation, we might fallaciously assume a uniformity of conditions which does not exist. What we have said of affective conditions in general, in the previous paragraph, holds equally true when these affective experiences have a sensation as their basis, and need not be repeated here.

The distinction between expected and unexpected transitory sensations is one of considerable import for our interpretation.<sup>2</sup> This is the more true the more powerful the sensory stimulus employed. It becomes relatively insignificant as such stimuli approach the limen. The effect of an expected sensation upon attention will, if the sensation be not so intense as to produce shock, nor so feeble as to require excessive effort to detect it, be the securing of a slight strain of anticipation, with at times a somewhat definite relaxation when the sensation is felt. On the whole the play of attention is relatively free and unimpeded. When the stimulus is so faint as to require great concentration we may get more irregularities, owing to the fluctuations of attention from fatigue, distraction, etc. But still the conditions are relatively stable. With the very intense stimulus, whether expected or not, there is sure to be something approaching shock, and with this the introduction of a distinctly disagreeable affec-

<sup>1</sup> Ward's formulation of pleasure and pain in terms of the effectiveness of attention has much that is allied with the view we are presenting. It does not, however, seem to do full justice to the neutral-toned consciousness of moderate intellectual labor, where attention is apparently exercised with distinct effectiveness. (Cf. *Ency. Brit.*, article 'Psychology'.)

<sup>2</sup>The conditions involved in expected and unexpected sensations approximate closely those of voluntary and involuntary sensory attention (cf. *Mentz*, *ibid.*). The so-called cases of involuntary sensory attention are such as occur when a stimulus succeeds in breaking in upon a condition of mental pre-occupation, whether one be engaged in intense thought processes or in some of the various forms of reverie. We cover both these cases, but do not use this terminology.

tive condition, whose consequences we have already canvassed. Indeed, a sensory stimulus of very moderate intensity may, if unexpected, produce this shock in a rudimentary way, and this more violent disturbance to attention is the principal difference noticed between the expected and the unexpected sensation. With many unexpected sensations this shock may practically disappear. The process going on at the time the stimulus is given will determine in large measure its effectiveness or ineffectiveness in producing such shock.

Sensations which are continuous, provided they be not very intense, produce conditions of attention which are relatively regular and stable. We speak here of the cases in which attention is intentionally fixed on the sensations. If continued long, we shall get rather definite fluctuations of attention, but these need not be violent within any ordinary limit of time. Sooner or later we should meet total collapse of attention, preceded by the phenomena of mal-coördination that accompany fatigue. Continuous sensations, which are not made definite objects of attention, produce very various results, sometimes being relatively ineffective and at other times seeming to modify materially the attentive process. The psychological conditions involved appear too ambiguous to warrant laying much stress on these cases.<sup>1</sup> A series of very intense sensations, or a really continuous sensation of this kind, will produce, as in the case of the transitory sensation, distinct shock and its disturbing consequences for attention.

From the standpoint of attention the intellectual processes involved in memorizing and in simple mathematical calculations have much of affinity with the continuous sensation and represent relative stability and regularity. If the task becomes too confused, as it may when one is required to multiply mentally one three-place number by another, then we may meet with breaks and irregularities in attention. Moreover, we shall often find that such experiences are accompanied by a slight feeling of anxiety and distress, springing from the interest in accom-

<sup>1</sup>Cf. Mentz (*ibid.*). The chief difficulty in these instances arises from attempting to apportion the responsibility for the changes observed between the existing mental processes, into which the continuous sensation is supposed at times to inject itself, and the sensation itself.



plishing the work promptly and correctly. Where this element enters to any degree, we may look for the characteristics of emotion. Indeed, we may properly remark in this connection that under ordinary conditions these processes, which we are analyzing separately, necessarily overlap one another at times. The mental application in the case of the intellectual processes is usually to problems received from sensory sources, visual, auditory, etc. The sensations impinge upon already existing affective and intellectual conditions, and emotions are, with the best of intentions on the part of the experimenters and subjects, likely to intrude themselves in some measure upon all the processes studied. When the attention is relatively strained and tense, we meet the greatest stability and regularity, if the task in hand is just difficult enough to be successfully carried forward at the rate at which new aspects of it open up. Thus we may obtain great regularity of functioning, if series of problems in addition or multiplication are presented at just that rate which permits their most rapid solution, avoiding on the one hand unoccupied leisure between the problems, and on the other hand insufficient time for completing them.

The cases in which one attempts to recall past events show considerable variations, depending on the nature of the subject-matter recalled. Verbal material which has been learned by heart may be recalled under conditions of great stability and regularity of attention. Events, on the other hand, may or may not be recalled readily, and if they do not come to mind easily we shall get more or less instability of attention, the results depending on the amount of effort put forth. Such processes are especially prone to take on emotional coloring with its tendency to instability.

Revery, in the proper sense of the word, represents frequently a high degree of free and regular play of attention, interrupted now and then by the emotional suggestiveness of the subject of thought. When revery passes over into drowsiness, the attention becomes much relaxed and functions on a low level of intensity, but yet as a rule with a considerable degree of smoothness. Individuals vary vastly, however, in the nature of the revery process.

If we turn now and arrange our psychological conditions in a hierarchy representing increasing stability of attention, we shall obtain something of this kind: (1) The profound emotions, presenting sometimes an appearance of stability, but even here distinctly of the abnormal, paralytic type. (2) The more violent affective conditions, certainly the disagreeable and painful experiences, less confidently the instances of extremely pleasurable experiences. (As has often been mentioned, it is exceedingly difficult to produce very intense pleasures under laboratory conditions.) (3) Cases of transitory and relatively superficial emotions (including cases in which emotional excitement occurs, although the conditions are ostensibly those of intellectual processes, *e. g.*, recall of past events) intermingled with responses to unexpected sensory stimuli of moderate intensity and brief duration. Expected sensations, if relatively intense, also belong in this class, together with many agreeable sensory experiences. (4) Cases of continuous sensations, the regularity being greater in proportion to the effort made to attend and being, perhaps, greatest with relatively weak sensations. Mental application, as in the case of mathematical calculations, when executed under the most favorable conditions as above described. Many cases of non-emotional revery. The revery of drowsiness differs in its type of regularity from that manifested by application to a problem, in that one is accompanied by the phenomena of relaxation and the other by those of greater organic excitation. But both are relatively stable.<sup>1</sup>

This brings us to a consideration of our experimental material. After a brief description of the conditions under which we have worked, apparatus, etc., we shall proceed to show how radically the physiological accompaniments of apparently similar psychological conditions may vary from time to time, depending on the manner in which attention functions. It should

<sup>1</sup>It will be understood that this classification pretends to nothing but a rough suggestiveness of the relations these different processes bear to one another when attention is employed in this way for connecting them. It will have served its purpose if it brings out a few salient relationships, such, for example, as the community of certain sensation processes with intellectual conditions and that of certain other sensation processes with emotion.

not be understood that we dogmatically deny any constancy of changes aside from the form of constancy we emphasize, although such constant conditions are rare. We simply maintain that from our observations the only feature which appears essentially constant under ALL PSYCHOLOGICAL conditions is the relative stability and instability (of the dynamic type) which these organic activities manifest in connection with the different processes of attention. We have stated this previously, but repeat it to avoid confusion and misunderstanding.

#### IV. REPORT OF EXPERIMENTS.

The experiments which give rise to this paper consist of two very complete series of tests taken from two different subjects. Less extended observations upon a number of other subjects have tended to confirm our confidence in the general position we adopt.

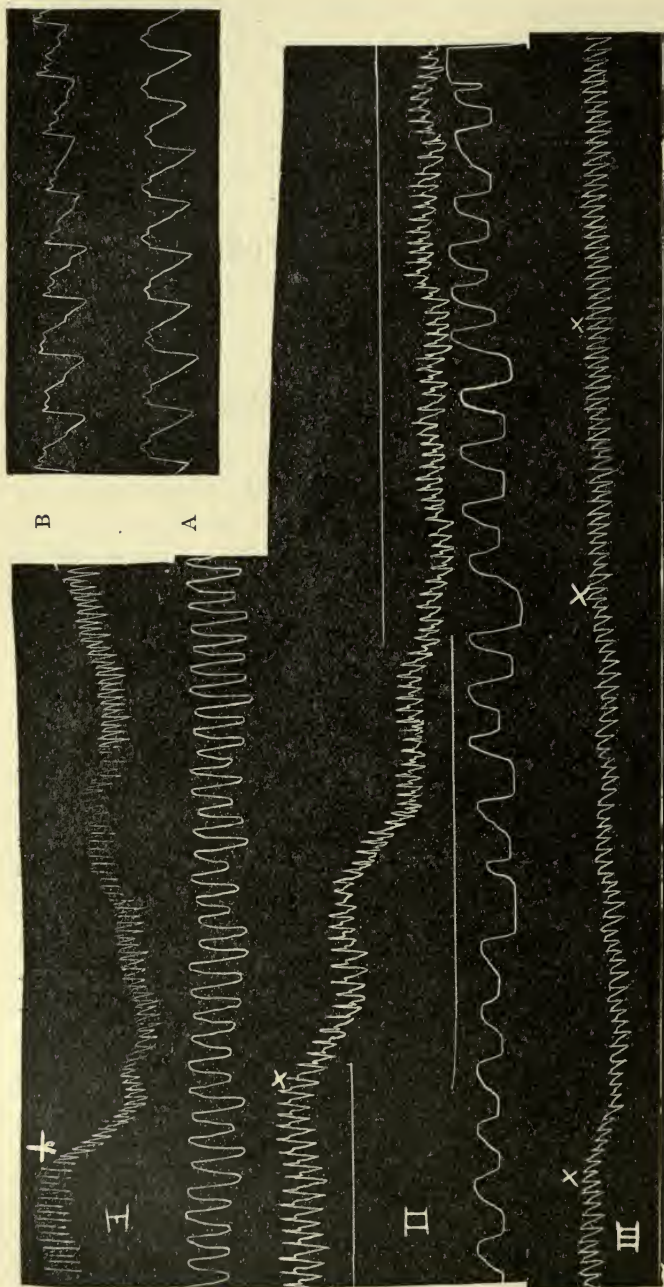
The curves showing circulatory changes are all capillary pulse tracings, taken with the air plethysmograph invented by Hallion and Comte. The air plethysmograph was adopted in preference to the water plethysmograph, cardiograph, or any of the methods of taking the arterial pulse directly, because of the greater delicacy and accuracy with which it registers slight variations in the form and amplitude of the pulse curve. The plethysmograph was connected with a Marey tambour, writing in the ordinary manner upon a smoked drum. After a somewhat extended series of experiments upon technique, the most advantageous bodily position, adjustment of the plethysmograph, quality of rubber and length of pointer for the tambour and relative position of tambour and drum were adopted and preserved throughout the experiments.<sup>1</sup> Careful tests upon the accuracy of the instruments have convinced us that the curves may be relied upon for recording the direction of changes in the volume of the blood in the hand, in the rate of the heart-beat, and in the form and amplitude of the capillary pulse.<sup>2</sup>

<sup>1</sup>The conditions adopted by us agree substantially with those of Binet and his co-workers. The fact that a few of our curves are to be read in a different direction from the others arises from a temporary reversal of the drum, which has no effect whatever on the curves, although we regret the lack of uniformity in their appearance.

<sup>2</sup>The apparatus does not record pressure changes.



## PLATE I.



I., Anger; II., Emotional Excitement; III., Cross 1, Embarrassment; 2, Addition; 3, Multiplication.

## EXPLANATION OF CUTS.

1. All curves read from left to right, except when arrows indicate the contrary. 2. The process illustrated by the curves begins in every case at the point indicated by a cross. 3. A fall in the pulse curve always indicates vaso-constriction, and a rise vaso-dilation.
4. The respiratory curves, when given, are always placed immediately under the pulse curves with which they belong.

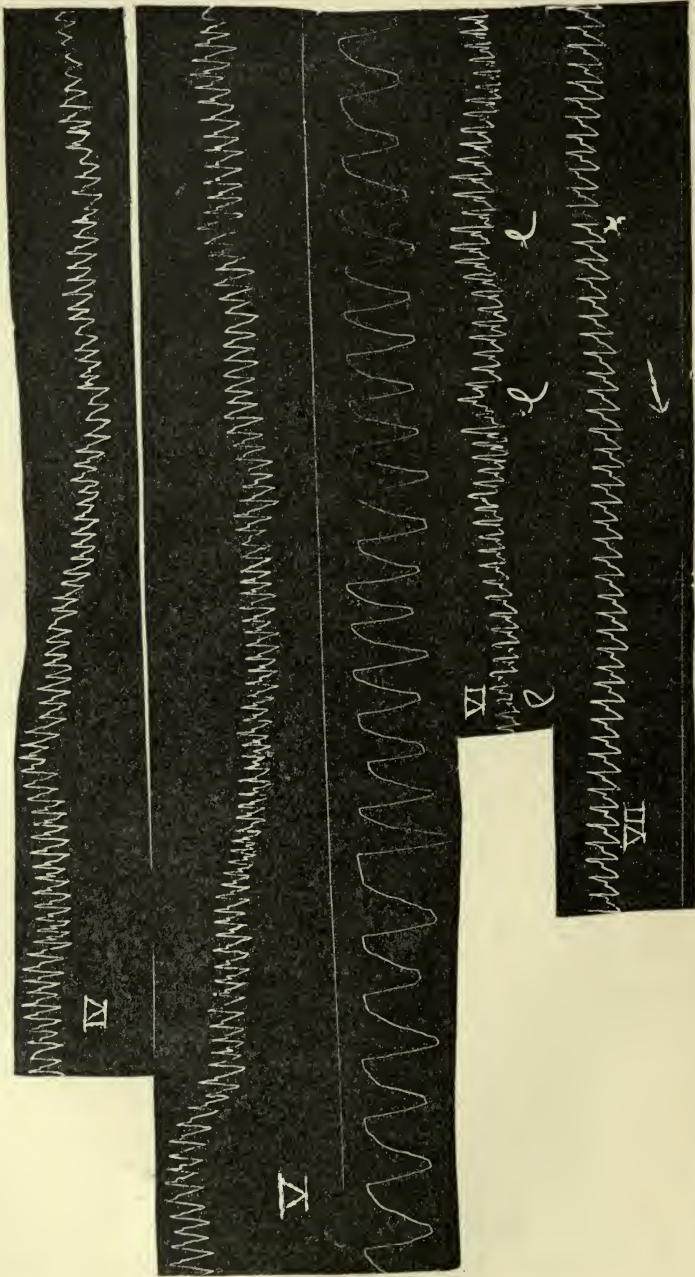
The absolute measurements of the curves of one day could not, however, be compared with those of another, since slight changes in the adjustment of the plethysmograph or of the tambour produce slight alterations in the absolute dimensions of the curves.<sup>1</sup> A modified form of Bert's respirator was used for recording the breathing curves.

The emotional experiences of this series of experiments were spontaneous emotions arising from the subject's own thoughts when left to himself. The most noticeable effects of emotional states upon the bodily processes are the sudden, violent changes and irregularities produced. The vaso-motor shifts are the most evident of these changes, although marked irregularities in the rate and amplitude of both breathing and pulse curves occur. In Plate I., Figs. I. and II. show characteristic cases of violent emotion; Fig. III. is an example of one of the milder emotions, embarrassment.

It is in the case of the emotions, where the agreeable and disagreeable experiences are most intense, that we should expect to find the most marked and constant correspondence of agreeable states with one set of physiological processes and of disagreeable states with an antithetical set, if any such relationship existed. But our curves show not the slightest evidence of such an interconnection. None of the various factors involved, vaso-motor level, rate and amplitude of the pulse curve, position and emphasis of the dicrotic notch, or rate and amplitude of the breathing, changes uniformly in one direction for agreeable experiences, and in the opposite direction for disagreeable experiences. No doubt cases occasionally occur where some regular connection of the kind mentioned is found. But it is occasional and not invariable, in fact probably rare. Almost all of our emotional experiences, whether agreeable or disagreeable, produced vaso-constrictions. This agrees with the observations at the Sorbonne and is what Mosso's work would lead us to expect. Figs. IV. and V. (Plate II.) are examples of an unpleasant and a pleasant anticipation respectively, experienced

<sup>1</sup> Binet expresses a greater confidence in the comparability of curves taken at different sittings than our experience has led us to feel. But, as the matter is largely one of skill, he is doubtless justified in his assurance.

## PLATE II.



IV., Unpleasant Anticipation; V., Pleasant Anticipation; VI., Laughter at each point marked 'e'; VII., Amusement.

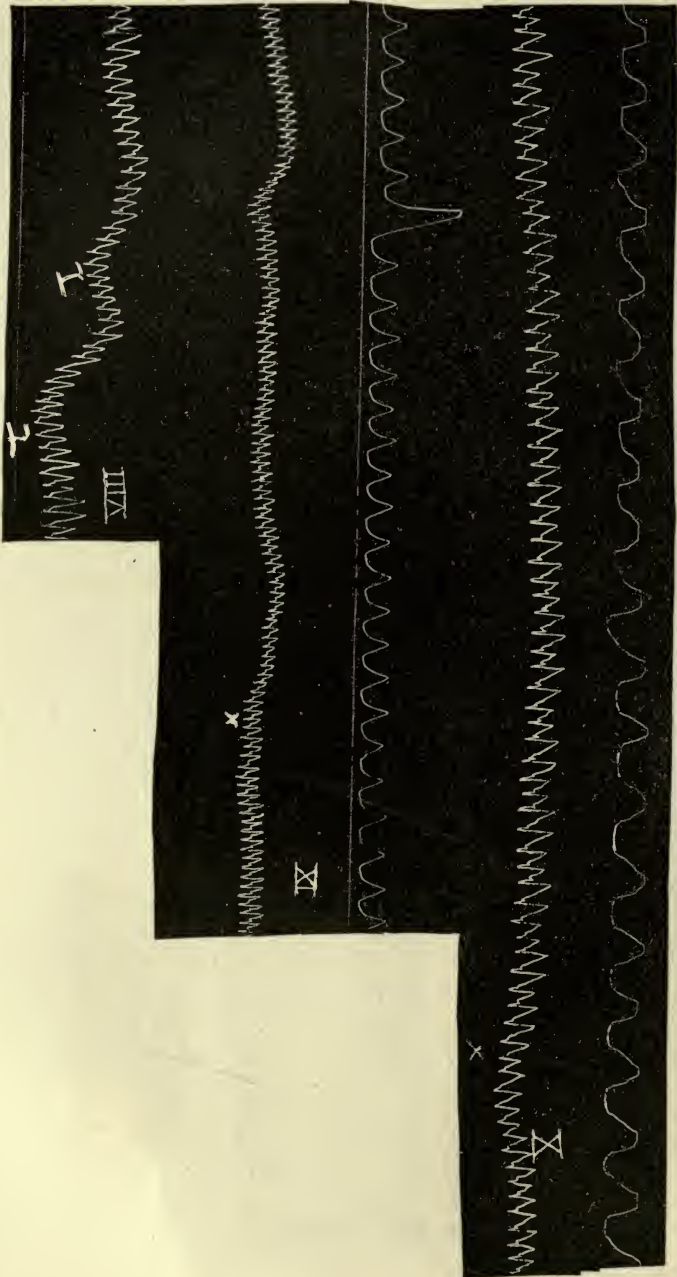


by the same subject at one sitting. Both cause violent vaso-constrictions. The most important exception to this rule is the fact that with one of the subjects, laughter causes a slight, sudden vaso-dilation (see Fig. VI., Plate II.).

At first sight the vaso-dilations due to laughter would seem to be a confirmation of the theory that agreeable experiences are accompanied by dilations of the peripheral blood vessels. But there are several facts which take away the value of this evidence. In the first place, the vaso-motor change seems to be a secondary effect of the sudden spasmodic change in the breathing. Of course, the spasmodic breathing of laughter is an essential factor in it, and it is impossible to separate the secondary vaso-motor changes due to breathing from those accompanying the feeling of amusement in laughter. But the character of the vaso-dilations seems to run parallel with the breathing changes rather than with the feeling of amusement, which does not, as every one knows, always correspond with the heartiness of the laughter. A hearty laugh, causing sudden, violent changes in the breathing curve, is accompanied by the sharpest and most marked vaso-dilation, while a smile or mild laughter causes much slighter and more gentle changes in the vaso-motor curve. In confirmation of this view, we have one curve from this same subject, in which mere feeling of amusement, unaccompanied by any of the breathing changes of laughter, produced a slow vaso-constriction (VII., Plate II.). But more important still, as contrary evidence, is the fact that, in the case of the other subject, constriction and not dilation is the most marked vaso-motor accompaniment of laughter. VIII., Plate III., shows characteristic laughter curves for this subject. They display slight initial dilations followed by marked constrictions. In this case, too, the amount of the vaso-motor change is in general proportional to the amount of the disturbance in the breathing. But why in one subject spasmodic breathing should have vaso-dilation as its concomitant, and in another subject vaso-constriction, is a mystery. However, the facts show that the dilations of laughter in this case can not be taken as confirming the theory that vaso-dilation accompanies pleasant experiences.

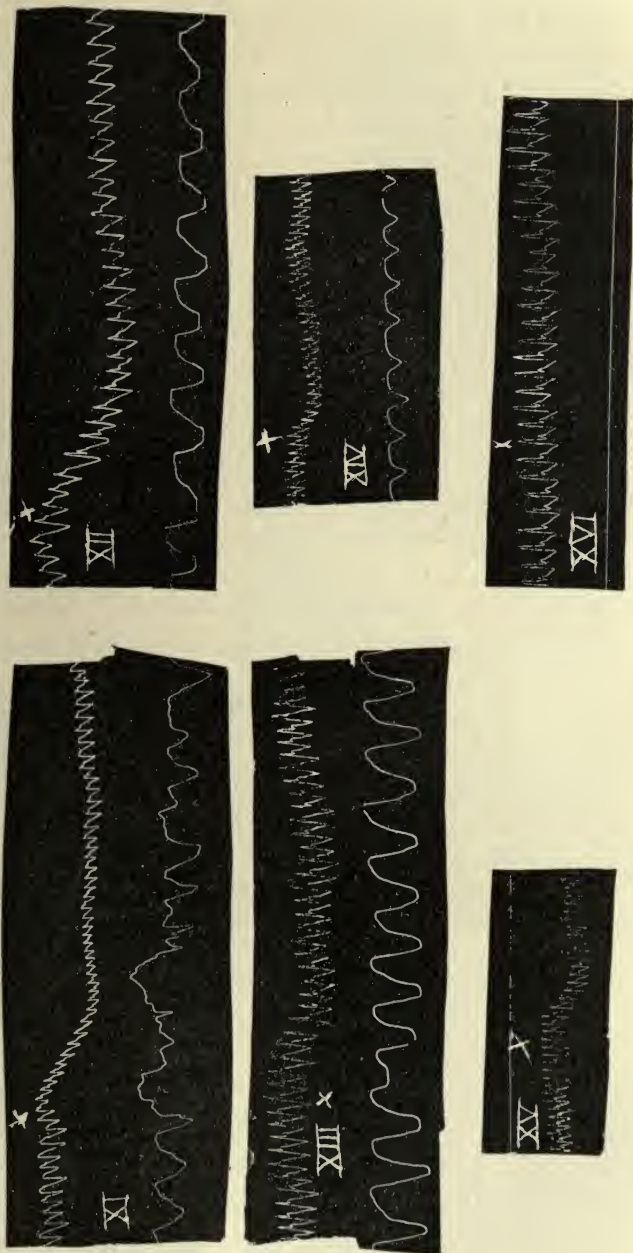
The amplitude of the pulse curve shows a greater or less

PLATE III.



VIII., Laughter marked 'L'; IX., Interest followed by Emotional Excitement at Cross; X., Apprehension, warned of impending shock.

PLATE IV.



XI., Cold; XII., Noise; XIII., Camphor; XIV., Cold; XV., Noise; XVI., Camphor.

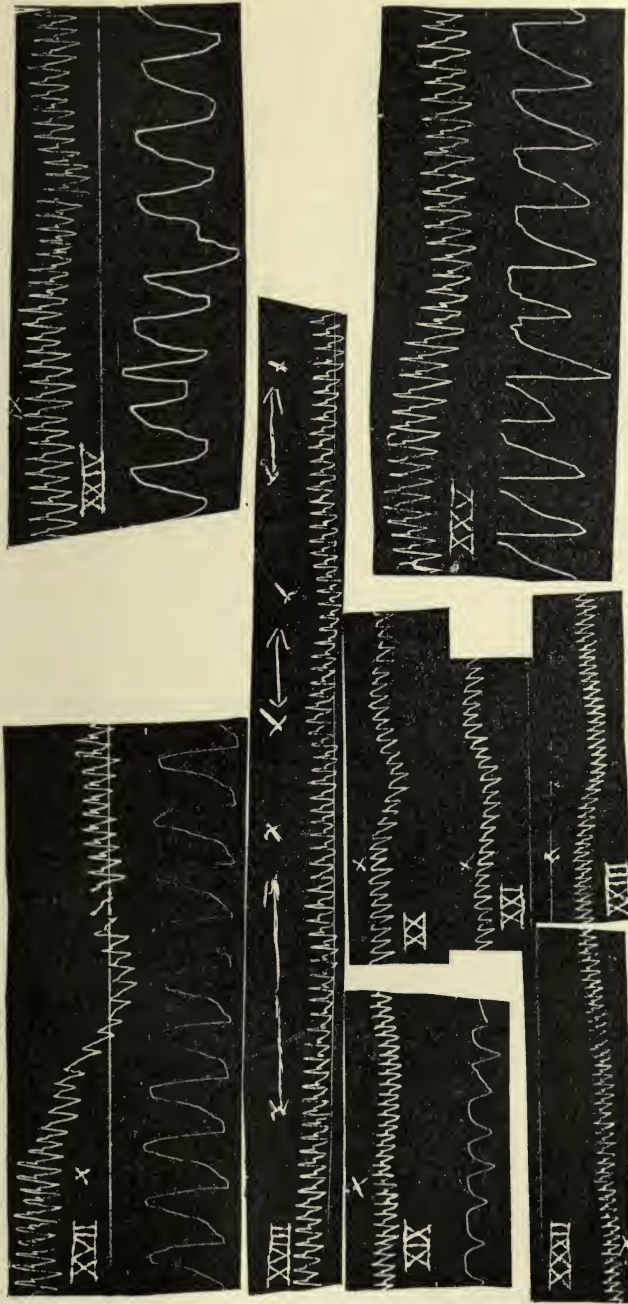


decrease for both subjects in almost all of the emotional experiences, whether agreeable or disagreeable. There were, however, a few cases of increase of amplitude. The great irregularity of amplitude during emotional experiences is a more uniform factor. (See Figs. I., II., III. and IX.) The rate of the heart-beat is sometimes increased *on an average*, sometimes decreased, and sometimes not changed at all. Increase of rate is much the most frequent occurrence regardless of the quality of the emotion, but all of these changes take place during each of the two great emotional states (compare Figs. I., rate unchanged; II., rate increased; III, rate both increased and decreased; and X., rate decreased). But whatever the average change of rate may be, a more uniform and, in our opinion, more significant feature is the spasmodic irregularities of the rate characteristic of curves corresponding to emotional states (compare Figs. I., II., III., IV., V. and X.), a feature strongly indicative of the general physiological instability of emotional states for which we are contending. The changes in form and position of the dicrotic notch<sup>1</sup> are as erratic as the amplitude and rate changes. The notch is sometimes raised, sometimes lowered, sometimes emphasized and sometimes flattened, with entire disregard to the affective tone of the emotion.

The breathing during emotional experiences shows no greater uniformity in direction of change than the pulse. All the variations of increase and decrease of both rate and amplitude of the breathing are found accompanying both agreeable and disagreeable experiences. In the more violent emotions (see Figs. I. and II.), and, of course, in laughter, the breathing becomes very spasmodic and irregular as to both rate and amplitude. The lesser emotions show smaller disturbances (see Figs. IV., and X.), while some of them show no change at all (see Fig.

<sup>1</sup>The emphasis and position of the dicrotic notch vary greatly in our different curves. This is no doubt largely due to the fact that the dicrotic notch varies so greatly at different times of the day and under different conditions of nutrition and of activity. (See Binet and Courtier, *L'Année Psychologique*, 1897, p. 10.) It may also be due in part to slight differences in the adjustment of the instruments at various sittings. But, since our conclusions are based upon immediately successive changes in the form of the curve only, this is a matter of no moment in the present case.

PLATE V.



XVII., Heat; XVIII., Color red, eyes open where arrows occur; XIX., Expected noise; XX., Slight noise; XXI., Cold; XXII., XXIII., Disagreeable odor, capsicum; XXIV., XXV., Camphor.

IX). The breathing, then, at least in the case of the more violent emotions, shows the same functional disturbance which has already been shown to be characteristic of the pulse curves.

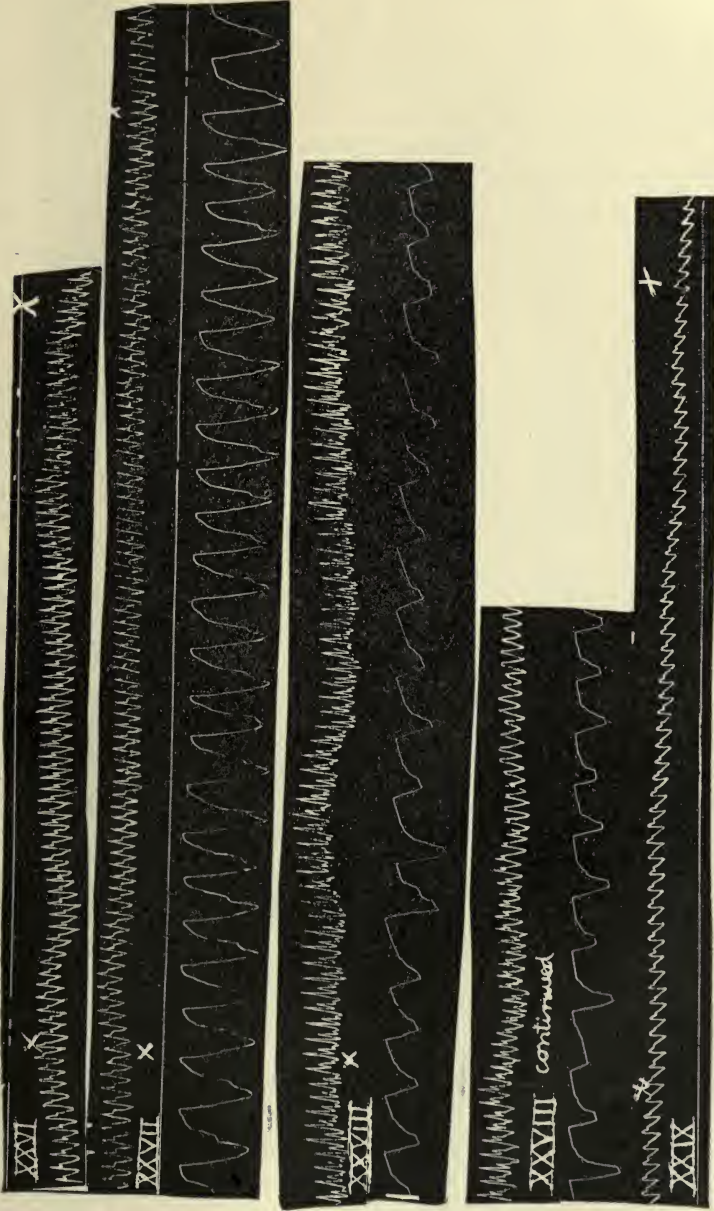
According to the psychological analysis of the states known as emotion, sensation and intellectual application, which was offered in the preceding section, we found that, when classified with respect to the stability of the attentive process involved, sensation occupies a middle range between emotion and mental application. It was also pointed out that the term sensation covers a great variety of experiences, some of which border closely upon emotional states, while others approach the intellectual conditions. If the hypothesis is correct, that the degree of stability of the physiological processes runs parallel with the degree of stability in the attentive process, we ought to find in general the curves for sensation showing less disturbance than those for emotional states, and more than the curves for intellectual application. Moreover, we ought to find the curves representing sensation varying from curves approaching the emotional type to those closely resembling the type of intellectual application, according to the quality of the sensation as regards the functioning of attention. This is exactly the relationship which a study of our curves reveals.

Figs. XI. to XVI., inclusive, are typical curves of sensory stimulation. The first three of the set are taken from one subject, and the last three from the other. A comparison of these curves with the preceding ones of emotional experiences and with the succeeding curves of mental application (Figs. XXVI. to XXXI.) will show that the vaso-motor shifts for sensory stimuli are not so great as those for emotional experiences, but are much greater than those in the mental application curves. The amplitude and rate are less spasmodic and irregular than those for emotional states, but not nearly so uniform and even as the rates and amplitudes of the curves of mental application. The amplitude and rate changes which occur are often equal in amount to those of emotional experiences, but they are less jerky and irregular. They approach more nearly the even, progressive changes of mental application.

But within the large class of psychic states known as sensa-



PLATE VI.

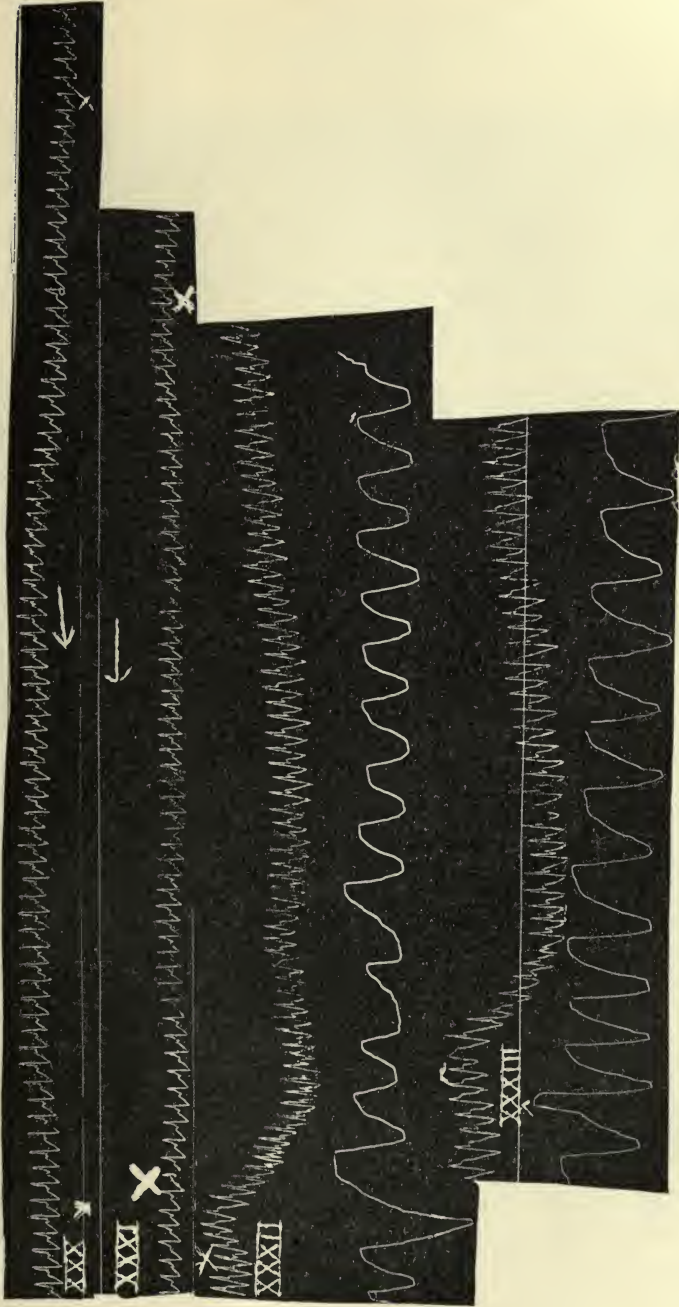


XXXVI., Addition; XXVII., Memorizing; XXIX., Multiplication.

tion, for which the statements in the preceding paragraph are true in general, we find the wide variations in the accompanying bodily processes which our psychological analysis has led us to expect. Various sensory stimuli produce experiences of widely different intensities. A hot object touching the skin produces a much more intense experience than a colored light impinging on the retina, and, therefore, makes a much more imperative demand for attention. The shift from the preceding state to the new experience is much more sudden and violent in the case of the hot stimulus, and involves a rudimentary shock, which is entirely lacking in the color stimulation. The different bodily processes accompanying these two sensations are shown in Figs. XVII. and XVIII. The sudden violent changes in vaso-motor level accompanying the heat stimulus are much like those of the emotional experiences, while the slight gentle fluctuations of the color experience approach the mental application curves.

But even the same kind of sensory stimulus occasions states which differ greatly in intensity at various times, according to the actual physical intensity of the stimulus, the nervous irritability of the subject at the moment when the stimulus occurs, and the element of surprise involved. A loud noise, for instance, produces a much greater shock, and a correspondingly greater disturbance in the bodily processes, when it is unexpected than it does when the subject is prepared for it. Fig. XV. shows the effects of an unexpected noise. Fig. XIX. is a curve for noise taken at the same sitting, with the sole difference that in Fig. XIX. the noise was expected. The shock involved in the unexpected noise made the experience take on an emotional tone, which is reflected in the spasmodic change of vaso-motor level and amplitude, while the expected noise produces only a slight irregularity in the curve. When both noises are unexpected, a loud noise produces a much more violent shift of attention than a slight one. Fig. XII. is a characteristic curve for a loud noise, while Fig. XX. shows the effect of a slight noise upon the same subject. The contrast is much the same as that between the expected noise and the unexpected one. Furthermore, a stimulus which occurs while the subject is

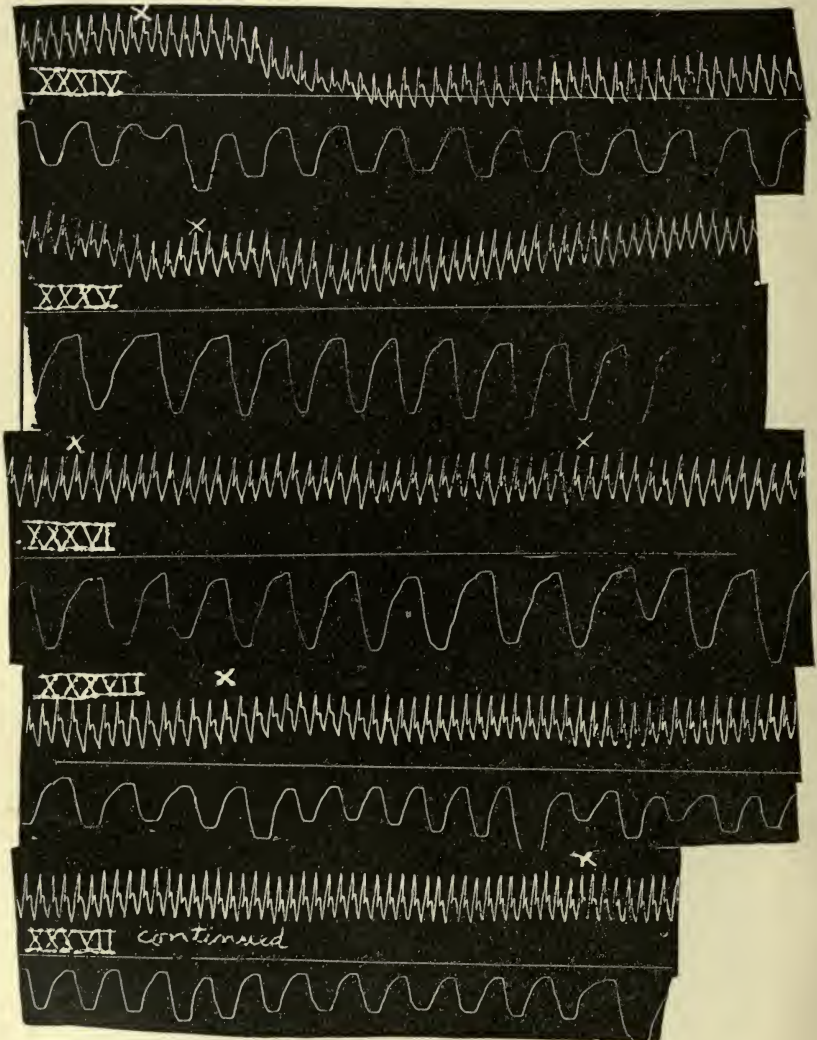
PLATE VII.



XXX., Addition; XXXI., Multiplication; XXXII., Emotion, thought of friend's illness; XXXIII., Noise.



## PLATE VIII.



XXXIV., Capsicum; XXXV., Knocking; XXXVI., Cross 1, Camphor; Cross 2, Rubber Cement; XXXVII., Addition.

nervously excited produces a much more disturbing effect, and makes the readjustment of attention a much more difficult matter, than the same stimulus would if the subject were calm. Fig. XI. is the curve of a cold stimulus which occurred during a state of emotional excitement. Fig. XXI. is also the curve of a cold stimulus, given at the same sitting, but at a time when the subject was calm. The difference in the effect of the two stimuli is entirely disproportionate to the slight difference there may have been between the absolute intensities of the two.

The breathing during sensory stimulations undergoes irregular changes in rate and amplitude, more or less analogous to those of the pulse. An experience intense enough to cause a profound change in one usually shows itself in the other also (see Fig. XI.). When the experience is less intense it sometimes produces an effect on the pulse curve, but none on the breathing, although the reverse seldom happens. On the whole, the breathing in cases of sensory stimulations is characterized by slight spasmodic irregularities, usually of short duration (Figs. XI., XII., XIII., XIV., XXV.). With the weaker and less effective stimuli these are often lacking, and sometimes fail even with the more intense experiences (see Fig. XVII.). As compared with emotional states, the disturbances of the breathing during sensory stimulation are of less frequent occurrence and of briefer duration.

The search for uniformity in classes or kinds of sensory stimuli is as fruitless as it proved to be in the emotional states. An overwhelming majority of sensory stimuli of all kinds, whether agreeable or disagreeable, caused vaso-constrictions. The few cases of pronounced vaso-dilation do not correspond to the distinctly pleasant stimuli. The most distinctly pleasurable stimulus used, harmony, caused constrictions on all of the few occasions when it was given. Unpleasant odors, such as camphor and capsicum, caused dilations on a few occasions. Figs. XXII. and XXIII. are the curves for two disagreeable odors, both capsicum, given within a few minutes of each other to the same subject. As the curves show, one caused a slight dilation and the other a somewhat greater constriction.

The amplitude changes of the pulse curve show no greater

constancy in the direction of change for different kinds of stimuli than does the vaso-motor level. There is a great preponderance of decreases of amplitude over increases for sensory stimuli as a whole, but here again there seems to be no particular significance in the direction of the change for different experiences. For instance, disagreeable odors cause sometimes increase and sometimes decrease of amplitude (see Figs. XXII. and XXIII.). Cold is sometimes accompanied by a decrease of amplitude (Figs. XI. and XIV.) and sometimes by increase (Fig. XXI.). The relatively few cases of harmony among our tests all produced an increase of the amplitude of the pulse curve, a fact which suggests increase of amplitude as a correlate of pleasant experiences, until we notice that discords have the same effect.

The rate changes of the heart-beat during sensory stimulations are about equally divided between increases and decreases. Cold, noise, odors—in fact, all the stimuli of which we have any considerable number of tests—cause sometimes one and sometimes the other in a manner which, on present data, is entirely erratic. The cold stimulation shown in Fig. XIV. causes a slight temporary decrease of the pulse rate, while that of Fig. XI. is accompanied by a progressive increase. Figs. XXIV. and XXV. show the curves for two camphor stimulations, the first of which causes an increase of pulse rate and the second a decrease.

The dicrotic notch changes its emphasis and its position with reference to the apex of the pulse curve with as little regard to the nature of the stimulus as is shown by the other factors. With one subject the dicrotic, when it suffers any change at all, undergoes an almost uniform flattening during sensory stimuli of all kinds. With the other subject it is emphasized almost as often as it is flattened. The position of the dicrotic remains unchanged during the great majority of sensory stimuli for both subjects. When it is raised or lowered it happens apparently without reference to the nature of the stimulus.

The mental application tests used were chiefly simple arithmetical problems given as fast as the subject could perform them. In a few cases the memorizing of a series of nonsense syllables was employed. The curves of mental application are characterized by the slight amount of the vaso-motor changes involved, and



by the even progression in which changes in rate and amplitude take place, when they occur at all. The vaso-motor level usually shows slight fluctuations, although they are always less than the fluctuations of revery for the same day. Frequently the changes are so slight as to be scarcely noticeable (Figs. XXVI., XXVII. and XXVIII.). The respiratory rhythms disappear.

The breathing is more regular in most cases, although there are some exceptions. In memorizing nonsense syllables the breathing curve is broken up by a tendency to pronounce the syllables. In some cases of mental application, such as Fig. XXXVII. there are occasional irregularities. The fact that the breathing is under voluntary control, and that it is immediately affected by any tendency to use motor images of words, would lead us to expect that the uniformities would be less evident in that case than in the case of the purely reflex vaso-motor phenomena. As compared with emotional experiences and with sensory stimulations, and even with revery, the bodily processes accompanying mental application are characterized by greater stability and regularity. If our psychological analysis is correct, mental application is a state in which the attentive process is most stable, runs most smoothly and offers greatest resistance to change. Here, again, the correspondence between the degree of stability of the attentive process and the degree of stability of the accompanying bodily processes holds.

But as in the former cases, so in mental application, the direction of the various changes which do occur, offers no basis of classification which articulates with the psychological classification, either into intellective as opposed to affective states, or into agreeable as opposed to disagreeable states. The subjects found no distinctly affective tone in the various mental application tests used. Since no psychological classification on the basis of agreeable and disagreeable is possible in this case, it would be absurd to interpret the vasodilations and constrictions as having such a significance. If the direction of the changes characterized intellective states as opposed to affective states we ought, of course, to expect to find some uniformity in the direction of change of mental application tests as a whole. But this is entirely lacking. In almost half

of the mental application tests the vaso-motor level shows both dilations and constrictions within a single test (Figs. XXVI., XXVII. and XXVIII.). Where the vaso-motor level changes in only one direction it seems to be an even chance whether it shall be a dilation or a constriction. Frequently there is no change of level. Fig. XXIX. illustrates a mental application test where there is only constriction and Fig. XXXVII. one where there is only dilation.

The amplitude of the pulse curve in mental application shows a greater tendency to decrease than to increase. In all of the few tests made, which exceeded two minutes in time, there was a marked decrease of amplitude at the end, even when there was an increase at the beginning. But among the tests of shorter duration there were several where the amplitude increased without any subsequent decrease. The pulse rate of mental application shows a greater tendency to increase than to decrease, but the cases of decrease of rate, although less numerous than those of increase, are frequent.<sup>1</sup> But, whatever the direction of the change, it takes place slowly and gradually. (See Fig. XXXVII. for increase of pulse rate and Fig. XXXI. for decrease of rate.)

The rate and amplitude of the breathing curves change in contrary directions during mental application in quite as erratic a manner as the pulse curves.<sup>2</sup> One subject shows an almost uniform increase of breathing rate, while the other has a few more cases of decrease than of increase. With both subjects the amplitude is more often decreased than increased, but there are frequent cases of increase. (See Fig. XXVII. for increase of breathing amplitude and Fig. XXXVII. for decrease.)

As a summary of the results of these experiments, we can offer nothing better than a series of tests illustrating each of the different types of processes from curves obtained at a single sitting of about an hour's duration. There is first the emotional experience of the sudden thought of a friend's illness (Fig. XXXII.),

<sup>1</sup> Most investigators report much greater constancy in the cases of increase of rate in the heart under these conditions. Certainly it is the most usual occurrence.

<sup>2</sup> MacDougall and the French writers report increase in rate and decrease in amplitude as constant. Delabarre's observations suggest a considerable difference in individuals in this respect.

with its marked fall in vaso-motor level and its irregularity in all the features of pulse and breathing curves. Next comes the startling noise (Fig. XXXIII.), which involves as sudden and violent a shift of attention as the emotion and produces a very similar curve. Next in order is a disagreeable and annoying odor—capsicum (Fig. XXXIV.). In this case the vaso-motor fall is less, though still very evident, and irregularities in amplitude and rate are decreased. The slight disturbance caused by an unexpected knock at the laboratory door, makes still less of a fall in vaso-motor level, but yet shows other irregularities (Fig. XXXV.). The odors of camphor and rubber cement, which were not at all annoying unless strong, produce no marked change in the curve (Fig. XXXVI.). The slight shifts of vaso-motor level, and slight irregularities of rate and amplitude, resemble closely those of the preceding state of revery. No strong demand for attention is made by them. Finally, mental application (Fig. XXXVII.) produces a steady strain of attention, which is accompanied by a curve practically devoid of fluctuations in vaso-motor level, with an amplitude which is almost constant, and a slowly, progressively increasing rate.

All the processes with which we have been dealing are cases of readjustment of an organism to its environment. Attention is always occupied with the point in consciousness at which the readjustment is taking place. If the process of readjustment goes smoothly and evenly, we have a steady strain of attention—an equilibrated motion in one direction. The performance of mental calculation is a typical case of this sort of attention. But often the readjustment is more difficult. Factors are introduced which at first refuse to be reconciled with the rest of the conscious content. The attentive equilibrium is upset, and there are violent shifts back and forth as it seeks to recover itself. These are the cases of violent emotion. Between these two extremes comes every shade of difficulty in the readjustment, and of consequent intensity in emotional tone. We have attempted to show in the preceding paper that the readjustment of organism to environment involves a maintenance of the equilibrium of the bodily processes, which runs parallel with the maintenance of the attentive equilibrium, and is an essential part of the readjustment of the psychophysical organism.



## PROFESSOR MÜLLER'S THEORY OF THE LIGHT-SENSE.<sup>1</sup>

BY CHRISTINE LADD FRANKLIN.

Professor Whitman, in his address as Vice-President of the Section of Physics, has given an admirable account of the present state of discussion upon the color-sense, as far as it regards the theories of Helmholtz and of Hering, and he has also devoted much time—more, perhaps, than they deserve!—to the modifications of those theories which have been made by Ebbinghaus and v. Kries. He has, very politely, refrained from anticipating what I have to say by giving an account of the subject of this paper, the theory of Professor G. E. Müller, a theory which, in my opinion, deserves to be put quite in the front rank of the various attempts that have been made to account for the color-process of the retina. I regret very much that this paper of Professor Whitman's was given before the physicists at an hour when it could not be listened to by the members of this Section, for it contained a very clear account of the recently discovered facts of color-vision, a knowledge of which, on your part, would perhaps have lent something more of interest to my discussion of the theory of Professor Müller.

This theory is set forth in four papers which have been printed in 1896 and 1897 in the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*; these papers cover some two hundred and fifty large pages, and form therefore practically a book on the subject. The appearance of this volume, as it may properly be called, marks a real epoch in the long discussion that has been going on in the effort to reduce to order and system the phenomena of the sensation of light. Its author has shown a remarkable mastery of the immense mass of facts which have a bearing upon the case, and a no less remarkable keenness

<sup>1</sup> Read before the Section of Anthropology, A. A. A. S., August, 1898.

of logic in attributing to them their proper weight in the discussion of theoretical considerations. Professor Müller won his spurs, as a young man, by his very acute treatment of the fundamental problems of psycho-physics; his reasoning processes are of a far more rigid character than those which are usually consecrated to the subject of color-vision. His theory presents great points of superiority over the Hering theory, and it is by far the best attempt that has yet been made to construct a theory based upon the assumption of antagonistic retinal processes for the colors and for white and black.

Professor Müller himself modestly refers to his theory, in many places, as merely a modification of the theory of Hering; at other times he speaks of it plainly as *die hier vertretene Theorie*, in distinction from that of Hering. There is no question that the latter designation is the correct one. When a theory has some points in common with another it is difficult to know just where to draw the line between regarding it as the same and regarding it as different—it is impossible to lay down any general rule that shall cover such cases. In regard to the extraordinary fact of color vision—so totally unlike anything that happens in sound or in any other quality of sensation—that when certain two colors, which are neither particularly alike nor particularly unlike, as far as one can tell beforehand, are seen together, they both absolutely *disappear* from consciousness, that their place is taken by a plain undifferentiated gray—in regard to this fact<sup>1</sup> there are, in two different respects, two different lines of explanation. In the first place, this extraordinary fact of disappearing color-pairs may be a matter of physiology or it may be a matter of psychology,—that is, it may be (in the latter case) that it is the judgment, or rather the imagination, which causes us to lose all sense of color in the sensation of white, or it may be, on the other hand, that the loss takes place in some lower stratum of the passage from external light to the

<sup>1</sup>It is an act of discourtesy to the adherents of one or the other of the two great schools of color-theorists to call such color-pairs as this either *antagonistic* or *complementary* colors, for either term commits us at once, of course, to an opinion as to the intrinsic nature of the processes which call them forth. The difficulty can be avoided if we refer to such a pair of colors as a *disappearing* color-pair.

final sensation—for instance, in the photo-chemical process, whatever it may be, of the retina. In each of these two cases the loss of color may be of the nature of a *composition* into a resultant gray, or it may be of the nature of an *antagonism*, and a suppression of color, with the re-emergence of a gray which was present all the time in a state of abeyance or of concealment.<sup>1</sup> The only psychical theory that has been proposed hitherto is the Young-Helmholtz theory; but there might equally well be a psychical antagonistic theory, in which, by the action of the mind, the colors of a color-pair, upon proper occasion, destroyed each other. This theory would have quite as much reason in its favor as the psychical complementary theory of Helmholtz, and it is merely by an oversight, no doubt, that it has not yet been seriously proposed.

If the cause of the disappearance of color is physiological, it may, as I have said, be either of the nature of a re-composition of the several constituents of white, or of the nature of an antagonism, and a suppression of color. Now the theory of Müller belongs to the same one of the four possible classes of theories as that of Hering—it is physiological and antagonistic. But that is not enough to make it the same theory. If the assumption of four antagonistic colors and a separate process for black and white were sufficient to characterize a theory, then the theory of Hering would not belong to Hering, for all that was maintained by Mach for ten years before the appearance of Hering's first paper on color-theory. What *is* distinctive of the Hering theory is the assumption of assimilation and dissimilation as the bases of the antagonistic sensation-pairs. For this Professor Müller substitutes the conception of 'reversible chemical action,' and he shows conclusively the utter inadequacy of the processes of assimilation and dissimilation to play the part required of them. (Aside from all other difficulties, they are not even antagonistic processes; dissimilation does not inhibit assimilation, but, on the contrary, the more rapidly any tissue is being used up, the more quickly does nature hasten to restore

<sup>1</sup> Thus there are four possible classes of color-theory—the psychical and the physiological compository theories (or complementary theories) and the psychical and the physiological antagonistic theories.



it.) This difference in the character of the antagonism concerned is very fundamental—so much so that in the one case (that of the assimilation and dissimilation of Hering) it is quite impossible to accept the theory based upon it, while the other theory (that of Professor Müller) is not to be so lightly brushed aside, but, on the contrary, gives room for serious discussion. I have, therefore, no hesitation in designating the theory of Professor Müller as a *new* theory, although it is a theory belonging, like the theory of Hering, to the class of physiological (instead of psychical) theories, and to that of antagonistic (instead of complementary) theories.

Professor Müller's paper begins with an acute discussion of the doctrine of psycho-physical parallelism. He sets forth the several axioms, five in number, into which that doctrine may be resolved. He shows that Hering has violated these principles in a fundamental manner, in assuming that the quality of a sensation of gray depends only upon the *proportion* of black and white which enter into it, while their absolute amounts may vary to any extent without affecting sensation. This palpable defect in the theory of Hering is removed by Müller by the assumption that the effect of light upon the black-white photochemical substance is such that whenever more white process goes on, by just so much there is less of the black process, and hence that a given *proportion* does not, as matter of fact, occur with different absolute amounts of the two elements which make it up; if it did, we should see a given *quality* of gray in various different degrees of *intensity*, what is not the case.

Another application of the principles of psycho-physical parallelism is made by Professor Müller to determine what he calls a psychic quality-series. Sensation is subject to ceaseless change; we assume it as self-evident that we are capable of distinguishing whether a sensation is varying *in a constant direction*, or not; if, for instance, we are making a purple light out of a physical mixture of red and blue, we shall assume that we can distinguish between the several cases, whether the operator is adding always more and more blue to the mixture, whether he suddenly begins to add more red, or more white, or more black; if the sensation is growing steadily more and more *like*

a distinct *other* sensation, the series shall be called a series which varies in a constant direction; in this case we are required to assume that the underlying physiological process which immediately precedes consciousness is also in some sense a series which varies in a constant direction; this may be a constant change in quality (a change in a vibration period, for instance), or it may be a constant change in the relative intensity of the two elements of a mixture. Professor Müller shows, by an exhaustive piece of reasoning, that in the case of the color series there is every reason to believe that the latter is the case, that the physiological process is a varying mixture of two processes. This is, in fact, the only supposition that is possible if the physiological process is of the nature of a chemical change (for a chemical change is not capable of a very large number of different qualities); and that it *is* of the nature of a chemical change there seems to be very little reason to doubt. Nothing else would be at all possible except an electrical process, and that, if it took place, would be due to a preceding chemical effect; so it is simpler, in the absence of reasons to the contrary, to assume a chemical effect only.

The question which next arises is this,—and it is a very important one. Does the whole gamut (a circular gamut) of saturated color-tones correspond to one or to more than one, and if to more than one, then to how many *psychic quality series*, in the above sense of the term? It is plain that there are *four* such series in the whole congeries of color-tones. The end members of the series are the fundamental colors, red, yellow, green and blue. Four fundamental color-tones have been assumed before, but many psychologists have objected to the grounds upon which both Mach and Hering urged the claims of these four colors to their exceptional position. They have said that in looking at an orange color, for example, it is impossible to extract from it the red and yellow of which it is composed, in the same way in which one can hear in a chord its separate notes; and violet does not ‘remind’ us, they say, of red and blue any more than red and blue remind us of violet. Professor Müller concedes that when we look at the colors *singly* they appear all to be of equal dignity. By simply looking at a

given color, we cannot tell whether it is a mixture or more or less nearly an elementary sensation. The colors must be arranged in quality-series; then the difference between the end members and the intermediate members of these series becomes distinct. Are a given set of quality changes proceeding in a constant direction or not? The colors of the spectrum are not well adapted for obtaining the answer to this question—they differ too much in brightness. It is better to take a complete series of papers, or, better still, of gelatine sheets, which can have their brightness regulated by putting different grays behind them. Then, if one looks on dispassionately, one cannot help saying to oneself: When I pass from red through orange to yellow the change in the quality of the sensation proceeds constantly in the same direction; so when I pass from yellow through olive to green. These are, in the above sense of the term, psychic quality series. But when I pass from orange to yellow and from yellow to olive, or from olive to green and from green to blue-green, the second part of the change is *not* in the same direction as the first—these color-sensations are *not* members of one and the same quality series. For a person who cannot perceive the difference that is here insisted upon, there is indeed nothing to be done; but to the unprejudiced observation it may be confidently predicted that, if we put in say five, or seven, color-tones between purple and orange, through the reds, and also between red and yellow, through the various tones of orange, the difference in character of the two series will ‘spring into the eyes’ with perfect distinctness.

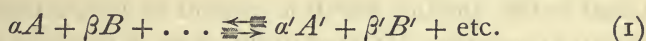
All Professor Müller's discussion of this subject, of which I can give only a brief indication here, I regard as excellent, and I adopt it bodily as a firm substratum for my own theory. But, unfortunately, it does not fit Professor Müller's theory well at all. In order that the four colors which a careful inspection of our sensations tell us are fundamental may also be antagonistic (or, in the phrase of the other theory, *complementary*), it is necessary to manipulate those colors a little. The blue and the yellow which consciousness tells us are end-members of psychic series are, it is true, also complementary; but green (in any ordinary signification of the word) is not complementary to red



at all. In order to make things fit, Hering is obliged to assume as green a color which the unbiased eye would pronounce to be a distinct *blue-green*. (As confusion is constantly arising from the fact that the simple word *green* has two totally different significations, according as one is speaking in the language of Hering or of Helmholtz, I propose to modify the spelling of that green which is in reality a blue-green, and to write it *grehn*, meaning by this word *the green of Hering*. It is then *grehn* and not *green* which is the complementary color to red.) Now, this beautiful structure of the psychic quality series goes all to pieces as soon as we attempt to fit the Müller-Hering theory to it; the *grehn* which is the complementary or antagonistic color to red is *not* an end-member of a psychic quality series, but it is a member which comes very distinctly in the middle of such a series. While then this whole discussion of Müller's gives my theory a firm standing-ground against those psychologists who profess not to be able to see that orange is a mixture, it works immediate destruction to the theories of Müller and of Hering.

The central idea of the theory of Professor Müller is, as I have said, that of a reversible chemical action. (Assimilation and dissimilation are *opposite* chemical actions, in a sense, but they are not reversible—the products of dissimilation are not reformed into the original tissue.) Professor Müller places at the head of his presentation of his theory this passage from the *Theoretical Chemistry* of Nernst: "We were formerly of the opinion that the reversible reactions belonged to the exceptions, or that it was necessary to distinguish between two different classes of reactions, the reversible and the not-reversible. But we know now that a sharp limit of that kind is wholly non-existent; there can be no doubt that, upon proper arrangement of the experiment, every reaction can be caused to proceed now in one and now in the other direction—that is, that every reaction is, in principle, reversible." This very modern idea of the chemist furnishes the basis for Professor Müller's conception of the antagonism between the fundamental retinal processes, and he refers here not only to the color-pairs, red-grehn and blue-yellow, but also to the connected

pair of sensations, black and white: that is, he conceives that the 'proper arrangement of the experiment,' which Nernst says is the only condition necessary to make chemical reactions reversible, has been secured for the vertebrate retina. Confining himself at first to the black-white pair of sensations, in order to facilitate speaking about them, he goes on to *préciser* his conception of the retinal chemical process at the base of them in these terms. A white-reaction, expressed in quite general terms, consists in this: that  $\alpha$  molecules of a substance  $A$ ,  $\beta$  molecules of a substance  $B$ ,  $\gamma$  molecules of a substance  $C$ , etc., come together in order to form  $\alpha'$  molecules of a substance  $A'$ ,  $\beta'$  molecules of a substance  $B'$ ,  $\gamma'$  molecules of a substance  $C'$ , and so forth. And then a black-reaction consists in  $\alpha'$  molecules of  $A'$ ,  $\beta'$  molecules of  $B'$ , etc., returning to their original places so as to form again  $\alpha$  molecules of the substance  $A$ ,  $\beta$  molecules of  $B$ , etc. This chemical reaction may be expressed in this form:



If this is read from left to right it is a white-reaction; if it is read from right to left it is a black-reaction.

Nothing, of course, is to be said as to the actual degree of complexity of these reactions, that is, as to the number of the substances, on the right hand and on the left, which are involved in them; they may be numerous, or they may be one only. In any case the total amount of the substances  $A$ ,  $B$ ,  $C$ , etc. constitutes the white-material which is present, and the amount of the substances  $A'$ ,  $B'$ ,  $C'$ , etc. constitutes the black-material which is present; the separate substances,  $A$ ,  $B$ , ...  $A'$ ,  $B'$ , ... are the components of the black and of the white material respectively. For a chemically homogeneous portion of that layer of the retina which is sensitive to light, the *intensity* of the white process will be equal to the number of white reactions which take place in unit volume of the retinal substance during an element of time. The law of mass action, which plays an important rôle in his theory, will then be exhibited in this form:

$$I_{10} = \frac{K_{10} \alpha^{\alpha} \beta^{\beta} \gamma^{\gamma} \dots dt}{v^{\alpha + \beta + \gamma}}$$

where  $a, b, c, \dots$  are the masses of the photo-chemical substances (expressed in gram-molecules)  $\alpha, \beta, \gamma, \dots$  are the numbers of these present in a given portion of the substance,  $v$  is the volume of that substance, and  $K$  is a coefficient, depending upon the temperature and other factors, which may be called the specific velocity-coefficient of the white-process. We have a similar expression for the intensity of the black-process. But as there is no occasion for concerning ourselves with the separate components of the black and the white material, we may write these equations

$$I_w = K_w M_w dt,$$

$$I_b = K_b M_b dt,$$

with similar expressions for the two pairs of color-processes. This is simply the mathematical expression of the fact that the intensity of any photo-chemical process in the retina is proportional to the amount which is present of the material of the reaction in question. Stated in this way, it does not seem to be a proposition which any one need hesitate to admit.

It will be seen that the theory of Professor Müller, in making use of the ideas of the *reversible chemical process* and of the *effect of mass-action*, is a theory of the very highest fashion. Let us see in detail how he overcomes the discrepancies which exist in the theory of Hering. A chief objection to the view of Hering, for those who have been interested in its theoretical aspect, is the inconsistency which meets us at the very beginning; why should black and white be regarded as an *antagonistic* sensation-pair, when they do not destroy each other, but give us, on the contrary, the whole series of grays? Professor Müller, in his general rectification of Hering's theory, has devised an ingenious means of meeting this difficulty. He assumes that the black and white chemical processes *do* exactly neutralize each other when they take place in equal amounts *in the retina*, but that there is a continuous black-white excitation going on in the *cortex* (unaccompanied by any color-excitation), that that is the cause of the so-called self-light of the retina—the faint gray sensation which we are never free from though the eyes are closed—and that any gray excitation sent up



from the retina is added to or subtracted from this endogenous cortical excitation, corresponding to an antagonistic pair of colors, according as the black or the white predominates in it. If the black and white processes are equal in amount, no additional excitation of the brain is sent up from the retina, and the self-light is the only sensation experienced. But this is to introduce a fresh difficulty as serious as the one which it is attempted to remove. Why should not the yellow-blue substance of the cortex be subject to a continuous excitation as well as the black-white substance? If the two pairs of chemical processes must be so unlike in the end, why take the trouble to make them so like in the beginning? The situation is this: for *sensation*, black and white constitute a psychic-quality series (in the above defined sense of the term), while yellow and blue do not; as regards the *assumed chemical processes*, the one for the yellow-blue series is finally antagonistic in the retina, while the one for the black-white series must have a distinct and additional process which can be superimposed upon it in the cortex. Is it not much simpler to admit once for all that black and white do *not* stand to each other in the same relation as yellow and blue (nor as red and grehn)? Why force them into an unnatural resemblance which they must immediately afterwards be despoiled of? Why introduce a wholly unnecessary difficulty into a theory merely for the sake of showing with what ingenuity it can afterwards be done away with? This whole construction of chemical processes is at best purely an imaginary one; absolutely the only virtue that it can have is the virtue of consistency. The idea of black and white occupying the same place, in a proposed system of explanations, with a disappearing color-pair is, *von vorn herein*, most causeless and most unfortunate, and no amount of bolstering up by subsidiary hypotheses can make it anything else. Professor Müller's real ground for executing this *tour de force* is that he conceives it to be demanded by the phenomena of contrast, successive and simultaneous. The fact that unless black and white are regarded as a perfectly congruent sensation-pair with yellow and blue (and red and grehn), the explanations of contrast and of after-image do not run exactly *pari passu* for colored and for colorless sensations, is

regarded by Müller as a fatal objection to every theory except his own and that of Hering. As matter of fact, Helmholtz gave too much importance to the psychical elements involved in contrast, and we are all now convinced that the correct explanation is to be looked for along physiological lines—a conviction which we owe to Professor Hering and one for which our gratitude is due him. But every explanation of contrast which can be given in the language of Hering and of Müller can be given just as consistently in the language of any of the complementary color theories; it is true that it will read a little differently according as the contrast to be explained is one of colored or of uncolored surfaces, but that, instead of being a defect, is altogether a point of merit; since the grays do not constitute a series of the same nature as the mixtures of yellow with blue (or of red with green), it is a feature of extreme ineptitude when their contrast-effects are explained in exactly the same language.

An admirable chapter in Professor Müller's work is his discussion of the visual purple, as it was unfortunately named, since it is now known pretty positively that it is not a chemical basis for *vision* any more than it is *purple* in color, in the English meaning of that term. Professor Müller adopts my assumption that the rod-pigment (as I prefer to name it) is a secondary means for securing adaptation to a faint light, and not directly a vision-producing substance at all; I suppose that it acts by absorbing (for the purpose of re-enforcing faint-light vision) a large amount of the light which usually passes entirely through the transparent rods and cones to be lost in the choroid coat, and Professor Müller takes it as acting as a sensibilisator, in the photographer's sense of the term. In Professor Müller's assumption, its *color* has no significance. In mine it is of great importance; it is adapted to aiding vision in the gloomy depths of forests, because green light is the light which it absorbs; and fishes, which alone, of all vertebrates, have a rod-pigment of a distinctly different color, are exactly fitted for utilizing the last rays of the light which penetrates deep into the water of the sea. v. Kries opposes this view, and he is led thereby into countless inconsistencies and contradictions (especially in regard to the gray-vision of the periphery of the normal

eye). He goes so far as to regard the rods as functionless whenever they lack their purple coloring substance.<sup>1</sup>

The progress of our knowledge of color-blindness forms one of the interesting chapters in the history of science. The Young-Helmholtz theory of three (instead of four) fundamental colors having been taken as *fact* instead of theory, the conclusion was jumped at that a defect in color-vision must consist in the absence of some one or more of these three colors, and the common forms of the defect were described as red-blindness and as green-blindness. By a very brilliant piece of reasoning, it was discovered, about 1850, by William Pole, F.R.S. (professor of Engineering in University College, London, and author of the well-known work on whist), who is himself color-blind, that as matter of fact the sensations experienced in both cases are blue and *yellow*, instead of blue and red or blue and green. This discovery did not awaken as much interest as it ought to have done, and the reasoning by which it is established, cogent as it is, did not prove convincing to all; Sir John Herschel said, "What the sensations of the color-blind really are we shall never know with certainty." It happened not to occur to him that there was a possibility of some individual being color-blind in one eye only. It was only after the true state of the case had been put wholly beyond question by several cases of monocular color-blindness that it became matter of common knowledge—in fact, it continues to be most unaccountably ignored in England to the present day. This fact, that the senses for red and for green are lost together and that the color-

<sup>1</sup> It has lately been affirmed by Sherman and others that there is a certain amount of change in the relative brightness of the different portions of the spectrum for the rodless regions of the retina when the light is diminished—that is, that the Purkinje phenomenon is not wholly wanting there—an amount so small however, that it has been denied to exist by other observers. This would be a fact very damaging to the assigning of the adaptation-function to the visual purple, if it could not be explained. It can, however, be explained very easily. Kühne, who is still the chief authority on changes in the photo-chemical substances of the retina, expressly states that the yellow coloring matter of the macula is subject to a slight change in quantity with a changing intensity of the illumination; the change in relative value of the blue sensation which is affirmed to exist by Sherman is, therefore, just what is needed to parallel this physiological variation in the amount of a colored substance which absorbs blue light.



vision which persists in these cases is vision for blue and yellow, was taken, and very properly, as working immensely to the good of the theory of Hering. But immediately it appeared that the situation was not so simple as it had seemed to be; while the warm end of the spectrum is seen by all alike as yellow, nevertheless there are still two classes of these defectives as regards *what part* of the warm end of the spectrum is seen to be of the *brightest* yellow; and these classes are totally distinct—there are no intermediate cases. It is as if red-vision had fallen out and green-vision had been turned into yellow-vision for the one sort; and for the other sort it is as if green-vision had fallen out and yellow-vision had taken the place of red-vision. Hering denied for a long time that there is anything in this difference, more than can be explained by individual differences in the yellow coloring matter of the macula. But Müller admits the fact, and endeavors to account for it. The red light of the spectrum, he assumes, besides its effect on the red-green substance, may have also an effect on the yellow-blue substance, and it may even have two such effects—it may act upon it in the first place directly, by producing out of the decomposition of the red-green substance some one or more of the constituents of the yellow material (with which, in the original form of the hypothesis, red light had nothing to do). The first type of the red-green blind—those formerly called red-blind—are totally lacking in the red-green substance; these are the typical yellow-blue visioned. But the second type—those formerly called green-blind—see yellow in the place of both red and green for some totally different reason—either because the nervous fibres which conduct the retinal excitation are not of the normal constitution, or because some still other constituent which is usually found already prepared in the retina is now absent. In this fashion it will be seen that the so-called red-blind lack all the *indirect* effect of the light of the spectrum upon the yellow-blue substance, while that indirect effect still persists for the green-blind. It is plain that this is an explanation which is complicated and far-fetched in the extreme.

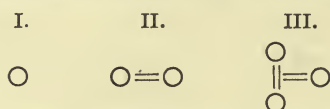
The value of a theory which is offered as explanation of any series of connected facts can be determined only by the

method of comparison with the other theories which it is proposed that it should replace. Any theory is better than no theory, and the theories of Newton, Young, Helmholtz and Hering have filled a useful function in giving direction to the immense amount of work which has been expended upon this subject in laboratories. But as soon as theories are thought out which offer a more probable and a more natural conception as to the nature of the unattainable link between external light and conscious light-sensation, then the usefulness of the provisional theories is at an end. It has been said, within a few years, by the Vice-President of this Section, who is also one of the editors of the *PSYCHOLOGICAL REVIEW*, that the best theories now in the field are the theory of Donders, that of Wundt and my own; in this opinion I heartily concur! In all of these theories the attempt (which is fore-doomed to failure) to represent the black-white series as of the same nature as a self-destroying color-pair, although it is plainly a non-self-destroying pair of sensations, is given up. Black is regarded as a sensation which is connected with white not differently from the way in which it is connected with blue or with green, or that in which blue and green are connected with each other. By this means the ingenuity which is required to explain the fact that black and white do *not* destroy each other is rendered unnecessary—there is no reason why they should destroy each other, for they are in no sense antagonistic. These three theories are all of the physiological type: the cause of the mutual destruction of a color-pair is in the retina and not in the imagination. Two of them are of the component type; the other (that of Wundt) is of the antagonistic type. Wundt, as matter of fact, assumes more than four elementary colors, but there is no reason whatever why he should do so. If Wundt were to accept Professor Müller's demonstration of the fact that there are, for consciousness, just four—no more and no less—elementary colors, and if he were to translate into the language of his own theory all the Hering-Müller explanations of contrast and after-image (which are, in reality, not explanations at all, in the correct sense of the term), his theory would be an admirable one.

My own theory possesses the simple advantage that it is able

to assume all the good points of all the other theories, and to avoid all their bad points! It takes on, for instance, the whole doctrine of the color-triangle, which is a *bête noire* for Hering, but which nevertheless is the expression of a vast body of *fact* in the domain of color-mixture; and it takes on the whole new doctrine of the perceptible psychic quality series of Müller, which renders absolutely necessary the assumption of four elementary colors instead of three, but which is destructive to Müller's own hypothesis, because the green which it takes as elementary cannot possibly be *both* an end member of a psychic-quality series and the antagonistic color to red.<sup>1</sup> Moreover, there is no explanation of any fact of contrast which is given by the theory of Müller which cannot be given just as well by my theory and in quite parallel terms. The idea of a partial chemical decomposition is in no sense more speculative than that of a reversible chemical process. A reversible chemical action has no distinct analogy in any other known physiological

<sup>1</sup>While my theory assumes four fundamental colors, red and green are nevertheless not complementary. I represent diagrammatically the progressive development of a color-substance in the retina in this way:



I suppose that a primitive undifferentiated substance is composed of molecules which (though they may be of any degree of complexity) are indifferently completely destroyed by light of every wave-length; thus in a later stage this substance consists, in each of its molecules, of two distinct parts, one fitted to be shaken to pieces by light from the warm end of the spectrum and the other by light from the cold end of the spectrum; and that in a third stage of development the yellow-producing constituent is in its turn broken up into two parts of such different internal vibrative periods that they respond respectively to the red light and the green light of the spectrum. (It cannot be said that there is anything repugnant to the ideas of the chemists in this representation of molecules; this very diagram III. has lately been used in another connection as a picture of a molecule by Duclaux.) Partial color blindness is an atavistic condition in which the second stage of development is permanent, and in total color-blindness the entire color substance remains in the primitive condition in which gray is the only sensation produced. Blue and yellow are complementary colors, but red and green, when acting in conjunction, re-compose the yellow-producing substance out of which they have been developed, instead of together making white. In other words, the fundamental green of this theory is a real green, and not the evident blue-green of Hering.



process ; but we have not far to go for an analogy for a partial chemical decomposition—the retina itself presents in the rod-pigment (which is first broken down into a yellow substance and then into a colorless substance) exactly the analogy which is required. And this is not all : it seems to have been made out by Hamburger and by Köttgen and Abelsdorff that this decomposition in two successive stages occurs in no animals lower than man ; this is an instance of a progressive differentiation of function in an adjunct photo-chemical substance which is exactly what is needed to form a parallel case with the development of a color-substance subject to a partial decomposition out of an undifferentiated gray-substance, which is what my theory requires.

## SHORTER CONTRIBUTIONS AND REPORTS.

### PROFESSOR GROOS AND THEORIES OF PLAY.

Professor Groos's book on *Animal Play*—to which renewed attention is given by its appearance in English dress—invites discussion and criticism from several points of view. In the first place, we must regret that human play is deferred for separate treatment, for assuredly the child is preëminently animal in its plays. A monkey which delights in snapping open a match-safe and a child which does the same are at the same stage and should be treated together. To divide plays into animal and human, and animal, into plays of birds, mammals, etc., has little significance.

Professor Groos sets forth elaborately the practice theory of play. Play is the expression of developing instinctive tendencies, an anticipatory, tentative practicing, which accomplishes no immediate serious service. Thus the kitten pounces on the straying leaf, and so practices for pouncing on its prey. We may express it by saying that Nature here shows her prentice hand, or that here is Nature's school, but no forced attendance, no specified time, no set lessons, and only free, spontaneous, pleasant activity. Under natural selection this play period of instinct has been developed, wherein the energies of the protected young act in non-serious forms in preparation for mature life.

To this theory of play we must object that instinct as such needs not practice, and again that instinct fully formed at birth is the more advantageous. If the kitten could at once seize a mouse as a chick does a grain of wheat, it would be much to its advantage. If the prey of cats were of definite size, color and motion, and always appeared at the same distance, an instinct would work at the first occasion; but as it is, instinct cannot cover the varying complexity, but intelligent play practice is called in. The kitten has the instinct to spring, but the regulating for size, distance, etc., is acquired intelligently. The learning may be by hereditary impulse, but yet it is better to define play in terms of intelligence rather than in terms of instinct.

But much of the developing activity of animals can scarcely be brought under the term play as psychic. Thus the young bird, flut-

tering and trying its wings, seems not playing, but serious, as also the child taking its first steps. But when the bird becomes skilled in flying, it flits playfully, and when the child is able to run easily, it delights in running plays. A large proportion of Professor Groos's examples of play fall under work. The imperfect, immature, practice is not thereby play, as, for instance, the calf sucks your finger as seriously as it does the teat. Real sucking play we see in children sucking through straws. Play belongs to childhood rather than to infancy, though both are full of developing activities.

We must, then, dissent from bringing all the inceptive preliminary activities of young animals under play. Further, it is not proved that play as fact of biologic history originates in these activities. Has it yet been shown that play does not appear first as life method among organisms adult from the start, long before a youth period emerges? Some micro-organisms seem to swim playfully, and a sham alarming and attacking is seen among adults low in the scale of life. It is quite conceivable that play started with adult practice, and was conserved and developed through natural selection, at length becoming most prominent in earliest phases of life period, and so making the youth time. The adult practicing among his mates must be harmless, and so easily became sportive, that is, practice-work became play, a pleasing method of unreality.

Another outlook for the origin of play is in the one-sided form, teasing. A very combative animal, having no enemy to fight, will be led to attack its mates, but not violently, as that would break up all its associations, but teasingly. This tendency is so strong in the horse that we have the term 'to nag.' The tricks of boys and the chaffing and practical jokes of men are plainly a low form of play, and perhaps point toward the primitive form. The scare game is also a very popular and crude play with animals and children. To rouse real though groundless fear gives an aboriginal delight in sense of power both to achieve and deceive. After a time the teased one would learn that its best defense is not to resent, but to play back, and hence arises full, two-sided, mutual play. The teasing hypothesis of the origin of play is, like practice of young or adult, a possible mode which ought to be kept in mind by the investigator.

One point which deserves notice in this connection, but which does not appear to be touched on by Professor Groos, is the relation of the play of domestic animals to the play of their wild congeners. Though the dog has been domesticated for millenniums, its play is wild and wolfish, and so contrary to practice for its adult life. The colt



in its play rears, kicks and bites—wild habits which have to be overcome in its domestic life. The colt does not play at drawing loads, nor the collie at driving sheep. Selection by man is but a kind of natural selection, and we might expect some preparatory play, or at least some reduction in the period of wild play. But is this the case? Though young domestic animals do not have tame plays, yet they may play at work taught by man, as the elephants at Bridgeport sometimes playfully practice standing on their heads. It is probable that many wild animals are more playful than their tame congeners largely because intelligence and alertness are more required in the wild state. Wild sheep are a case in point. But if man had bred the sheep for intelligence rather than wool, would not the domestic sheep be more playful than the wild? Some breeds of dogs, being bred for their playfulness, are doubtless far more playful than any wild dogs. But what we need here, as everywhere on play, are data.

Another point which is not considered by Professor Groos is the close relationship of play and work, which requires a study of both to understand either. The life of animals and children is a complex of play and work, a rapid passing from one to the other. Man, adult and civilized, is the only persistent player or worker. Birds in building or nesting seem to be continually interrupting play with work and work with play. A boy begins to pile wood in the shed as play, but as soon as it is felt to be work he ceases, unless you offer a wage. The start is often play, but the continuance work. In most plays the pressure of companions makes some continue playing against inclination, that is, makes them work. From dead earnest to pure play is a long series of mergent psychoses. Pure play and work are rare, most activities being merely dominantly one or other.

We have noted that preparatory activity of young is not always play, and *vice versa*. Thus the fledgling, coaxed and compelled in its learning to fly by its parents, is plainly working, and the colt rearing, kicking, and doing just what he must not do as adult, is plainly playing. Now, both reversion and recapitulation are practically ignored by Professor Groos. But it is certainly worth inquiring whether play tendencies of survival origin may not exist, say among monkeys. Is not there a general psychic embryology which has play forms? The place for reversion in the play of youth is evident in such actions as climbing, swinging, playing with stones, animal plays and plays with animals, deceit plays—civilization is founded on truth—hunting, fishing, camping. That these latter sports appeal little to the gentler sex may be due to the inherited reminiscence of camp

drudgery of savage life, and the fact that the excitement of the chase was not then their part. Indeed, anticipatory play is more common with woman than reversionary, because reversion is mainly joyless. If the work methods of past life are the plays of to-day's life, the works of to-day will be the play of 'beyond-man' millenniums hence.

If we divide all conscious activities into work and play, we have a difficulty in distinguishing them in the *plexus* of life. Take the commonest actions in daily routine, as the morning toilet. Here, many are merely mechanical, and so neither work nor play. But so far as consciousness enters it is generally work for men and play for women. In walking down town one will often go several times through the variations of work, play and the mechanical. But play is constantly emerging in life as activity for its own sake. Thereby it is amateurish, is not a means, and has no wage. Is then pleasurable work play? Professor Groos remarks (p. 253) that bird courtship, while being set a 'real end, yet may have the psychological aspect of mere play,' because of 'satisfaction' in the exercise. But we think that 'satisfaction' applies to work. Work-pleasure is satisfaction in results achieved by effort, but play-pleasure is not satisfaction; rather it is the immediate, fleeting, inherent pleasure in the act itself. Fresh, free, joyous spontaneity is the mark of play, which begins, ceases, re-begins when, where and how it pleases. And play-pleasure has no form of its own, but is that of power, skill, competition, possession, etc., that is work-pleasure, so pointing to work as the origin of play.

Professor Groos's definition of play, 'instinctive activity exerted for purposes of practice or exercise, and without a serious intent,' begs the question. 'Not of serious intent' means play, and that is what he is defining. Practice, being less serious than the thing practiced, easily degenerates into play, but, so becoming less efficient than serious practice, it would not be favored by natural selection. We often see play originating as degenerate practice, as when a company assemble to practice for an exhibition, but this completely spoils the practice. The less playful practice is, the better it is. We surely grant all that can be said for practice, yet much play is recapitulation or embryologic, and reversionary or degenerate or recreative, or it may be wanton. Plays of the degenerate type are gambling, drunkenness, debauchery and similar amusements, and, indeed, all degeneration belongs under play rather than under work. In degenerate activities play freedom is only subjective; there is really bondage. And all play freedom is false and unreal as being mere hereditary impulse, and in so far as the play world is one of unreality and illu-

sion. Rational and real freedom lies in work; the captain of an Atlantic liner has a truer and higher freedom than a boy sailing a mimic craft in a pond. It is easy to idealize play as spontaneous practice, perfectly free, pure and joyous, but at best play is only conservative, and very often is reversionary and even degenerate. Play is a low form of life; and it might be said to be on the whole of more disservice than service, and to be supplanted in the highest evolution of man by work-satisfaction, of which we already see some evidences.

Play, as we have noted, may be described as fresh, free, joyous, spontaneous, impulsive, self-contained activity, whether practice or any other mode. The unseriousness of a practice does not of itself make play, for the unseriousness may be carelessness and laziness, the reverse of play.

To the elements of play we have mentioned are we to add shamming? It might seem that activity which did not contain shamming must be earnest, and so not playful, but earnestness enters into play, and makes it real play. In shamming, the activity is so far unreal, and if playful, doubly so. That is shamming, deceit, guile, is a work form of activity evolved in the struggle of existence, and may, like any other, become play. Shamming play is then merely one definite kind, rather than, as Professor Groos would make it, a phase general to later forms. Much of later play is not mock activity, but a real activity used as a pleasure in itself. The boy driving his ponies is playing as well as the boy who is playing horse by driving his companion or riding a stick, and the man yachting is playing as well as the child sailing chips in a tub. So any activity once fully integrated through work by race or individual may be either played or played at, both modes growing side by side through the whole history of play.

Professor Groos traces æsthetics to shamming play. But the only fine art for which such play could account is realistic portraiture. However, mere *resemblance* is not art. An interest in clever counterfeits of reality for the skillful way they deceive is plainly not the æsthetic feeling for the intrinsic beauty of the thing. Do not 'What a good likeness!' and 'What a beautiful picture!' indicate different mental states? Now the method of development shows the method of origin, and the method of progress in fine art is plainly one of severest toil. Æsthetics must then originate as work-form under natural selection as mode of socialization, specially in sexual relation, and becoming integrated, reappears, like other integrated activities, in play form. Struggle, effort, is the initiating and developing factor in evolution, and how is art an exception by a unique relation to play?



To the artist and art-lover art is the most serious and highest work. If art originated in shamming play we should expect the earliest art-plays of children to have this form. But we see children showing a crude æsthetic enjoyment in whistling and drumming, which cannot be accounted deceptive play. Woodpecker music—unnoticed by Professor Groos—is a similar play. So also if animals and children show æsthetic appreciation of bright colors, this cannot be based on deceptiveness. (We have discussed the subject more fully in 'Evolutionary Psychology of Feeling,' Chapter XVII.)

As to classification of plays Professor Groos's scheme is certainly not shown to be a complete and connected natural whole. He omits humor, which ought to be discussed as a possible play with the very highest animals; and scare plays are unnoticed. My dog took the same delight in coming up quietly behind a small dog and giving a terrifying bark as does the child in jumping out from a corner and crying boo! Fighting and hunting plays are hardly to be separated as two distinct kinds. He makes the *rationale* of fighting play among non-predacious animals to be preparation for struggle for the female; but do not also young females fight, and may not such fighting be preparatory to struggle for food and for defense of young? He makes courtship a division of plays, but it is plain that 'calf love' and flirtation are in strict sense the only love plays, that is, playing love and playing at love. Again does not imitative play enter into all kinds of play?

But what we need as a basis of classification is a thorough scientific record and study of the facts. For instance, a continuous study of a dog from birth to death for play and work with photographs and phonograms would be a first step in a science of play, if made by a psychologist familiar with dogs. The records which Professor Groos uses, as made by travellers and naturalists, are mostly incidental observations of slight value. However, it is plain that, since any voluntary activity may be played, the classification of such activities becomes that of plays also. Thus among children even winking and breathing may be used playfully, and we also have finger plays, toe plays, etc. But such an anatomical or physiological classification is little fruitful for psychology. It would see more in such stages as simple play, play and plaything, playing at, player and person played to. As any psychosis may become a play form, a genetic classification of psychoses would apply to plays. Any psychosis well integrated as instinct in the race or as habit in the individual—habit plays are lawyers' jokes among lawyers, etc.—may issue in play. Play is

a peculiar emotion which may invest any action. Play is probably the earliest of the complex emotions. Its distinctness in kind is shown by the fact that we can define it only in terms of itself; when we say an action is playful we have but used the simplest terms, and every one who can play recognizes the peculiar psychosis implied. Play is a generic general phase of emotionalism, which may express itself in the form of any intellectual or feeling mode or of any outward activity.

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### THE STRUGGLE FOR A SPIRITUAL CONTENT OF LIFE.<sup>1</sup>

Eucken's latest and most significant work has as yet received no adequate notice in the English or American philosophical magazines. The following is an attempt to give the reader some understanding of the methods and conclusions of this remarkable contribution to the metaphysics of our time.

While the reviewer is, of course, responsible for the general run of the following, he has taken the liberty of paraphrasing the text in numerous places. In this connection it may be well to say that the paper was submitted to Professor Eucken, who advised its publication in its present form.

He who, to-day, raises the question of the meaning of existence and the goal of our activity is caught up not only by the stream of the time, but also by a great flood which springs out of the world's history. The answer to which he is carried is clear and simple. Man belongs to nature—he is a part of her—body and soul. She surrounds us from outside; she rules us from within. She points out to us the only way to truth. When man in his pride and strength turns from nature, his home, and pictures to himself the existence of an independent world of spirit, he has only fallen into error—he has only gone out in search of a fabulous realm. And this vain thought of a spiritual world is only a bar to truth and to happiness. As such it is to be fought and conquered.

With this thought of our time has come a great turning-point in history. Through it there arises the hope of a return to primitive,

<sup>1</sup>Der Kampf um einen geistigen Lebensinhalt; von Rudolf Eucken, Professor in Jena. Veit & Comp., Leipzig. 1896.

elemental truth. Once more humanity shall be revived through closer and more intimate contact with nature.

Into such a relation to nature our time has actually brought us. And this it was able to do just by means of its incomparably sharp distinction between spirit and nature. At first both were one, indistinguishably woven together. The reflection of ancient times and the Middle Ages spun around nature a web of human thought. But this network was to be broken—nature was to be seen in its independence, in its self-existence. Its true being, as distinct from man, was yet to be distinguished, and the soul, as an evil spirit, was to be driven out. This accomplished, nature was seen to be a complex of spiritless masses and motions—all unity was dissolved into small and smallest elements, all worths and ends as mere figments of mind were banished in behalf of a self-sufficient, self-satisfying actuality. For in this vision of nature, so simple and so complete, where is there place for thought? in this vision of world-energy governed by an imminent logic of its own, where is there room for spirit?

As soon as this reduction was accomplished and spirit was driven out of nature, spirit and nature were seen to be two opposing realms. The independence of each was recognized. But nature revolted against this opposition. She turned against man in his isolation and in his alienation. She went out to draw him to her again, to subjugate him. And she succeeded. That she was successful was due to the very power which man had acquired over her by virtue of his alienation from her.

The decay of the vision of the Middle Ages and the rise of an exact natural science was a triumph of man over nature, a subjugation of the external world through human thought. And so, also, the application of technical knowledge and skill. But, again, it was in reality the vanquished which was victorious. For the vision which resulted from the outgoing of man to the external world to overcome it, was so clear and well ordered that it acquired a peculiar power and charm, by virtue of which it was carried over to the inner world. Our concepts of the natural became so definite and exact that on their lines was built up our view of the spiritual. The more our knowledge of the exterior world develops, the more are we occupied with it; the more our technical skill increases, the more is life bound up in the machine; the more power man wields over nature, the more she rules his thought, his life and his senses. In this manner man, who believed that he had subjugated nature, was overcome by nature. And thus we find that the opposition between man and nature has been over-



come by our time. It has been dissolved by the rise of our knowledge of nature and by the development of our technical mastery over her. There exists no longer a recognition of the two independent worlds, each existing in and for itself.

But the effect of this dissolution was not the reduction of nature to the inner power by which we came to rule over her, but the opposite. Man has been reduced to nature. He has become in his thought of himself a mere moment in the world processes, an insignificant factor in the varied interplay of phenomena. In the laws which grind out the world he must find his satisfaction; in the hard actuality which opposes him he must see the end of his existence. He falls back again into nature; there is a return to the naïve view of the world in which man and nature are one.

And in this philosophy of our time is there not a certain sublimity? He who would give up his self-existent spiritual world, who would again turn himself out into nature, must be prepared to leave behind many a desire cherished through centuries. But the reward of this self-denial is the falling down of the barriers between humanity and the all.

But sublime as this philosophy is; as deeply rooted in our time as it is; as strongly as the current of world history forces it upon us—we must still raise the question as to whether it can be our philosophy. For if it be true, our ethics and our religion lose their significance; if it be true, a really developing life is no longer possible; if it be true, there is, in short, no spiritual world at all. It is the primal thesis of our work that it is not true. It is the central belief of our author that man has broken through nature—that there exists in and for itself a self-centered spiritual world.

Of the thesis let us at the start make sure. Nature knows of no working from within, no being for itself, no self-activity, no initiative of the thing. Rather every element is bound to its environment, it exists only as a link in an endless chain; all work is the result of stimulus from without and is directed to another. If we men were mere things in nature, time and space would completely dominate; by them we would be forever limited and hemmed in. But the old opinion is false; man transcends time—it is his very nature to rise above the temporal flow. True, we stand in time and seem to be driven about in its stream. But not quite. Were it so, human history would be impossible, for such a history does not arise in a mere flowing by of events. In order that there may be a history man may not merely live his span of years, but he must reach back into the

past; that which is gone must become present; he must unroll the course of time anew. And in doing this we do not merely contemplate the past: we take it up into our life; it becomes a part of our doing; it complements the present. Through history the limitation and contingency of the moment are overcome; through history we conquer time. And so with space.

And thus through history there arises a unity which transcends time and space. This unity is more than a mere summation—it is a welding of the life of the past with the life of the present. But as such it transcends nature. For nature is dominated by time and space. And in nature there is no unity which is not a mere being together—a mere summation. Man as a part of nature is a mere point—like other parts—the mere point of self-preservation. An inner subordination of the one to the whole, the recognition of the right and love of another as products of nature were wonders, compared with which the wonders of the religions pale. From time immemorial the energies of man have endeavored to reduce the source of all action to the mere interest of the individual. But through all time the self-sacrifice of man stands out in protest. What has the overcoming of self to do with the interest of self? Are the heroes and martyrs only sharper Jews than the rest of us? Is not all this a protest against a mere natural ordering of things? In our history and art, in our ethics and religion, in our very penetration of the external world, have we not overcome nature?

These facts of human life are the proof of our thesis. Through them we see the necessity of the existence of an independent world of spirituality over against nature.

But this new reality did not fall as a ripe fruit into the lap of man; it does not surround us as something given. It was fought for through work and experience; it was produced by self-activity; it is the result of a long and fierce struggle. For the mere natural processes could never have produced this spirituality—it must be the result of a free act, a transcending of nature through self-activity.

The first step in the struggle for a spiritual life was the lifting of ourselves above the mere natural environment, a freeing of ourselves from the limitation and narrowness of the immediate, a breaking away from the life which exhausts itself in its effort to preserve itself and to adapt itself to the immediate situation. For in order to have a spiritual world at all we must transcend the *here* and the *now*, we must raise ourselves above the demands of a natural environment.

This step marked the great turning-point in the history of humanity. It brought about something utterly new, something opposed to

the merely natural life. It freed us from nature in that it freed us from the bonds of the immediate and from the littleness of the natural self. For as long as our activity was consumed in a mere adjustment of ourselves to the situation of the moment there could be no freedom, no opportunity for the creative work of spirit. As long as ourselves were mere points, the mere centers of natural instincts, there could arise no spiritual world. The immediateness of life and the punctuality of the selves of that life had to be transcended. For the spirit-world is not a world which rests upon a point. It is a world which rises above all points, which embraces all reality. Thus this spirituality which arose out of nature as a new creation was a sphere which floated above the individual. It was a whole, a unity. For the individual as such could never have conceived it, could never have created it. It was the whole which spiritualized the individual. And the spiritual life wherever it exists is something which is not dependent upon any one self, but something in which the particular self finds its existence. It is always an independent, self-existent whole. And yet this whole was brought about in the creative activity of man. His free act is needed that it come into being. It found its origin in a union of free, creative activity transcending all particularity.

And what is this common activity but *work*?<sup>1</sup> It is the idea of work which marks the great turning-point in life. Through work there arises a new content of life. By means of work we raise ourselves above the limitation and contingency of the natural *here and now*. For all work arises out of some transcendent whole; in it a unity to be realized is always presupposed. Work means life-work. At first this unity is the particular unity, a whole to be gotten by the individual. But even so the work of the particular brings about the work of the whole. The particular works are worked into the work of the whole. Reality is the work of works.

As this common reality is the result of work, so is work the result of transcending the immediate *here and now*—the product of our struggle to rise above the mediocrity of life, of our strife against the average.

In work we see a new creation, something which cannot be deduced from the spiritless masses and motions of nature. It is a self-centered spirituality, a self-active life, a world resting in the struggle of the whole. As such it hangs over against nature and the punctual self.

And thus, in conclusion, we see that the spiritual life has no exist-

<sup>1</sup>This word is a translation of *Werk* as distinct from *Arbeit*.



ence and no meaning, no strength and no marrow without a rising above the average; that no such transcendence is possible without spiritual self-activity; that there is no self-activity at any one point without a self-activity of the whole, without a universal life, without the opening of a new world.

Man, as it were, hangs between these two worlds—between spirit and nature. Both are real. Of this there can be no doubt.

Nature confronts us at every turn. A large portion of our life is devoted to her. On the other hand, the world of spirit is with us also. To deny its reality is equally absurd. And what is man to do with these confronting realms? Both cannot equally claim him. At the start let us do away with the idea of an empty compromise. Nor can he reduce the spiritual to the natural. This attempt, as seen in the philosophy of our time, amounts simply to a denial of the reality of the spiritual. For centuries the idea of the development of all reality from the pure activity of spirit has exerted a fascinating power over man. But if we follow the links of the chain from Plotinus to Hegel we see that either life became a mere sum of abstractions, or that experience, which at the beginning had been so sharply pushed aside, was unconsciously called in to complete the intuition, to give life to the otherwise empty forms. Nor does the distinction between form and matter aid us. For, on the one hand, the spiritual is far too self-centered and real to stand merely as part of the whole. And on the other, nature is far too cold and capricious when seen in contrast to the spiritual to adapt itself as matter to form.

And yet the dualism is intolerable. The idea of the independent existence of two coördinate but different worlds of reality in life is more than can be borne. The opposition must somehow be overcome. These two masters man cannot serve. And yet neither of them can be denied.

If we would work our way out beyond this dilemma there is but one course for us to take. Both of our worlds are real and yet different. If we are to see them united in life we must see them both as the product of a reality deeper than either.

Here, again, we stand at a great turning-point and once more great possibilities open before us. We must grasp the world as the development of a substantial spirituality, of an essential life. We must see our own free activity as a part of a deeper life. We must see nature as a product of profounder reality.

With the idea of a substantial spirituality, which brings forth a

new and peculiar being out of the depths of activity itself, there opens a new view of our world and our life, an altogether new experience of reality. Both of those worlds, each of which lived in its own right and seemed to pretend to the whole of reality, are seen to be expressions of an essential being. Our free activity must have a deeper life behind itself.

If these two realms are one in their origin, the problem is to realize that unity. It is ours to find in nature this primordial force, this substance; to wrest it from her; to appropriate it to ourselves. It is ours to conquer nature. The act shall take being up into itself; thus it shall itself become real.

This substantial spirituality is not a part of us; we are rather a part of it. Through it we acquire freedom; as a part of it man makes his history; we feel it stirring in us, and we go out to create. But this primordial activity will tolerate no limits; it will not be imprisoned; it will be hemmed in by no barrier. There is nothing in the world which may oppose it; it will brook nothing foreign or alien to itself. Therefore we, as stirred by it, must conquer the exterior world; we must transform it by our spirituality; we must take it up into our own activity. As the artist spiritualizes nature—as he, accomplishing the impossible, transforms it with his own inner life—so must we all overcome the outer world.

It is by this free act that the spiritual acquires character and determination. The inner world, hanging over things as the spirit hung over the waters at the creation, lacks substantiality. As it broke loose from nature and rested in its alienation from her, a certain indeterminateness possessed it. It saw nature as something hostile and foreign. But the spiritual life can acquire substance only as it, through activity, takes nature up into itself—only as it spiritualizes the external through free activity.

The first great struggle of mankind was for the existence of the spiritual. It overcame nature in the battle for a spiritual life. But the life lacked determination, and thus arose a second struggle. It too was a fight against nature; but the end of the struggle was not her destruction, but her spiritualization—it was the strife for the character of the spiritual.

And man, in so far as this world-power rises up in him, becomes world-power himself. Man as a part of nature is driven hither and thither by forces exterior to him; for he is but a mere point, a link in an endless chain. But when he rises up into his primordial activity he thereby becomes world-power. Through him, in him, by him, is

world-history brought forth. Hereby he transcends himself, he rises above the punctuality of life, he becomes a part of the whole, his life moves on in the world life. Thus the individual becomes microcosmos, and the all a world of worlds. When doubt rises up in us, doubt as to God, the world, men and life, it must at last lead back to doubt of ourselves, the doubt as to the ideality of our nature, the presence of a spiritual substance in our life.

In our descriptions up to this point we seem to have traced the triumphal march of spirit, but in reality we have been following only the progress of battle. And this struggle is one which never ends. The strife continues in each plane of life. At first it was the struggle of spirit to raise itself above the chaotic manifold of nature—the struggle for existence; then it was the struggle to lift experience up into spirituality, that the latter might acquire determinateness—the struggle for character; and now we have to see this spirituality enter into a new strife for the world-power of spirit. For as soon as the spiritual life takes on concreteness and character, there arises a host of new contradictions which threaten its destruction, which seem ready to shatter the new life. The powers which seemed to have been conquered spring up again with new force; the old contradictions which seemed to have been transcended appear in new forms. The very forces which worked for good now seem to take on an evil nature. The woe of the world, its contradictions, the thought of death, which seemed to be the kindly spirits of man aiding him on, now turn against him. The whole structure of spirit seems to have been founded on sand and to be on the point of being shattered by the contradictions which have grown up within itself. Doubt as to the reality of the spiritual life arises; we faint in the struggle against the evil; the contradictions seem insurmountable, and we are threatened with the loss of that which we so hardly won and a return to naturalism appears to await us.

The first attack on the new life arises out of our new relations to nature. Here lies the first battle-ground in the struggle for the world-power of the spiritual realm. For the better we have come to know the exterior world, the more foreign it seems to be; the more we have to do with it, the colder and more alien it grows. The forces of nature work in blind objectivity, without respect to the spirituality they hem in and destroy. Fire, water, storm and earthquake annihilate; the little forces of destruction do not sleep. Nature works without respect to high or low, good or bad; she operates without feeling, and



handles the individual, the masterwork of spirit, in perfect indifference.

And as the knowledge of nature grows with the development of spirit, there grows also the knowledge of the utter dependence of the individual on nature. We have long known that the soul is, in general, conditioned upon the body; but we now see in oppressive clearness how every spiritual activity is based upon the corporeal. And then the thoughts of heredity and environment steal in and shed their weird light over the world. And thus man, in his thought of himself, becomes a mere episode in the world-processes. Is it any wonder that he loses faith in the new world—in the spiritual substance?

And so it is with the moral world.

The ethical life has always been the great refuge of man from the bondage of nature. When all else topples and falls the eternal worth of the pure heart and noble mind has stood fast. Here, if nowhere else, man rises above the limitation of nature to the freedom and reality of the spiritual; in this realm, if in no other, the dark despot of the punctual self of nature is overcome. But, even here, the spiritual is threatened by the destroying might of contradiction. After this world has been brought into being, we see for the first time its inconsistencies. For, instead of an all-embracing love and justice, there arises a love and justice bounded and limited by the instincts and passions of the self of nature. Our morality is well and good only so long as it favors ourselves, our party, our narrow circle or our race. And thus the object of the moral world is a manifold of scattered units instead of the totality of humanity—a collection of little centers separated by a wide gulf, instead of the whole of mankind. Here we see, in all clearness, the triumph of evil, the unconquerable nature of sin, bound up as it is in all our spiritual activities. So we doubt the reality of our ethical life.

And we fare no better when we turn to history, for here we find the past to be only a great mass of actions done and gone. We are unable to draw from it the fresh, active present for which we had hoped. History should solve our problems for us—in the past of human life should be seen a line of steady development making for eternal truth. But instead of that, we find only a nexus of contradictory intuitions and faiths. History should have solved our problems, but instead of that it has travailed only to bring problems forth. And we lose our faith in the past of human experience.

And so it is also when we consider the nature of society, fate and the very spiritual power itself.

The evil does not lie on the surface; it reaches down into the fundamental conditions of our very existence. The spirituality which does appear seems too weak to overcome the great attacks which are made upon it unceasingly. In its own development it constitutes rather an indifferent form to a content than a full world. History has rather increased than diminished the complications. The new world has been brought forth, but only cracked and broken by fundamental contradictions. Once more we find ourselves in a great dilemma—again we have reached a position where we must either turn back or go on still further. No compromise is possible; to stand still is intolerable; some solution to the problem of our relation to the evil in the world must be found.

The history of world-thought contains many attempts to escape this dilemma; indeed, it has been largely occupied in endeavoring to offer some solution to the problem. There have been in general two great answers to the question. According to the first of these, the evil in the world is mere appearance, the good alone is real; according to the second, the evil alone is real, the good is mere appearance. The former is optimism; the latter, pessimism. Each comes to its conclusion by denying the reality of one side of the dilemma.

The first great plea of optimism lies in the thought that if we could but see the world from its center the evil would disappear. The bad is in the last analysis good, for through the bad the good rises up and grows. The second plea lies in the thought that if we would but do the good the evil would cease to exist for us—if we would but center our lives in the world of the pure and high the world of the impure and low would be forced into non-existence. But whether the optimism be that of consideration or of action, its falsity remains. For, no matter what our point of view may be, no matter how our actions may be directed, evil remains and must ever remain. It were as easy to prove that the good serves the evil as the evil the good. If we must write our theodicies why not also our satanodicies?

The essence of pessimism lies first in the thought that the world down to its very center is evil, and second in the act of giving up the hope of attaining the good or the true. The theoretical view of the world as bad and truth as an illusion must end practically in the giving up of the good and the true—the philosophy of evil must inevitably result in the philosophy of self-denial.

But if there be neither happiness, virtue nor truth in the world, what have we to give up? If the idea of the good be an illusion,

what is the meaning of self-denial? The very fact that the turning away from happiness costs us so much effort and gives us so much inspiration is *prima facie* proof of the existence of happiness. And so with truth and goodness. Truth and goodness remain in all their sublimity; pessimism is but the turning away from them.

Pessimism and optimism may annihilate each other in their struggle for supremacy; but the good remains good, evil does not cease to be evil, and both are real. And the moralist still moralizes on the 'mixed character of human life.' Truly life is mixed, not merely on its surface, but in its very depths. And after all the dilemma still remains unsolved and still demands solution. But if there be any power and courage left in life there is one way of escape open—we can transcend the sphere of conflict, we can raise ourselves above the contradictions into a new realm. The world of struggle is not the only world; its barriers shall not block our progress. There opens a sphere of activity beyond the sphere of these contradictions; in it must we center our life.

This new order would appear as something transcendental, something which lies beyond the horizon of our world. But in truth it should only expand the real; it should command for itself the first place in our world and force all else into a subordinate position; but still it should be within the real, our real. The ultimate and final it must always be; it must contain all absolute truth; in it must be builded the world-power. As such and as such alone can it transcend all contradiction and overcome all opposition.

This is a hard saying—this last step in our spiritual life is the most difficult of all to grasp in thought and to realize in action. But the saying nevertheless is true. And that it is such we can see, if we can see at all, from the workings of religion. For this struggle and this realm are essentially the struggle and the realm of religion.

In religion the individual rises out of the contradictions of the world. He does this because he lays hold of the absolute life. The contradictions of the world do not disappear; they are rather intensified; but in that something absolute is realized, they are transcended. The barriers are not broken down; we leap over them. For in religion we leave this world of struggle behind, we rise up into a realm of absolute truth which knows no contradictions—the spirit reaches the plane in which it becomes the world-power. For the struggle to reach an ultimate point and to bind all life to it is essentially the struggle of religion.

But this new life is not an abstraction—it is a personal activity, a



coming into relation to the absolute life. It is not something given, but something to be gotten by a free and original act. It is to be gotten because all that confounded us in the lower life is overcome in something higher.

If this new and final realm is to be seen from the standpoint of religion, it is now clear that the rest of spirituality is to be seen from the standpoint of culture. First of all we have a struggle for the creation and preservation of the spiritual in general; then the struggle to penetrate and overcome nature, and finally the struggle to rise to an ultimate reality beyond the contradictions of the former stages. The second of these steps belongs essentially to the world of culture, the third to that of religion. And these two worlds must ever remain distinct and separate spiritual experiences. They have often striven with each other; each has claimed its sole right to exist, but mankind ever returns to them both. Neither can be omitted from the great system of the spiritual.

In conclusion we may sum up the foregoing as follows:

We saw the world of the real open itself in three levels.

The first problem was that of the independence of the spiritual life. This life cannot exist as a mere expression of a foreign activity. It must be self-centered. That it arises in its purity was due to a breaking away from turmoil and particularity of nature and the formation of a new and peculiar realm of ideal worths. With the development of the independence of the spiritual there arose not a world of particulars which left alone the rest of reality, but the soul of the real itself. Through it came about a turning of being to its own truth, a finding of itself, a delving into the depth of life. As such it is more than a mere human phenomenon. Mankind is thereby taken up into world movements which raise man above particular powers and interests; which raise him above the mere average of life.

But necessary as this step was, by means of it alone the spiritual attained only general outlines. A second step was required to secure it character. This was taken in order that the great opposition between the spiritual and non-spiritual external world be overcome. Thus, there took place a return of the spiritual to mere existence. Nature was grasped from the standpoint of the new life in order to spiritualize her. But this struggle could not meet with success without transcending of the work of the mere individual and the formation of a unity of spiritual life. Thus there arose a life-system, a totality of work, a spiritual substance, a struggle for

the character of life. And thus the happy end of life and satisfaction with the real seemed near at hand. But the struggle with the content of our human experience developed unforeseen but fundamental contradictions. The new life was seen to be rent with irreconcilable oppositions, the actuality with which it had to deal, cold and indifferent. All attempts to reconcile or wipe out proved failures; the whole was lost but for the opening of a new world, the rising of spirit to a new level.

Thus a third step was undertaken, and there came about a struggle for the world-power of spirit. Life was raised above the contradictions; it was united to the absolute life.

But in so doing the old world was not to be given up. Its levels were to be preserved in their distinction and integrity. In the relation of each to the life process was to be found the consummation of spirituality.

From beginning to end this spirituality was to be conceived as a noologic reality, something not based on the *psyche*, but on the *nous*. It is a coming to itself of reality, being turning to its own truth.

We may not start with the world or with the soul, but with a life which is over and beyond these; which does not belong to man, but of which man may become a part.

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### EXPERIENCE UNDER THE INFLUENCE OF ETHER.<sup>1</sup>

I have called this a description of my experience under the influence of ether, although chloroform was used for a few moments at the start, producing no noticeable effect, however, except a taste of sweetness. The ether had a decidedly disagreeable taste.

After about two minutes the larger muscles of the leg began to be affected, those of the calf first, closely followed by those of the upper leg. The feeling was that of tiredness, with a prickling sensation somewhat like that felt as the blood begins to course after one's limb has been 'asleep,' somewhat like that felt in an electric bath, but not exactly like either. The ends of the nerves seemed to vibrate, as it were, and I imagined the nerves contracting in length.<sup>2</sup>

<sup>1</sup> An experience of I. B. communicated by Professor E. A. Kirkpatrick; suggested by the communication from Professor W. James, in the May number of the REVIEW.

<sup>2</sup> I have always had a nervous feeling similar to this when hearing of one's passing through excessive pain, and can quite readily reproduce it since my experience with ether.

These sensations were soon followed by a feeling of inability to get breath, like that produced by the pressure upon the chest of a bather who is not accustomed to the water. The muscles of exhalation, however, did not seem to be much affected. Later there was a slight sense of suffocation, but I do not remember that breathing became at any time very laborious.

The next muscles to be affected were those of the arms, the biceps first. The sensations produced were much like those in the legs, but milder.

By this time the prickling sensations in the leg muscles had been gradually replaced by a weariness that was almost unendurable. Every muscle seemed to be utterly exhausted. I would seek temporary relief by crossing and recrossing the legs, until they finally became so heavy and numb that I could not move them. I do not remember any muscular relaxation, nor any internal feelings, as of the heart or stomach.

The last sensation I remember was that of my weight upon the stretcher. I have no remembrance of the time when the smaller muscles became affected, nor of the bodily feelings just before entire insensibility came on; it seemed to come upon all parts of the body at once.

Following bodily insensibility began a horrible mental struggle even more indescribable than the bodily sufferings. It was a life-and-death struggle between existence and non-existence. I seemed to see myself as in a dream, a space of light about four or five inches in diameter. Surrounding this space was non-existence, a thick, heavy, material darkness, which steadily encroached upon the limits of the light. The awful part of it was that I seemed bound to resist to the last possible moment and yet realized that darkness must finally triumph. I had no sense of having or wishing weapons to use in the struggle with darkness, nor any remembrance of ever having had bodily members.

For a short time there remained the idea, somewhat comforting, that I should sometime awake from my condition and be free again. Nearly to the last I retained an indistinct visual memory, and I saw myself going through some former activities. The last thought I remember was: 'Thou alone art able; so, Lord, watch over me.'

This mental struggle seemed to last some ten to thirty minutes; for the most of which period of existence, this luminous space seemed to be in the form of a funnel, and gradually to decrease in size until it became a point of light, which vanished in about the direction of the North Star. The struggle seemed to begin in an enclosure about



the size of my head. This enlarged in all directions until its limits became infinite.<sup>1</sup>

On awakening,<sup>2</sup> my desire was to blow; at first my efforts were very feeble. I desired principally to blow the doctor out of existence. I was in a general fighting state of mind, and as I went out upon the street desired to shake my fist at show-bills and people, but refrained for fear I should be thought crazy. It took a day or more for this effect of the ether fully to wear off. The ether made me quite sick for nearly a week, and for several years afterward the smell or mention of it produced a feeling of dread.

I. B.

<sup>1</sup>I have several times had this experience in the partial delirium of fever; with this difference, however, that in fever the sensation is a painful one, the room in which I imagine myself enlarging until the thought of it gives pain; often, too, this enormous space will seem painfully empty, with perhaps two or three voices in different parts or enormous animals tramping about.

<sup>2</sup>My attendant informed me that while coming out of the sleep I would raise one leg, then the other, striking my heels heavily on the couch.

## PSYCHOLOGICAL LITERATURE.

*The Groundwork of Science.* A Study of Epistemology. ST. GEORGE MIVART. The Science Series. New York, G. P. Putnam's Sons; London, Bliss, Sands & Co. Pp. xviii+328.

The term science is used in this work with an ambiguous meaning. At times the author employs it in the general etymological sense to cover all forms of systematic knowledge and inquiry. For the most part, however, he means by it the empirical sciences, divided into two classes, those which relate to the physical world and those which have to do with the inner, psychical life. The aim of the work is to investigate the principles which lie at the basis of 'science;' and so the inquiry becomes an essay in general epistemology, with particular reference throughout to the noëtics of science in its more narrow and technical significance.

The psychological foundation of Professor Mivart's theory of knowledge is a sharp distinction between the lower and higher faculties of knowledge. The former belongs to animals as well as man, and the harmonious working of its several subordinate processes ('consentience') enables the brute to manifest phenomena which resemble the results of man's higher psychical activities without in the least being the same as these. The human infant, it is true, in many respects is not unlike the animal, but the likeness is superficial, since, in reality, there is a great gulf fixed between the two orders of consciousness. Latent in the mind of the child lie the germs of his future thought; but the animal is limited to his lower level, with no hope of future development. Moreover, those who maintain that the higher faculty in man has been reached by a process of natural selection are in error; for natural selection has no power in itself to bridge a gap 'in kind,' and the truths which are attained by means of the faculty in question are in the great majority of cases such as 'never could have given their possessors an improved chance of survival' (pp. xiii., 272 ff.).

This higher faculty of knowledge is described in a variety of ways. It is 'intellectual,' in distinction from sensuous, 'perception;'; it is the source of 'intellectual conceptions;'; it is intellectual intuition; it is

reflex self-consciousness; it is the power of cognizing relations; above all, or rather in all, it is the source whence we derive the primary and self-evident principles which form the ultimate premises of all ratiocination and without the support of which science, like knowledge of every kind, becomes an impossible dream. As these original intuitions are considered by Professor Mivart the foundation of all thinking, he is careful to repeat his summary of them at various points in his work. The briefest statement of the list is as follows:

“The existence of certainty; the existence of an external world; our continuous substantial existence; the validity of the process of inference; the self-evidence of some truths; the principle of contradiction; the evidence of axioms; the principle of causation; the uniformity of nature; and the existence of necessity and contingency” (p. 310).

Chief among these, at least in the amount of energy expended in its defense, is the second, which is stated more in detail on page 106: “An external, objective world exists and is truly apprehended by some of our intellectual acts, an absolutely certain knowledge of objectivity being afforded us through memory, which reveals to us real existence external to all our present experience.” The entire third chapter of the book is devoted, in the spirit of this principle, to the refutation of idealism. The argument is vehement and decided, although it is not till a later stage in the inquiry that the Berkeleyan is classed with the insane, for the author believes that physical science is possible for those who accept idealistic views only because a beneficent nature has endowed them with faculties that guide them aright in spite of their speculative vagaries. Nevertheless, it would be difficult to maintain that Professor Mivart’s discussion furnishes many new weapons with which to repel the idealistic attack. In fact, it is doubtful whether he clearly distinguishes between empirical idealism and metaphysical idealism; and it is certain that, confident in the *a priori* arguments, he fails to avail himself of the realistic implications with which modern physical science abounds.

In the last two chapters of the work we come in sight of a theistic capstone for the epistemological edifice; while the last of all, with the same title as the whole book, brings a short consideration of the metaphysics of science (force, energy, time, space, etc., p. 297 ff.).

The type of philosophy thus presented is so familiar that no extended critique is demanded. The way in which it is presented, however, calls for rather more attention. The following quotation may serve as an example of the care with which the argument is conducted,



for it is scarcely to be credited that it is a fair specimen of the author's philosophical erudition :

“For the whole of the philosophy of Germany and Holland, from Spinoza to Hartmann, has been a result of the mental seed first sown in men's minds by Berkeley, who explicitly produced what was implicitly contained in Locke” (pp. 40-41).

Here are two examples of accuracy in reasoning about the subtlest epistemological questions :

“Though we are for the most part content to act on reasonable probabilities, yet certainty attends us at every turn. \* \* \* If we find, on returning to our library, that a window which we had carefully closed before starting, is open, we are quite sure that some one must have opened it” (p. 98).

“The first and most important of these” [self-evident] “principles is the perception of the reality of existence—that which we perceive to exist evidently does in truth so exist. This is often expressed by the formula ‘*A is A*’ \* \* \*” (p. 242).

Finally, we may cite one or two instances of philosophical humor such as rarely lighten the labors of the epistemological student :

“To the other idealistic extreme, that by Hume, we will sacrifice no space, for, in spite of its author's acuteness and great ability, it does not really admit of logical statement, so utterly incoherent is it, and so confident are we that its ingenious author had no belief in it himself, but was laughing in his sleeve at his inept admirers and disciples” (p. 83).

And this, in criticism of Mr. Spencer's assertion of our anthropomorphic interpretation of physical causation :

“Surely greater nonsense has rarely been written. Let us suppose the partly-sawn-through tree to be not even touched by us, but that a gale has sprung up which, after having swayed it to and fro, breaks it off and prostrates it, just as we have supposed it prostrated by human efforts. Are we not then to say that the wind has exerted as much force as was ours? Can we not say this confidently, without being such idiots as to attribute ‘feelings’ to the wind?” (p. 261).

These, it is true, are among the most striking illustrations of the style of the work. But they are not isolated cases. And, in spite of the learning and insight which are evidenced in this as well as in the other products of this well-known author's pen, it is to be regretted that the philosophical volume in a scientific series should itself be so unscientific in character.

A. C. ARMSTRONG, JR.

*L'Art et le Réel; Essai de Métaphysik fondée sur l'Esthétique.*

Par JEAN PÉRÈS. Paris, Felix Alcan. 1898.

An attempt at developing the metaphysical implications of the æsthetic sentiment, this work, though lacking somewhat of the systematic character of the masters (of a Schelling, for instance, under whose influence, by the way, much of it seems to be conceived), is yet true to the French traditions of good style and does justice to the more important æsthetic and sociological intuitions of the time.

Though mainly of metaphysical interest, it is full of keen psychological analyses, especially of the sentiments of the beautiful and the real, upon the identity of which (as seen in the analysis) a striking argument for a monistic doctrine of experience and existence is built. The advantage of going out from the æsthetic side of experience, in a metaphysical reconstruction of the real, lies in the fact that art is double-faced, on the one side historical reality, and on the other, as embodying the sentiment of beauty, it implies nature. Transcending alike the abstract dualism of subject and object, which abstract thinking entails, and the dualism which volition discovers between the self and the world, sentiment alone can attain the absolute unity of intuition. The method of this metaphysical analysis of sentiment is not, therefore, the abstract analysis of psychology—its beauty is not the beauty of psychology, nor its real the real of science. It is the difference between concept and sentiment. Now the sentiment of reality, as distinguished from the notion of the real, is possible only when the *habitual* of intellection or volition is transcended, for the conceptual is the sphere of the possible; the individual volitional, from the standpoint of immediate experience, is the sphere of chance. The sentiment of the real is always an intuition of universality, of destiny, brought about by the 'choc' of a novel experience. This sentiment of the real in contrast to the notion of the real has in it the inherent power of grasping subject and object, nature and soul in a higher unity, and is characterized as '*une vivante analyse du réel,*' called forth by states of high action and intense nervous energy. An analysis of the æsthetic sentiment shows it to be of the same character, in so much that the author calls it the culmination of the sentiment of the real. The highest form of the love of existence is the love of beauty.

The concept of life, therefore, as a primal activity expressing itself in its highest potencies in the sentiment of the beautiful real, is fundamental; "il semble en effet que l'homme embrasse le réel par la connaissance et la contemplation de toute l'ardeur de toute l'intensité

du sentiment par laquelle il comprend ce qui lui manque pour être ce qui est."

Upon this basis the book proceeds to show in detail the manner in which art has been a progressive realization of elementary forces of life by means of the emotions of reality which it affords. The chapters upon the æstheticising of the categories of space and time, of unity and infinity, upon the social necessity of reflection upon the real, and the essentially æsthetic qualities of the past as compared with the present and future are interesting, both as developments of M. Pérès' doctrine, and also as specimens of fine metaphysical analysis.

In conclusion we may call attention again to the originality of these analyses of the sentiments of the Real and the Beautiful.

WILBUR MARSHALL URBAN.

URSINUS COLLEGE.

*Studies of Good and Evil.* JOSIAH ROYCE. New York, D. Appleton & Co. 1898. Pp. xv+384.

In this collection of essays Professor Royce has presented the principles of his idealistic philosophy in their application to the problems of life. They show the author at his best in the sphere of concrete thought. His idealism is here essentially a philosophy of reality. He touches upon various themes, most of which bear upon the ethical aspects of life. His topics range from the problem of Job to modern character studies in the essays on *Meister Eckhart* and *Jean Marie Guyau* and such social problems as are suggested by *The Squatter Riot of 1850 in Sacramento*. The several essays on consciousness and also the one entitled *The Case of John Bunyan*, must prove of special interest to the student of psychology; those who are interested especially in the mental experiences of the abnormal type will appreciate Professor Royce's exhaustive analysis of Bunyan's religious experience.

JOHN GRIER HIBBEN.

*The Conception of God.* A Philosophical Discussion Concerning the Nature of the Divine Idea as a Demonstrable Reality. By JOSIAH ROYCE, JOSEPH LE CONTE, G. H. HOWISON and SIDNEY EDWARD MEZES. New York, The Macmillan Co. 1897. Pp. xxxviii +354. Price, \$1.75.

These papers were originally presented in a discussion concerning the being of God which was held under the auspices of the Philosophical Union of the University of California. To the main discussion there is added a supplementary essay by Professor Royce outlining



his position more in detail. The main argument is by Professor Royce, and is unfolded with his usual skill and dialectic subtlety. In the implications of our ignorance he finds the intimation of an absolute knowledge, and in the attribute of omniscience he discovers the germinal concept of God, insisting, however, that absolute knowledge also implies love, will, wisdom and the other divine attributes. His position of monistic idealism is combated by Professor Howison, who urges the impossibility of reconciling an immanent God with the free activity of individuals. Professor Royce, in rebuttal, discusses at length the principle of individuation which according to his view lies in that exclusive interest which is characteristic of the individual will. He, therefore, very stoutly maintains that the unity of the world in terms of self-consciousness does not destroy individuality as Professor Howison would insist, but, on the contrary, while transcending the category of individuality, the unity of consciousness need not, however, sunder the individuals which are embraced in it.

Professor Mezes' main criticism of Professor Royce's argument is that the concept of God as given by Professor Royce seems to lack the elements of spirituality. In defense, however, it is urged that the idea of spirituality, so far as it is a valid idea, must be one of the ideas which the Absolute finds fulfilled in his experience. In other words, Professor Royce contends that the idea of spirituality is implied in his concept of God, inasmuch as an absolute knowledge and experience must be an absolute knowledge and experience of the spiritual as well as of the purely intellectual elements of consciousness.

Professor Le Conte in this discussion urges that the concept of God is illumined by a true interpretation of the phenomena of evolution. He finds in the process of evolution a real progress from what he designates a diffused form of Divine Energy to a personal form. Professor Royce does not seem, however, to be in sympathy with Professor Le Conte's metaphysical interpretation of the theory of natural evolution, and insists, moreover, that along such lines of thought very little progress will be made towards a solution of the vexed problems of evil, of immortality, or of freedom.

This discussion is of special significance inasmuch as it has not been a mere clashing of conflicting opinions, but, on the contrary, the several participants seem to agree substantially "in recognizing," as Professor Howison himself remarks, "in some form or other an organic correlation among the three main objects common to philosophy and religion—God, Freedom, Immortality."

JOHN GRIER HIBBEN.

*The Problems of Philosophy.* By JOHN GRIER HIBBEN. New York, Charles Scribner's Sons. 1898. Pp. 203.

Professor Hibben has undertaken an important service for philosophy in this country. It may be a little querulous to ask why not write a philosophy itself instead of a mere description of its problems. It is a philosophy that we need much more than a propædeutic to its problems. But there is a good excuse for doing the latter, and it is that no one will permit another to make the undertaking with any peace. There is too little belief in its possibility for any one to have the necessary courage. But it is permissible to state what the insoluble problems are. Professor Hibben thus labors under a disadvantage in the restraints which a philosophical public imposes upon him, and I judge from some observations in his book that he would be glad to be free from them. But the task, whether grateful or ungrateful, has been done in a very clear and concise manner. In mapping out the lines of thought for the student the book will be found to have performed an excellent service. I would criticise only the introductory chapter, as too much of an attempt to purloin an interest for the subject from the field of literary ideals. Philosophy has taken on the severer aspect of science, and does not well tolerate an appeal to sentiment. The other portions of the book, however, sustain a different tone, and ought to accomplish the object for which it was written.

J. H. HYSLOP.

COLUMBIA UNIVERSITY.

#### INDIVIDUAL PSYCHOLOGY.

*I 'testi mentali' per l'esame degli alienati.* Note di psicopatologia individuale dei dottori G. GUICCIARDI e G. C. FERRARI. Reggio-Emilia, Calderini. 1896. (Riv. Sperim. di Freniat., 1896, XXII.) Pp. 20.

*Di alcune associazioni verbali.* Ricerche di psicologia individuale dei dottori G. GUICCIARDI e G. C. FERRARI. Reggio-Emilia, Calderini. 1897. (Riv. Sperim. di Freniat., 1897, XXIII.) Pp. 26.

*Il lettore del pensiero 'John Dalton.'* Contributo allo psicologia delle piccole percezioni e dei movimenti minimi. G. GUICCIARDI e G. C. FERRARI. Reggio-Emilia, Calderini. 1898. (Riv. Sperim. di Freniat., XXIV.) Pp. 56.

In the first paper the authors describe a series of tests used in the psychological laboratory of the Psychiatric Institute at Reggio for

determining the higher mental powers of patients. The tests were of five classes: (1) *Motor phenomena*. As a study of more or less unconscious movements the subjects were told to write a set of 20 figures, dictated at a uniform rate, then a similar set dictated at an irregular rate, and finally to perform a simple operation in division. The graphical errors, repetitions, delays, etc., which varied greatly among different patients, were taken as measures of individual and type differences. To test the conscious control of movements the Charrière apparatus was employed. (2) *Vaso-motor phenomena and emotional states*. The sphygmograph was used while the patient was asked various questions, some of which he could answer, others not. The record indicated the patient's emotional excitability. (3) *Field of apperception and attention*. In a dark room a number of stimuli were simultaneously illuminated by a spark from a Holtz machine; the stimuli used in different tests were figures, capital letters in chance order, the same forming a word, and letters of different colors in chance order. The object was to determine the number of stimuli recognized in each case. (4) *Reasoning, æsthetic emotion and association*. Reproductions (uncolored) of celebrated pictures, simple in motive, such as the Angelus, were shown; also a series of eighteen photographs representing a celebrated French actor in various guises. The subjects were to tell what each represented or suggested. (5) *Organic memory; sense of time and space*. A dial was arranged with a hand moving in one direction, slowly and uniformly. This hand was first made to move over a determined space on the dial; the subject was then blindfolded and told to make the hand cover the same space, starting and stopping it by an electric key.

The authors report typical results obtained from the patients for each test, which they compare with results obtained on normal individuals. They do not attempt, however, to tabulate the entire series. The third and fourth tests are mentioned as giving especially interesting results, which differ greatly from the normal. In connection with the fifth test, the authors note the distinction between spatial and temporal types of individual, as well as a neutral type, the latter measuring the rate equally well by either datum.

The second paper describes experiments performed on 54 normal persons—30 men and 24 women. Each subject was tested separately. A paper was given him, on which were written the five combinations, *ile, eno, ago, ondo* and *olle*, and he was asked to set down as many words ending in each of these as he could think of in ten minutes. The experimenters noted the order in which the words were set down,



with special reference to the skipping about from one ending to another; they also noted the number set down during each successive minute. From the tables given we find that the men's average was much greater than the women's (39.2, as compared with 27.5); that of persons 30 years of age or over greater than of those under 30 (for the men 41.8, as compared with 33.6), and the average number found during the first five minutes from two to three times greater for every class of individuals than during the last five minutes. A difference was observed in the procedure of men and women; the former generally endeavored to think of a number of words with one ending before passing to another; the latter usually attempted to find one word under each head successively. The curve representing the number found each minute falls steadily for both men and women, except during the seventh minute, when it makes a decided rise.

There are, unfortunately, several serious numerical errors in the tables published—notably the division (three times!) in Table I. by 39, instead of 30, the number of males. These and other errors throw doubt on the reckoning generally and tend to impair the value of the conclusions.

In the third paper the authors describe their experiments on the 'thought reader' John Dalton—who is, by the way, a great-nephew of the discoverer of Daltonism. Dalton lays no claim to any occult powers, but simply to unusual acuteness of perception for small muscular indications and other minute forms of expression. He submitted himself willingly to all the tests, and seemed eager to assist as far as possible in elucidating the phenomena in question.

Cranial measurement showed nothing noteworthy. The visual field was normal; the visual acuteness slightly below the normal. A number of mental tests made were similar to those described above in the first paper. In the test of spatio-temporal memory Dalton proved to be of the purely spatial type. The test of the apperceptive field, with letters illuminated by a spark, showed him to be rather above the normal. With the Charrière apparatus for testing motor ability he proved to be extraordinarily expert in both hands. A series of memory tests with numbers, colors, etc., indicated a high degree of development of the memory. The reaction times on sight, hearing and touch were rather large. The association test showed a decided preponderance of visual images, with a large number of ideational associations; purely verbal associations and images from other senses were few in number. The subject was asked to describe, blindfolded, the nature of objects placed in his hand; the results showed great acuteness of tactual perception.

One of Dalton's professional 'specialties' is the designation of an object chosen among a set and thought of earnestly by the 'willer,' whose thoughts he 'reads.' He accomplishes this both with and without direct contact with the 'willer.' In the case of direct contact the minute muscular expressions of the 'willer' furnish the general clue, but here, as in the case without direct contact, he declares that the ultimate indications are furnished by the latter's breathing. Tests of this were made at the laboratory, with and without contact, a pneumograph being used to record the 'willer's' breathing. Dalton picked out the required object at once, but purposely passed it over two or three times before finally indicating it. The breathing curve showed a marked change whenever he approached the object. The muscular indications proper were of course not open to direct experiment. Dalton asserts that they enable him to discover at once whether the subject is good or not. These indications are not always of the same sort; some subjects give 'guiding,' some 'opposing' indications; both classes of hints, however, are favorable. It is only when a subject, through lack of attention, fails to give any indications at all, or through nervousness makes various movements besides those due to the object in question, that he is unable to succeed. The indications he interprets are minute in character, and must not be confused with the more apparent muscular movements which the unpracticed observer might regard as indications.

In the latter part of the paper the authors discuss at some length the history and theory of 'thought reading.' Three distinct theories have been advanced to account for the phenomenon: (1) The direct passage of something from brain to brain—the spiritualistic interpretation. (2) The transmission of nervous force from brain to brain by some *dynamic* process not yet understood—by a sort of induction, or by some species of transformation of energy. (3) The transmission is only apparent; it is really due to minute and unconscious expressions on the one hand and an abnormally acute perception on the other. The present tests, corroborating Dalton's own statements, indicate that the third explanation will cover all cases where the 'thought reader' and his subject are in close proximity. In accordance with this view the authors ascribe the power to a partial dissociation of subconscious personality, which gives rise to a species of partial automatism. They believe that the study of telepathic phenomena proper must start from this point and work up, proceeding by means of psychological experimentation, and using, as far as possible, psychological laws already established.

HOWARD C. WARREN.

## VISION.

*Untersuchungen zur Pathologie der Pupillenweite und der centripetalen Pupillarfasern.* O. SCHIRMER. Graefe's Archiv, Bd. 44, pp. 358-404.

Disturbances in the pupillar reflex may be due as well to anomalies in the centripetal part of the reflex circuit as in the centrifugal, but almost no instances of such anomalies are to be found in the books, for the very simple reason that they are extremely difficult to diagnose. The subject has, however, acquired great interest since it has been affirmed by v. Gudden, and independently by Bechterew, that there are special centripetal fibres in the optic nerve for the regulation of the width of the pupil. These fibres are thicker than the visual fibres; they cross, in half, in the chiasma, like them; they run by the side of the visual fibres from the same point of the retina, and they leave the visual fibres only in the region of the external geniculate body. v. Gudden found in rabbits that the removal of one of the corpora quadrigemina produced contralateral blindness without affecting the movements of the pupil; only after portions of the thalamus were also disturbed did the pupil of the blind eye also remain widened. Schirmer has been able, by an admirable series of observations on the human eye, to make the following additions to our knowledge of this subject. The pupillar fibres of the optic nerve do not reach so far as the layer of rods and cones, but they start in an earlier layer of the retina; their terminal organs are in all probability the amacrine cells. The macula and its region is abundantly supplied with pupillar fibres, but they are not wanting in the periphery; they are sufficiently numerous there to keep the pupil more contracted than in the case of the cutting of the optic nerve. The pupillar fibres resist mechanical compression much better than the visual fibres do, but they are equally subject to inflammatory processes, and hence the width of the pupil may become an aid to diagnosis.

C. LADD FRANKLIN.

BALTIMORE.



## NEW BOOKS.

- Beiträge zur Akustik und Musikwissenschaft.* Herausg. von C. STUMPF. 2 Heft. Leipzig, Barth. 1898. Pp. 170.
- Essai d'une Philosophie nouvelle suggérée par la Science.* L. RIBERT. Paris, Alcan. 1897. Pp. 562. Fr. 6.
- System der Werttheorie.* CHR. v. EHRENFELS. Leipzig, Reisland. 1897-8. Bd. 1, pp. 277. Bd. 2, pp. 270.
- Spirit Slate-Writing and Kindred Phenomena.* W. E. ROBINSON. New York, Munn & Co. 1898. Pp. 155. \$1.
- Destinée de l'homme.* C. PIATT. Paris, Alcan. 1898. Pp. 244. Fr. 5.
- Les Origines de la Psychologie contemporaine.* D. MERCIER. Louvain, Institut Supér. de Philosophie. 1897. Pp. 686. Fr. 5.
- The Psychology of Peoples.* G. LE BON. New York, The Macmillan Co. 1898. Pp. xii+236. \$1.50.
- La Famille dans les différents Sociétés.* C. N. STARKE. Paris, Giard et Brière. 1898. Pp. 278. Fr. 5 and 7.
- Sammlung der Abhandlungen aus Pädagogische Psychologie.* Bd. II., H. 1. *Arbeitshygiene der Schule.* F. KEMSIES. Pp. 64. Bd. II., H. 2. *Psychologische Analyse der Thatsache der Selbsterziehung.* G. CORDES. Pp. 54. Berlin, Reuther u. Reichard. 1898.
- Instinct and Reason.* H. R. MARSHALL. New York and London. 1898. Pp. xiii+574. \$3.50.
- Aperçus de Taxinomie générale.* J. P. DURAND (de Gros). Paris, Alcan. 1899. Pp. 265. Fr. 5.
- Psychophysiologische Erkenntnistheorie.* TH. ZIEHEN. Jena, Fischer. 1898. Pp. 105.
- Le libre arbitre.* ERNEST NAVILLE. 2<sup>m</sup>e éd. Paris, Alcan; Bâle et Genève, Georg. 1898. Pp. xiv+311. Fr. 5.
- The Doctrine of Energy.* B. L. L. London, Kegan Paul. 1898. Pp. x+108.
- Année biologique.* YVES DELAGE. 2<sup>m</sup>e année, 1896. Paris, Reinwald. 1898. Pp. xxxv+808. Fr. 20.
- Report of the Commissioner of Education for the Year ending 1896-7.* W. T. HARRIS. Vol. I. Washington, Government Printing Office. 1898. Pp. vii+1136.

Contains a section devoted to the recent literature of imitation made up of articles by Miss A. Tolman Smith, W. T. Harris (Chairman), and E. H. Russell.

*Psychologie der Veränderungsauffassung.* L. WILLIAM STERN.  
Breslau, Preuss & Jünger. 1898. Pp. xiii + 264.

*L'Éducation des Sentiments.* P. F. THOMAS. Paris, Alcan. 1899.  
Pp. 287. Fr. 5.

*Truth and Error, or the Science of Intellection.* J. W. POWELL.  
Chicago, Open Court Co. 1898. Pp. 428. \$1.75.

## NOTES.

THE Macmillan Co. announce the early appearance of an English translation of M. Tarde's *Les Lois Sociales*, by Professor H. C. Warren, of Princeton. We reserve our notice of this important and timely *résumé* of M. Tarde's larger works until the translation appears.

IN accordance with the request of the Government of Venezuela and of the Committee on Organization, the III Pan-American Medical Congress is postponed to meet in Caracas in December, 1900.

THE prospectus has been issued of 'An American Journal of Anthropology' (name not yet decided), conducted by an editorial board of well-known anthropologists with F. W. Hodge as secretary and managing editor. The journal is to be the organ of Section H of the A. A. A. S., and it will replace the *American Anthropologist*. Advance subscriptions should be sent to Dr. Franz Boas, Columbia University, New York. Quarterly, \$4.

WE have received also the prospectus of the *Zeitschrift für Pädagogische Psychologie*, to be issued first in January, 1899 (Ed. Dr. F. Kemsies, Berlin; publisher, H. Walther, Berlin). Bimonthly. 8 M.

THE *Archiv f. System. Philos.*, Bd. IX., Heft 4, contains its annual Bibliography of Philosophical Literature for 1897 of 2307 titles. The *Revue Néo-Scholastique* also continues its quarterly *Sommaire Idéologique*, printed on one side of the paper only, for pasting on cards.

AT a recent meeting of the Academy of Moral and Political Sciences, the Gegner prize (3,800 fr.) was awarded to M. F. Pillon, the Jean Reynaud prize (10,000 fr.) to M. Paul Janet, and half of the Penanrun prize (2,000 fr.) to l'Abbé Piat for his book *La Personne humaine*.

THE attention of psychologists may be called to a 'Critical Review' on 'Modern Neurology' by Dr. Adolf Meyer, beginning in the *Journ. of Comp. Neurology*, November, 1898.



# THE PSYCHOLOGICAL REVIEW.

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## ON CERTAIN HINDRANCES TO THE PROGRESS OF PSYCHOLOGY IN AMERICA.<sup>1</sup>

BY PROFESSOR GEORGE TRUMBULL LADD.

*Yale University.*

The progress of any of the positive sciences is always dependent upon the personal characteristics of the men devoted to the special science, quite as much as upon any other condition. The history of their development in the past would show this statement to be true even in the case of those sciences which are most independent of all subjective influences. A survey of all the favorable and unfavorable conditions under which astronomy, physics, chemistry and biology are developing at the present time—in spite of the enormous recent increase in the importance of instrumentation and technique—does not, I believe, throw discredit upon the value of personal characteristics. It is, after all, the quality of its scientific men which largely or chiefly determines whether the rate of scientific progress shall be rapid or slow in any particular age.

If this dependence on the character of the mind which goes into them is obvious for the physico-chemical sciences, it may be taken for granted as the chief condition determining the rate of the progress of psychological science. For psychology is the science of mind—of the mental life and mental development of the individual man; it is, therefore, in its essential nature, more influenced than are the physico-chemical sciences by the kind of a mind that undertakes to deal scientifically with the things

<sup>1</sup> Read before the American Psychological Association, New York, December, 1898.

of the mind. Its method, moreover, is such as to lay emphasis, in respect of content, upon a rich experience of knowledge, sentiment and practical activities; and, in respect of way of approach, upon training in introspective analytics and in hermeneutical skill. Sympathy with all that is really human, and experience which covers all which is essentially human; these are indispensable qualifications of the high-class psychologist. In psychology, *personnel* and *matériel* are most intimately allied.

I am going to undertake an ungrateful task, and one which cannot expect to appeal strongly to the sympathies of my audience; perhaps it cannot even get its conclusions accredited by them as having a basis of accepted facts. For this reason the undertaking may fitly begin with a few words of more or less abject apology. And, first, what will be said cannot be proved true either by demonstration or by induction; and to attempt its proof by the method of illustration would be, of all ways, most unfortunate. Its value can be, at most, only such as may be accorded to the opinions of one who has been watching the course of psychological science in this country, from an interior point of view, during a score of years. The opinion you are respectfully asked to consider may be briefly summarized as follows: As compared with the increase in number of trained teachers and investigators, and in the amount and quality of laboratory and other equipment, the *science* of psychology is not making with us the progress which may rightfully be expected of it. When inquiry is made, however, into the hindrances of progress, and after due allowance for the intrinsic difficulties of the science and for all the remaining deficiencies in equipment, it is found that one of these hindrances consists in the limited and faulty qualifications of psychologists.

The grounds for this state of my own belief I shall now proceed to make clear in several particulars. And though my theme concern personal qualifications, I distinctly disclaim all intentional personalities.

The first hindrance to the progress of psychology in America to which I wish to call attention is an excessive aloofness from, and a consequent ignorance of, the real mental life and mental

development of the average human being. Now, we psychologists may define the subject matter of our science and limit its special problems as we please; and I have nothing to urge against, but much to say in favor of, the high specialization and careful experimental methods of modern psychology. At the same time, where such specialization is not based upon, and constantly united with, an ever widening and more sympathetic acquaintance with many men of many kinds, its results are unsatisfactory. This is likely to be true both from the scientific and from the practical points of view. If I may be allowed an old-fashioned term—which is, however, just as valuable and significant now as it ever was—psychology is nothing but the descriptive and explanatory study of the ‘souls’ of men. And there is no way of knowing what souls are, and can do, which does not involve the interpretation, in terms of one’s own self-consciousness, of the physical signs given of the conscious state of other souls. The psychologist, then, who is a *mere* experimentalist, or a *mere* scholastic student and teacher, or a *mere* reader of books, does not know thoroughly his business. For his business *is* human nature; and human nature shows itself, as it really is, only to the man who, having it all in himself and having a trained self-knowledge, is fitted to observe, and to interpret, and to theorize upon, the natures of his fellow-men.

“*Willst du dich selber erkennen, so sieh wie die Anderen es treiben;  
Willst du die Anderen verstehn, blick in dein eigenes Herz.*”

Whatever may be said in depreciation of the so-called ‘old psychology,’ it cannot be denied that its more permanent and rapid gains were largely due to the fact that the problems of the human soul have enlisted the efforts of so many men widely acquainted with, and sympathetically interested in, the whole body of their fellow-men. The knowledge of human mental life and development which is obtained by experience only with certain classes of people, or with certain aspects of human nature, is, indeed, usually prejudiced, narrow, and not entitled to credit for its scientific character. But it all furnishes invaluable material for the scientific psychologist. The man of business, the physician, the pastor, the police judge or keeper of



the jail, and even the gambler, the tramp, the prostitute, knows something that answers to fact and to reality about the soul of man. This knowledge, too, has a bearing upon the problems of the school.

In my judgment—to cite one or more examples—it is not scientific, not to say ethically proper, without knowing what observers of human nature in the large know, to establish determinism on the basis of a few thousand reactions in the psychological laboratory; or to resolve the moral and religious sentiments of humanity into modifications of the pleasure-pains; or to deny the rights of that instinctive metaphysics without which the ‘man of the school’ is justly deemed by the ‘man of affairs’ to be lacking in ‘common sense.’

The soul of man is no simple equation to be stated in terms of the ‘differentiation,’ ‘aggregation,’ ‘redintegration,’ of sensational factors. Its manifold beliefs, fears, hopes, aspirations, and even cognitions, that take hold on what is forever hidden from sense, and yet give support and value to sensation itself, are integral ‘moments’ in its own being. The scientific psychologist, much more than the Latin poetizer, is committed to the principle of not esteeming anything human foreign to himself. And while he must, of course, restrict his more special investigations to comparatively narrow lines, if this is done in the spirit of aloofness from, or in ignorance of, the actual human nature of the multitude of men, he cannot claim to have the best fitness for the pursuit of his chosen science. Above all other hindrances will the genuine student of psychology dread the limitations of academic narrowness and bigotry. For him the worst of slaveries is to be confined by the bands of the scholastic temper and habit of life. Better the ornithologist who knows nothing of birds in the wild wood, or the geologist who has studied only the collections of his college museum, or the anatomist who has dissected only the manikin of his medical school, or the artist who knows only the artificial poses of his favorite model, than the psychologist who has no wider acquaintance with the souls of men than the laboratory, or the class-room, or the book of his favorite author, can give him.

The teachers of psychology in the higher institutions of learn-

ing in this country are to-day, on the whole, much better trained in scientific spirit and method, much better acquainted with the technique and the literature of their science, than were the teachers of twenty-five and fifty years ago. For this reason they are more largely the younger men. These facts are on the whole hopeful for the more satisfactory progress in the future of psychology and the allied sciences. But unless at the same time their interest in, and acquaintance with, human life in a large way is correspondingly cultivated, these teachers will not in some respects be the equals, much less the superiors, of the teachers of the olden time. He cannot know, or understand—whether to describe or to explain—what is ‘in man,’ who has not somehow had it first in his own experience with himself.

It is perhaps partly in reaction against an excessive scholastic spirit and method, that another and quite opposed hindrance to the progress of psychology has recently appeared. I refer now to an absurd surplusage of attempts to render the science popular, which has emanated chiefly from writers who lack almost all the qualifications of the trained expert. It would seem as though the secret meditations of not a few of these popularizers of psychology might be expressed in somewhat the following fashion. “These professors of psychology, these *Fach-Seelenforscher*, are not up to their business; for they are not telling the people much that is new about human nature; and what they do tell is not intelligible to the people, neither is it expressed in an altogether taking way. Go to, now: I will show them how to do it. Since I am a teacher of something, or at least know what plain people want, I will be a teacher of psychology to these same plain people. That is to say, I will write a book which shall have all the science of the professor, and shall also be easily intelligible and practical.”

Perhaps such expressions as the foregoing misinterpret the consciousness of this swarm of improvised teachers of psychological science in America to-day. But there is one fact which seems to admit of only one conclusion: the multiplication of books popularizing psychology, written by authors who have never had any truly scientific training, is, on the whole, a distinct hindrance to the best progress of this science. And, in-

deed, what valid reason is there for such an altogether disproportionate affliction of this particular science in this particular way? Books on physics, chemistry, zoölogy, physiology, do not proceed in rival numbers from the pens of men who have never made any prolonged, well concentrated, and judiciously guided study of the subjects treated by these positive sciences. We are inclined to look somewhat too contemptuously upon the 'old psychology' and upon its teachers, because mere schooling as a minister, when made up into a college president, was esteemed a sufficient test of fitness to exploit one's self as an authority in psychological science. But what better is this modern way of gathering from here and from there, concealing in whole or in part the sources from which the information is gathered, emphasizing the didactic calling as practiced from the platform of the school rather than that of the church, and then issuing a patchwork book into the already overcrowded shelves of the publishers?

Fortunately, however, these two opposite tendencies may be expected in time to correct each other; and if those authorities who have scientific preëminence and scholastic opportunity finally get their scientific knowledge popularized, the crude attempt at popularization made by the multitude of tyros may have prepared the way for them. In the meantime, the multitude of books on psychology by those who have really never made any serious, not to say sufficient, study of psychology would seem to be a hindrance to the progress of the science.

Another hindrance to the more rapid advance of psychology is to be found, I think, in the *manner* in which much of the presentation and discussion of psychological problems is conducted. This manner is by no means always or even generally ill-meant: nevertheless, it seems to me inconsistent with the higher and more noble purposes of psychological science. For, in the first place, it excites suspicion, if it does not engender distrust and scorn, on the part of the community looking on from outside the inner circle of psychologists themselves. A dead uniformity of opinion, or an enforced consensus of expression, is, of course, not the genuine scientific attitude toward any subject of investigation; nor does it further the progress of



any science to try to secure prematurely such opinion, such consensus. At the same time, without some common basis of knowledge and of method, it is foolish to speak of the 'science' of psychology at all, and idle to form an association of men who are in the common pursuit of such science. For one, I believe that there already exists a science of psychology. It is not all a 'natural science,' in the sense of being a physiological or cerebral psychology: it is not all an experimental psychology, or a psycho-physical psychology, or a speculative psychology. It is simply the net result of human experience, gathered in whatever way, as to the faithful description and satisfactory explanation of the mental life of the individual man. These net results constitute a very respectable body of established truths; they are the science of psychology. Whoever underestimates and depreciates these commonly accepted truths, and over-emphasizes his own peculiar conclusions or methods to the discredit of these truths, is likely to hinder rather than to advance the real interests of psychology. Before the layman he makes the same impression which is made by the new recruit to the missionary force when he proceeds at once to proclaim loudly the differences of his sect or school from all others that bear the common name of Christian.

One cannot for a moment believe that the psychologists of this country are any less under the dominion of authority, or any more exacting in the tests they apply to their hypotheses and theories, than are the physicists, the chemists or the biologists. Yet it is my impression that the latter are, when compared with the former, more respectful toward matured opinions, more appreciative of long-continued services, more accustomed to place the emphasis—where it belongs—upon the growing body of accredited conclusions, and more courteous in the discussion of minor differences. It will be said that if such a difference really exists, it is due to the difference in the character of the subjects studied. For my part, I believe that this, too, is an affair of *personnel* rather than of *matériel*.

In close connection with these differences, one is almost forced to remark another difference that concerns the literary style of that discussion of mooted points in which we psycholo-

gists are apt to indulge. Am I wrong in believing that there is a higher average of dignity and reserve in the polemics of our colleagues of the physico-chemical and biological sciences? Certainly 'discussion' is indispensable to progress in psychology as in all the other positive forms of science; and the place given to it in the journalism or in the associations of psychologists is not too large. Even lively polemics over scientific subjects is not always undesirable: although it is probable that a careful examination of the history of the development of the positive sciences would show that polemics really counts for comparatively little as a contribution to such development. Most of the work which really advances science is done by those who are most diligent in research, patient in forming their own conclusions, and least eager to play the part of the brilliant and destructive critic of other men's conclusions.

But one fails to see—at least in the first instance—why the jaunty, snappy, newspaper style should be more appropriate to the science of psychology than to any of the other natural sciences. Is the soul of man itself such an inferior kind of reality that the discussion of its attributes, activities and development, should run the risks of the other worst improprieties, in order to escape the charge of dullness? Is not levity as inappropriate to the scientific examination and exhibition of the facts of human consciousness, as it is to the description and explanation of the behavior of an *amœba* or of the development of the egg from a common barn-fowl? Or, again, why should voluminous and prolonged descriptions of a certain species of micro-organisms be welcomed as worth years of scientific research, and then similar studies of mental development be subjected to jest or to fault-finding for the same qualities of thoroughness—and this by psychologists themselves?

On approaching the next, and fourth, kind of hindrances to the best progress of psychological science in America, so far as these hindrances are under the immediate control of psychologists themselves, I am well aware of treading on yet more doubtful and dangerous ground. I must again, however, ask a brief indulgence for the expression of somewhat vague beliefs and fears rather than conclusions based on indisputable facts.

These beliefs and fears it is which lead me to say that the growth of the *commercial spirit* within academical circles, and as touching scholastic affairs, is becoming a real hindrance to psychological science. Now the spirit of genuine science is sincere and unselfish. The man who adopts as his profession any form of science—to use a familiar expression—chiefly ‘for what he can make out of it’ is really not *in*, or inside of, that science at all. But the mind that follows the science of man’s mental life, through feelings of personal ambition, or under the influence of jealousy, prejudice or bigotry—either theological or anti-theological—makes itself thereby less capable of discerning and appreciating its full content, functioning and development.

That the ‘commercial spirit’ is, the world over, just now increasingly dominant in social and political institutions and relations, admits of no doubt. Hitherto the higher educational circles and institutions of this country have had a large and fortunate exemption from the influences of this spirit. They are still in the enjoyment of a relative exemption from these influences. The teachers of the sciences are still, I believe, less dominated by merely personal considerations in the pursuit of their ideals—the ideals of knowledge as related to the increase of human well-being—than are any other class of men in the country, clergymen not excepted. But there are signs that the commercial spirit is to a certain extent displacing the truly scientific spirit, even in these higher scholastic circles.

I find these signs of the intrusion of the commercial spirit upon the domains of science in the following results: To this spirit it is due, in part, that there is an increasing amount of premature publication on the subject of psychology. I am not one of those who believe that the student should withhold his conclusions until he has made them absolutely unassailable in respect of proof, and perfect in form. If this were the rule, no wise man would ever publish anything. It is quite legitimate, moreover, to subject one’s own work, while immature—and even on account of its confessed immaturity—to the criticism of one’s colleagues. For the metabolism of every body of a positive science consists in the appropriation of only a part of what



is offered to it, and in the rejection of the remainder. But when premature publication is largely encouraged by the ambition to get one's self into notice, or to better one's financial condition, rather than to advance the cause of science, its excess may become a positive hindrance to science.

In part, also, to the growth of the commercial spirit is due the practice of saying really commonplace things in strange and unfamiliar ways; as though, indeed, mannerism in expression, or license in the invention of new terms, would be mistaken for originality in research and for independence of thinking. Doubtless, psychology has the same right as any other science to develop a technical terminology. And if a new mental factor, or faculty, or law of mental life, is discovered by any student of psychology, and no appropriate term for the discovery seems to be at hand, why—I, for one, am not going to say that the discoverer is not entitled to signalize the triumph of his insight by giving a new name to his discovery. At the same time, the science of psychology may well be very conservative in such matters. A time-honored truth is no better, a time-worn fallacy is no more acceptable, because either is presented in language calculated to deceive the laity into thinking that it is the latest thing in modern psychology. Genuine science will not increase its speed by exacerbating our characteristic American impatience. After all, even modern science goes pretty slow; not a few of its most loudly applauded recent results will probably have to be carefully reconsidered and much modified before they are adopted into the body of its assured results.

Hitherto the pursuit of the positive sciences in this country has been remarkably free, as compared with European countries generally, from hindrances growing out of personal and institutional jealousies. This freedom has been partly due to difference in the mode of making academic appointments, and in the relations of supply and demand as touching the candidates for these appointments. There are some signs that the growth of the commercial as distinguished from the genuine scientific spirit is beginning to breed and to foster personal and institutional jealousies among us. If these signs tell the truth, then the truth is to be deplored. But surely the remedy for this hin-

drance to the more rapid progress of psychology, if it exist, is an affair of *personnel* rather than of *matériel*.

There is one other hindrance to psychology, as a claimant to some established position among the positive sciences, which I wish to note. This is a certain vacillating and insecure attitude toward the other most closely allied sciences—an attitude somewhat similar to that of the *nouveaux riches* toward the recognized aristocratic classes of society. I firmly believe that psychology, in respect both of subject-matter and of method, and also of available accumulations, might make itself entitled to take a place of equality—equally independent and free-spirited, equally docile and temperate—among the modern sciences. The physico-chemical and biological sciences all have much wealth of knowledge and of technique to share with psychology; I believe that psychology might have something approaching an equal value to share with them. And the recent ‘affiliation’ to which every meeting of this Association bears witness is one of the best signs of the ‘better-time-coming’ for psychological science.

The older psychology was too much disposed to maintain an attitude of exaggerated independence, of stiff and proud aloofness, toward physics, chemistry and the biological sciences. Its teachers knew that their souls were their own; and they often appeared to suppose that the scientific study of these souls could be best conducted in complete disregard of the physical conditions and environment in which all mental life and mental development is set. The new psychology, in its proper reaction from this attitude of unscientific isolation, is tempted to take an attitude of equally unscientific servility. Its teachers are less sure that their souls are their own, or even that there are any souls, than were the teachers of the earlier days. Some of them are less sure than is the average chemist, physicist or biologist, who—however modern he may be in his own specialty—is rather apt to be conservative with regard to the existence of his own soul.

In the history of scientific development always, but perhaps never more than of late, there has existed in the minds of some—enthusiastic dreamers, for the most part, albeit often men of

great eminence in the particular sciences—the captivating notion of a ‘*universal*’ science. If we could only get at this one science, in all its depth and height and length, then we should have at least the key to all the mysteries of universal Nature. She would, to be sure, still remain rather a complicated and somewhat freakish and irregular creature; but man would have the science of her, in the large, as it were. Of late, the last-century conception of the universal mechanism, under which all selves and all things alike come, has been somewhat thoroughly shaken up. But the demand, or the hortation, for another step toward the ideal of unity, is generally issued at present by some one of the particular sciences to those others which lie nearest its own door. For example, physics may be willing to unify chemistry—by absorbing it into itself. Chemistry may wish to effect a complete harmony with physiology, in somewhat the same way. Undoubtedly, in the minds of a multitude of biologists, psychology, as a science, is only a subdivision of biology, a dependent branch on the tree of universal life. All this reminds one of the current practical proposals to effect a unity of the Church, which, in the thought of each particular denomination, take the form of an ‘*embrace*ment’ of all the other denominations, by that particular one making itself the universal.

On the other hand, I wish to testify out of my personal experience that I have found more of the truly scientific reserve and caution, in the matter of premature and unverified extension of their own principles, on the part of the most thoroughly cultivated men in the physico-chemical and biological sciences, than on the part of a large number of psychologists when dealing with these same physical and biological principles. Suppose, for example, the question arises as to the bearing of the theory of the conservation and correlation of energy upon the psychological problem of the will; or that the accepted principles of cerebral physiology—granted the very doubtful claim that such principles can be found—be asked to contribute to the discussion of the hypothesis of psycho-physical parallelism; or what not among hypotheses of this order. It is my experience that the psychologist who has only a smattering of knowledge on these



physico-chemical subjects is much the likelier to take the unscientific and prematurely 'cocksure' position, with regard to their application to psychological subjects—in the name of the borrowed, but misunderstood and misapplied, authority of the chemico-physical and biological sciences.

But however this may be, there is little doubt that any other than an independent attitude, which is also respectful and docile, toward allied sciences is distinctly disadvantageous to the science of psychology. There is just as little doubt that the vacillating and uncertain or servile attitude toward certain other sciences, which not a few students of psychology assume, is a convincing witness to a raw and immature and misinformed condition of mind respecting their own science. Psychology, if it wishes to get more respect from the other members of the great brotherhood of science, must respect itself. In order to entitle itself to more self-respect and to more of respect from others, it must be, of course, respectful and teachable toward all truth; but it must also know its own peculiar rights of domain, must maintain and defend them, and must cultivate this domain by its own somewhat peculiar methods, with the free and independent spirit which belongs to every worker in every field of science. Psychology must remain 'affiliated;' it must enter more intimately into the circle of affiliated science; but it must go there more and more richly laden, to teach and to learn, as 'one among many' who are really all working toward the same end. That end is the scientific conquest of all reality, to the improvement of human society.

Well, brethren of the Association of Psychologists in America, I have expressed somewhat freely my private opinions. They are *mere* opinions; and you will, of course, take them only for what they may seem to you worth. There is, of course, another and brighter side; abundant helps and signs of progress, as well as certain hindrances and indications of an unsatisfactory rate of progress. It is of the latter, so far as they belong more to *personnel* than to *matériel*, that I have ventured to speak. And the practical lesson, if there be any, is obvious.

## THE EVOLUTION OF MODESTY.

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Modesty—which may be provisionally defined as an almost instinctive fear, prompting to concealment, and usually centering around the sexual nature—while common to both sexes is more especially feminine, so that it may almost be regarded as the chief secondary sexual character of women on the psychic side. The woman who is lacking in this kind of fear is lacking also in sexual attractiveness to the normal and average man. As a psychic secondary sexual character of the first rank, is it necessary, before any psychology of sex can be arranged in order, to obtain a clear view of modesty.<sup>1</sup>

I have not, however, been able to find that the subject of modesty has been treated in any comprehensive way by psychologists. Though valuable facts and suggestions bearing on sexual emotions, on disgust, on the origin of tattooing, on ornament and clothing, have been brought forward by physiologists, psychologists and ethnographers, few or no attempts appear to have been made to reach the general synthetic statement of these facts and suggestions.<sup>2</sup> The subject is indeed complicated by

<sup>1</sup> I may remark that the present paper is an abstract of a study to be published in the second volume of my *Studies in the Psychology of Sex*.

<sup>2</sup> It is true that many unreliable, slight or fragmentary attempts have been made to ascertain the constitution or basis of this emotion. Herbert Spencer, followed by Sergi and others, regarded modesty simply as the result of clothing. This view is overturned by the well ascertained fact that many races which go absolutely naked possess a highly developed sense of modesty. These writers have not realized that psychological modesty is earlier in appearance, and more fundamental, than anatomical modesty. A partial contribution to the analysis of modesty has been made by Professor James, who with his usual insight and lucidity has set forth certain of its characteristics, especially the element due to 'the application to ourselves of judgments primarily passed upon our mates.' Westermarck, again, followed by Grosse, has very ably and convincingly set forth certain factors in the origin of ornament and clothing, a subject which

the difficulty of excluding closely allied emotions,—shame, shyness, bashfulness, timidity, etc.—all of which, indeed, however defined, adjoin and overlap modesty.<sup>1</sup> It is not, however, impossible to isolate the main body of the emotion of modesty, on account of its special connection, on the whole, with the consciousness of sex. I here attempt, however imperfectly, to reach my own analysis of its constitution and development.

That modesty is based on fear, one of the most primitive of the emotions, seems to be fairly evident. It is, indeed, an agglomeration of fears, especially, as I hope to show, of two important and distinct fears, one of much earlier than human origin and supplied solely by the female, the other of more distinctly human character and of social rather than sexual origin.

A child left to itself, though very bashful, is wholly devoid of modesty. Every one is familiar with the shocking *inconvenances* of children in speech and act, with the charming ways in which they innocently disregard the conventions of modesty their elders thrust upon them, or, even when anxious to carry them out, wholly miss the point at issue.

Under civilized conditions, moreover, the convention of modesty long precedes its real development. It may fairly be said that this takes place at the advent of puberty. We should not, however, be justified in asserting that on this account modesty is a purely sexual phenomenon. The social impulses also develop about this time, and to that coincidence the com-

many writers imagine to cover the whole field of modesty. More recently, Ribot, in his work on the emotions, has vaguely outlined most of the factors of modesty, but has not developed a coherent view of their origins and relationships.

<sup>1</sup> Timidity, as understood by Dugas in his interesting essay on that subject, is probably most remote. Dr. H. Campbell's 'Morbid Shyness' (*British Medical Journal*, 26 September, 1896) is in part identical with timidity, in part with modesty. The matter is further complicated by the fact that 'modesty' itself has in English (like virtue) two distinct meanings. In its original form it has no special connection with sex or with woman, but may rather be considered as a masculine virtue. Cicero regards 'modestia' as the equivalent of the Greek *σωφροσύνη*. This is the 'modesty' which Mary Wollstonecraft eulogized in the last century, the outcome of knowledge and reflection, 'soberness of mind,' 'the graceful calm virtue of maturity.' In French it is possible to avoid this confusion, and 'modestie' is entirely distinct from 'pudeur.' It is of course with 'pudeur' that I am here concerned.



pound nature of the emotion of modesty may well be largely due.

The sexual factor is, however, the simplest and most primitive element of modesty, and may, therefore, be mentioned first.

This fundamental animal factor of modesty,<sup>1</sup> rooted in the natural facts of the sexual life of the higher mammals, and especially man, obviously will not explain the whole phenomena of modesty; it fails to account for ornaments and garments, and it scarcely appears to present an adequate basis for modesty in the male. For this we must, in large part at least, turn to the other great primary element of modesty, the social factor.

We cannot doubt that one of the most primitive and universal of the social characteristics of man is an aptitude for disgust, founded as it is on a yet more primitive and animal aptitude for disgust which has little or no social significance. In nearly all races, even the most savage, we seem to find distinct traces of this aptitude for disgust in the presence of certain actions of others, an emotion naturally reflected in the individual's own actions, and hence a guide to conduct. Notwithstanding our gastric community of disgust with lower animals, it is only in man that this disgust seems to become highly developed, to possess a distinctly social character, and to serve as a guide to social conduct. The objects of disgust vary infinitely according to the circumstances and habits of particular races, but the reaction of disgust is fundamental throughout.

The best study of the phenomena of disgust known to me is without doubt Professor Richet's.<sup>2</sup> Richet concludes that it is the *dangerous* and the *useless* which evoke disgust. Certain excretions and secretions, being either useless or, in accordance with wide-spread primitive ideas, highly dangerous, the sacro-pubic region became a concentrated focus of disgust. It is for this reason, no doubt, that savage men exhibit modesty, not only towards women, but towards their own sex, and that so many of

<sup>1</sup> For the detailed treatment of which the forthcoming work may be consulted.

<sup>2</sup> C. Richet, 'Les causes du dégoût,' *L'homme et l'intelligence*, 1884. This eminent physiologist's elaborate study of disgust was not written as a contribution to the psychology of modesty, but it forms an admirable introduction to the investigation of the social factor of modesty.

the lowest savages take precautions to obtain seclusion for the fulfilment of natural functions. The statement now so often made that the primary object of clothing is to accentuate rather than to conceal has in it—as I shall point out later—a large element of truth, but it is by no means a complete account of the matter. It seems very difficult not to admit that there is a genuine impulse to concealment among the most primitive peoples, and the invincible repugnance often felt by savages to remove the girdle or apron is scarcely accounted for by the theory that it is a sexual lure.

In this connection it seems to me instructive to consider a special form of modesty very strongly marked among savages in some parts of the world. I refer to the feeling of immodesty in eating. When this feeling exists, modesty is offended when one eats in public; the modest man retires to eat. Indecency, said Cook, was utterly unknown among the Tahitians; but they would not eat together; even brothers and sisters had their separate baskets of provisions, and generally sat some yards apart, with their backs to each other, when they ate.<sup>1</sup> Karl von den Steinen remarks, in his interesting book on Brazil, that, though the Bakairi of Central Brazil have no feeling of shame about nakedness, they are ashamed to eat in public: they retired to eat, and hung their heads in shamefaced confusion when they saw him innocently eat in public. Hrolf Vaughan Stevens found that, when he gave an Orang Lâut (Malay) woman anything to eat, she not only would not eat if her husband were present, but if any man were present she would go aside before eating or giving her children to eat.<sup>2</sup>

It is quite easy to understand how this arises. Whenever there is any pressure on the means of subsistence, as among

<sup>1</sup> Crawley (*Jour. Anthropological Inst.*, May, 1895) gives numerous similar instances, even in Europe, with, however, special reference to sexual taboo. I may remark that English people of lower classes, especially women, are often modest about eating in the presence of people of higher class. This feeling is no doubt due in part to the consciousness of defective etiquette, but that very consciousness is a development of the fear of causing disgust which is a component of modesty.

<sup>2</sup> Stevens, 'Mittheilungen aus dem Frauenleben der Orang Belendas,' *Zt. für Ethnologie*, 1896, Heft IV., p. 167.

savages at some time or another there nearly always is, it must necessarily arouse a profound emotion of anger and disgust to see another person putting into his stomach what one might as well have put into one's own. The special secrecy sometimes observed by women is probably due to the fact that women would be more sensitive to the emotion of disgust than the act of eating arouses in onlookers. As social feeling develops a man desires not only to eat in safety, but also to avoid being an object of disgust, and to spare his friends all unpleasant emotions. Hence it becomes a requirement of ordinary decency to eat in private. A man who eats in public becomes—like the man who in our cities exposes his person in public—the object of disgust and contempt.

Long ago, when a hospital student on midwifery duty in London slums, I had occasion to observe that among the women of the poor, and more especially in those who had lost the first bloom of youth, modesty consisted chiefly in the fear of being disgusting. There was almost a pathetic anxiety, in the face of pain and discomfort, not to be disgusting in the doctor's eyes. This anxiety expressed itself in the ordinary symptoms of modesty. But as soon as the woman realized that I found nothing disgusting in whatever was proper and necessary to be done under the circumstances, it almost invariably happened that every sign of modesty at once disappeared. In the special and elementary conditions of parturition, modesty is reduced to this one fear of causing disgust, so that when that is negated, the emotion is non-existent and the subject becomes, without an effort, as direct and natural as a little child. A fellow-student on similar duty, who also discovered for himself the same character of modesty, remarked on it to me with some sadness; it seemed to him derogatory to womanhood that what he had been accustomed to consider its supreme grace should be so superficial that he could at will set limits to it. I thought then, as I think still, that that was rather a perversion of the matter, and that nothing becomes degrading because we happen to have learnt something about its operations. But I am more convinced than ever that the fear of causing disgust—a fear quite distinct from that of losing sexual lure or breaking a rule of social etiquette—plays



a very large part in the modesty of the more modest sex and in modesty generally. Whatever magnifies self-confidence and lulls the fear of evoking disgust—whether it is the presence of a beloved person in whose good opinion complete confidence is felt, or whether it is merely the grosser narcotizing influence of a slight degree of intoxication—always automatically lulls the emotion of modesty. Together with the sexual factor, the social fear of evoking disgust seems to me the most fundamental element in modesty.

It is on this animal basis that the human and social fear of arousing disgust has developed. Among civilized people, it may be added, the fear of arousing disgust is the ultimate and most fundamental element of modesty.

Another factor of modesty, which reaches a high development even in savagery, and among more or less naked races, is the idea of ceremonial uncleanness. It may be to some extent rooted in the elements already referred to, and it leads us into a much wider field than that of modesty, so that it is only necessary to mention it here. Ritual tends to crystallize around any act of life on which men expend deliberate attention, and the duties of modesty among savages are a sufficiently serious part of life to constitute a nucleus for ritual. No doubt offences against ritual may be regarded as more serious than offences against modesty, but they are so obviously allied in early culture that the one reinforces the other, and they cannot be easily disentangled. All savage and barbarous people who have attained any high degree of ceremonialism have included certain animal functions more or less stringently within the bonds of that ceremonialism. It is only necessary to refer to the Jewish ritual books of the Old Testament, to Hesiod, or to the customs prevalent among Mohammedan peoples.

So far it has only been necessary to refer incidentally to the connection of modesty with clothing. I have sought to emphasize the unquestionable but often forgotten fact that modesty is in its origin independent of clothing, that physiological modesty takes precedence of anatomical modesty, and that the primary factors of modesty were developed long before the discovery of either ornament or garments. The rise of clothing probably

had its first psychic basis on an emotion of modesty already compositely formed of the elements we have traced. Both the main elementary factors, it must be noted, must naturally tend to develop and unite in a more complex, though, it may well be, much less intense emotion. A very notable advance, I may remark, is made when the primary attitude of defence against the action of the male becomes merely a defence against his eyes. We may thus explain the spread of modesty to various parts of the body. We see the influence of this defence against strange eyes in the special precautions in gesture or clothing taken by the women in various parts of the world against the more offensive eyes of civilized Europeans.

But in thus becoming directed merely against sight and not against action, the gestures of modesty are at once free to become merely those of *coquetterie*. When there is no real danger of offensive action, there is no need for more than playful defence, and no serious anxiety should that defence be taken as a further invitation. Thus the road is at once fully open towards the most civilized manifestation of the comedy of courtship.

In the same way the social fear of arousing disgust combines easily and perfectly with any new development in the invention of ornament or clothing as sexual lures. Even among the most civilized races it has often been noted that the fashion of feminine garments (as also sometimes the use of scents) has the double object of concealing and attracting. The heightening of attraction is indeed a logical outcome of the fear of evoking disgust.

The contention of Westermarck, that ornament and clothing are in large part due to the desire to give not concealment but greater prominence, may certainly be accepted, so long as we realize that it is not the whole of the truth, and that it is far from offering a complete explanation of the phenomena of modesty. The great artistic elaboration often displayed by such articles of ornament and clothing, even when very small, and the fact—as shown by Karl van den Steinen regarding the Brazilian *uluri*—that they may serve as common elements in general decoration, sufficiently prove that such objects attract rather than escape attention. And while there is an invincible

repugnance among some peoples to remove these articles, such repugnance being often strongest when the adornment is most minute, others have no such repugnance, or are quite indifferent whether or not their aprons are accurately adjusted. The mere presence or possession of the articles gives the required sense of self-respect, of human dignity, of sexual desirability. But, on the whole, all the motives already noted combine to concentrate modesty on the garment.

When clothing is once established, another element, this time a social-economic element, often comes in to emphasize its importance and increase the anatomical modesty of women. I mean the growth of the conception of women as property. Waitz, followed by Schurtz and Letourneau, has insisted that the jealousy of husbands is the primary origin of clothing and, indirectly, of modesty. It is undoubtedly true that married women are often only or chiefly clothed, while the unmarried women, though full-grown, are not. In many parts of the world, also, Mantegazza and others have shown, where the women are covered and the men are not, clothing is regarded as a sort of disgrace, and men can only with difficulty be persuaded to adopt it. Before marriage a woman was often free and not bound to chastity, and at the same time was often unclothed; after marriage she was clothed and no longer free. To the husband's mind, the garment appears—illogically though naturally—a moral and physical protection against any attack on his property. Thus a new motive was furnished, this time somewhat artificially, for making nakedness, in women at all events, disgraceful. As the conception of property also extended to the father's right over his daughters, and the appreciation of female chastity developed, this motive spread to unmarried and married women alike. It probably constitutes the chief element furnished to the complex emotion of modesty by the barbarous stages of human civilization.

The chief new feature—it is scarcely an original element—added to modesty when an advanced civilization slowly emerges from barbarism is the elaboration of its social ritual. Civilization expands the range of modesty and renders it more capricious and changeable. The French seventeenth century and



the English eighteenth represent early stages of modern European civilization, and they both devoted special attention to the elaboration of the minute details of modesty. The frequenters of the Hôtel Rambouillet, the *précieuses* satirized by Molière, were primarily engaged in refining the language, but indirectly also in refining feelings and ideas and in enlarging the boundaries of modesty. In England such famous and popular authors as Swift and Sterne bear witness to a new ardor of modesty in the sudden reticences, the dashes and the asterisks, which we find throughout their works. The altogether new quality of literary prurience of which Sterne is still the classic example could only have arisen on the basis of the new modesty which was then overspreading society and literature. Idle people, mostly the women in *salons* and drawing-rooms, people more familiar with books than with the realities of life, now laid down the rules of modesty, and were ever enlarging it, ever inventing new subtleties of gesture and speech, which it would be immodest to neglect, and which were ever being rendered vulgar by use and ever changing.

It would, however, be a mistake to suppose that this process is an intensification of modesty. It is, on the contrary, an attenuation of it. The observances of modesty become merely a part of a vast body of rules of social etiquette, though a somewhat stringent department of these rules on account of the vague sense still persisting of a deep-lying natural basis. The whole emotion has been, in a certain sense, undermined, and yields more readily than in its primitive state to any invasion supported by a sufficiently strong motive. The savage Indian woman of America, the barbarous woman of some Mohammedan countries, can scarcely sacrifice their modesty even in the pangs of childbirth. Fashion, again, in the more civilized countries can easily inhibit anatomical modesty, and rapidly exhibit in turn almost any portion of the body. In savage and barbarous countries modesty often possesses the strength of a genuine and irresistible instinct. In civilized countries any one who places considerations of modesty before the claims of some real human need excites ridicule and contempt.

It is, however, impossible to contemplate this series of phe-

nomena, so radically persistent, whatever its changes of form, and so constant throughout every stage of civilization, without feeling that, although modesty cannot properly be called an instinct, there must be some physiological basis to support it. Undoubtedly such a basis is formed by that vasomotor mechanism of which the most obvious outward sign is in human beings the blush.<sup>1</sup> All the allied emotional forms of fear—shame, bashfulness, timidity—are to some extent upheld by this mechanism, but this is especially the case with the emotion we are now concerned with. The blush is the sanction of modesty.

When the Brazilian offered Karl van den Steinen some food which he ate immediately in public, the Brazilian hung his head. Whether or not he blushed, he was certainly conscious of that capillary turmoil of the face, of which the shock of offended modesty is the cause and blushing the most visible sign. It is scarcely an accident that, as has been often observed, criminals or the anti-social element of the community—whether by the habits of their lives or by congenital abnormality—blush less easily than normal persons.<sup>2</sup> The importance of the blush and the emotional confusion behind it as the sources of modesty is shown by the significant fact that by skillfully lulling emotional confusion it is possible to inhibit the sense of modesty itself. In other words, it may be said that we are here in the presence of a fear—to a large extent a sex-fear—impelling to concealment, and the emotion naturally disappears, even though its ostensible

<sup>1</sup>The blush is indeed only a part, almost perhaps an accidental part, of an organic turmoil with which it is associated. Partridge, who has studied the phenomena of blushing in 120 cases (*Pedagogical Seminary*, April, 1897), finds that the following are the chief symptoms: tremors near the waist or passing from the feet to the head, weakness in the limbs, pressure, trembling, warmth, weight, a beating in chest, warm wave from feet upwards, quivering of heart, stoppage and rapid beating of heart, coldness all over followed by heat, dizziness, tingling of toes and fingers, numbness, something rising in throat, smarting of eyes, ringing of ears, prickling sensation of face, pressure inside head.

<sup>2</sup>Kroner (*Das körperliche Gefühl*, 1887, p. 130) remarks: "The origin of a specific connection between shame and blushing is the work of a *social selection*. It is certainly an immediate advantage for a man not to blush; indirectly, however, it is a disadvantage, because in other ways he will be known as shameless, and on that account, as a rule, he will be discriminated against in marriage. This social selection will be especially exercised on the female sex, and on this account women blush to a greater extent and more readily than men."

cause remains, when it is apparent that there is no cause for fear. Thus it is, to some extent at least, true that people are modest because they blush, or because they feel the possibility of blushing, rather than that they blush because they are modest. In the same way we may explain the curious influence of darkness in restraining the manifestations of modesty.<sup>1</sup> This mechanism of blushing thus runs parallel, on the physiological side, with that fear of evoking disgust to which I have already referred. It is to the blush also that we must attribute a curious complementary relationship between the face and the sacro-pubic region as centers of anatomical modesty. The women of some African tribes who go naked, Ploss remarks, cover the face with the hand under the influence of modesty. When, as among many Mohammedan peoples, the face is the chief focus of modesty, the exposure of the rest of the body, including even the sacro-pubic region, becomes a matter of comparative indifference. All such facts serve to show that, though the forms of modesty may change, it is yet a very radical constituent of human nature in all stages of civilization, and that it is to a large extent maintained by the mechanism of blushing.

It may still be asked, finally, whether on the whole modesty really becomes a more predominant emotion as civilization advances. I do not think this position can be maintained. It is a great mistake, as we have seen, to suppose that in becoming extended modesty also becomes intensified. On the contrary, this very extension is a sign of weakness. Among savages modesty is far more radical and invincible than among the civilized. Of the Araucanian women of Chili Treutler has remarked that they are distinctly more modest than the Christian white population, and such observations might be indefinitely extended. It is, as we have already noted, in a new and crude civilization, anxious to mark its separation from a barbarism it has yet scarcely escaped, that we find an extravagant and fantastic anxiety to extend the limits of modesty in life and art and literature. In older and more mature civilizations—in classic

<sup>1</sup>The influence of darkness in inhibiting modesty is a very ancient observation. Burton in the *Anatomy of Melancholy* quotes Dandinus: 'Nox facit impudentes,' rightly connecting the influence with blushing.



antiquity, in old Japan, in France—modesty, while still a very real influence, becomes a much less predominant and all-pervading influence. In life it becomes subservient to human use, in art to beauty, in literature to expression. Among ourselves we may note that modesty is a much more invincible motive among the lower social classes than among the more cultivated classes. Modesty is a part of self-respect, but in the fully developed human being self-respect itself holds in check any excessive modesty. We must remember, moreover, that there are more definite grounds for the subordination of modesty with the development of civilization. We have seen that the factors of modesty are many, and that most of them are based on emotions which make little urgent appeal save to races in a savage or barbarous condition. Thus disgust, as Richet has truly pointed out, necessarily decreases as knowledge increases.<sup>1</sup> As we analyze and understand our experiences better, so they cause us less disgust. As disgust becomes analyzed, and as self-respect tends to increased physical purity, so the factor of disgust in modesty is minimized. The factor of ceremonial uncleanness, again, which plays so urgent a part in modesty at certain stages of culture, is to-day without influence, except in so far as it survives in etiquette. In the same way the social-economic factor of modesty belongs to a stage of human development which is wholly alien to an advanced civilization. Even the most fundamental impulse of all, the gesture of sexual refusal, is normally only imperative among animals and savages. Thus civilization tends to subordinate if not to minimize modesty, to render it a grace of life rather than a fundamental social law of life. But an essential grace of life it still remains, and whatever delicate variations it may assume we can scarcely conceive of its disappearance.

<sup>1</sup> Disgust is a sort of synthesis which attaches to the total form of objects, and which must diminish and disappear as scientific analysis separates into parts what as a whole is so repugnant.

PROCEEDINGS OF THE SEVENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, COLUMBIA UNIVERSITY, NEW YORK, DECEMBER, 1898.

REPORT OF THE SECRETARY FOR 1898.

The seventh annual meeting of the American Psychological Association was held at Columbia University, New York, December 28, 29 and 30, 1898, the same time and place having been chosen by the American Society of Naturalists and the Affiliated Societies.

In point of numbers the meeting was the most successful in the history of the Association, there being fifty-one members in attendance at the various sessions. On the morning of Thursday, a joint meeting with the American Physiological Society was held, members of both societies contributing papers, and, by invitation, Professor Ogden N. Rood, of the Department of Physics of Columbia University, read a paper on, and exhibited his Flicker Photometer. On Thursday afternoon the Association adjourned for the discussion before the Naturalists on 'Advances in Methods of Teaching,' Professor Münsterberg representing the Psychologists.

The members of the Association, for the most part, attended the addresses by Mr. Morris K. Jesup and Professor Henry F. Osborn, at the American Museum of Natural History, on Wednesday evening, and later the reception to the visiting societies, given by Professor and Mrs. Osborn, at their residence. About thirty members were present at the annual dinner of the Affiliated Societies, held at the Hotel Savoy, on Thursday evening. President Hugo Münsterberg presided at the meetings of the Association.

At the business meeting of the Association on Friday morning, the following was transacted: Election of officers for 1899:

*President*, Professor John Dewey, University of Chicago; *Secretary and Treasurer*, Dr. Livingston Farrand, Columbia University; *Members of the Council*, Professor J. McKeen Cattell, Columbia University, and Professor H. N. Gardiner, Smith College.

The following new members were elected: Dr. Raymond Dodge, Wesleyan University; Dr. Eleanor A. McC. Gamble, Wellesley College; Dr. Gervase Green, Yale University; Dr. A. L. Jones, Columbia University; Mr. James H. Leuba, Bryn Mawr College; Professor Ernest H. Lindley, University of Indiana; Dr. Walter T. Marvin, Columbia University; Mr. Will S. Monroe, State Normal School, Westfield, Mass.; Miss Ethel D. Puffer, Radcliffe College; Professor George Santayana, Harvard University; Professor Langdon C. Stewardson, Lehigh University; Dr. Edward L. Thorndike, Western Reserve University; Dr. Gustavo Tosti, New York City.

The following amendment to the constitution proposed at the meeting in Ithaca, in 1897, was taken up and passed, viz.: That the Secretary be elected for a term of three years and be *ex-officio* a member of the Council.

On motion of Professor Baldwin, a Standing Committee on Psychological and Philosophical Terminology was appointed, consisting of the following members: Professors Münsterberg, Cattell, Sanford, Creighton, Royce, Minot and Baldwin. The duties of this Committee shall be: (1) To recommend, from time to time, new terms in Psychology and Philosophy. (2) To recommend choice of alternative terms in those fields. (3) To recommend foreign equivalents for translating work both into English and into foreign languages. (4) To keep the Association informed as to the growth of terminology in other departments, especially in Neurology. The Committee shall have power to get help from foreigners who are not members of the Association, such individuals to be known as 'Associates' of the Committee.

On motion of Professor Sanford, it was

*Resolved: First*, that the matter of the organization of the Association with reference to a possible philosophical section be referred to the Council, to be reported upon at the next meeting;



*Second*, that the Secretary be instructed in arranging the programme for the next meeting to gather philosophical papers as far as practicable into the programme of one session; *Third*, that the Secretary be instructed to send out during the course of the year a circular letter requesting, for the information of the Council, the opinion of the individual members of the Association on the above mentioned question of the organization of the Association.

Professor Cattell, Chairman of the Committee on Physical and Mental Tests, presented the report of that Committee upon its work during the past year.

REPORT OF THE TREASURER FOR 1898.

Livingston Farrand in account with The American Psychological Association.

DR.

To balance at last meeting.....	\$669 10
Dues of members.....	249 00
Sale of Proceedings.....	25
	<hr/>
	\$918 35

CR.

By expenditures for

Postage, telegrams, etc.....	\$11 20
Stationery .....	5 70
Printing, clerical work, etc.....	22 57
Expenses of meeting of Affiliated Societies.....	3 00
Committee on Physical and Mental Tests.....	75 00
	<hr/>
	117 47

Balance on hand.....\$800 88

Audited by the Council and found correct.

LIVINGSTON FARRAND,  
*Secretary and Treasurer.*

ABSTRACTS OF PAPERS.

Address of the President: *Psychology and History*. By HUGO MÜNSTERBERG.

The psychological view of human life and the really historical view are necessarily in conflict; for the one the personality is a complex of elements and causally determined, for the other it is

a unity and free. The naturalistic tendencies of the last half century have favored the analytic and explanatory treatment, but our time shows a new revival of historical thinking. In this conflict the belief in the rights and duties of the personality must destroy psychology if it cannot be shown that both are partial truths, and thus no conflict between them necessary, since the one does not exclude the other. Recent writers have claimed, indeed, that psychology and history are two coördinated ways of dealing with the reality of life in so far as psychology seeks laws and tries thus to explain, while history deals merely with the single facts as such. These arguments are untenable: first, because every law implies also existential propositions and offers thus descriptions together with the explanations; secondly, because every description of single facts includes the laws, since the conceptions by which we describe are the condensed results of explanations; thirdly, because the single object as such, really isolated, is not object of any science but always object of art. Every science connects the facts, and, therefore, the historical sciences too must deal with general facts. There is thus no methodological difference between history and psychology. And yet a most important difference between the two does exist: it is an ontological difference. Both connect their material by general facts, but the material of psychology consists of objects which as such can be described and explained, while the material of history consists of subjective will acts which as such can merely be interpreted and appreciated. Our interest in objects means merely our expectation as to what we have to await from them; if we consider mental life as object, we transform it in the interest of causal connection and seek causal laws. The subjective will acts on the other hand interest us in the first instance with regard to their meaning; we want to understand with what other subjective acts they agree and disagree, and we come thus to a teleological system in which every will act is linked with every other will act as every molecule in the causal world is dependent upon the whole universe. In such a teleological system the general fact is then not a causal law but a will relation of inclusive character. As every willing personality can be thought of as replaced by the psychophysical organ-

ism, that is by an object, therefore every human experience can be brought into the causal and into the teleological system. As long as they are not mixed each is true, but each is a transformation of reality and not reality itself.

*Discussion on the Relations of Will to Belief.* PROFESSOR JAMES and DR. MILLER, who were to have taken part in the debate, being prevented from attending, the discussion was carried on by PROFESSORS LADD, HIBBEN, CALDWELL and ARMSTRONG, as follows :

By JOHN GRIER HIBBEN.

It will doubtless be conceded, by all who take part in this discussion, that the will does either directly or indirectly factor in those complex mental processes which lead to belief. A question which naturally suggests itself from the standpoint of logic is whether the presence of will in belief is a reflection upon man's reasoning powers. Should all conclusions be reached in the 'dry light of reason,' and when this is impossible should we withhold judgment altogether? This is an ideal which, in certain situations, it is impossible to realize, for we must distinguish between the area of exact knowledge and that larger sphere of our experience which lies beyond this area of light. In the former sphere our beliefs form a series of judgments grounded upon knowledge, elements which comprise a system of inter-related, coördinated parts. Here belief arises from evidence mediated by experiment, and admitting of exact verification. In such a sphere to allow the 'passional nature' to influence our judgments is to prove recreant to our sacred obligation to follow the light of reason alone in the realm of exact knowledge. Lying without this region, however, are spheres in which the will may be consciously operative in the formation of our judgments, without sacrificing the integrity of our nature as rational animals. I would indicate three of these spheres :

1. Where complete evidence is lacking, and yet some action, which in itself is a decision, is imperative. A judge may withhold his decision for fuller evidence, but not so the actor in the struggle of life. He must often make up his mind from an im-



PLICIT apprehension of the situation viewed as a whole, and which resists all attempts to analyze it further.

2. Where our belief as to the result of our activity is itself a factor in producing that result. This is the sphere in which "hope creates out of its own wreck the thing it contemplates."

3. Where an initial interest in a proposition, or an investigation, is of such a nature as not to prejudge the result, but to stimulate the attention in such a manner that all possible evidence is duly considered. A will to attend is thus related intimately to the resulting belief.

By WILLIAM CALDWELL.

I. The relation of will to belief can be discussed only under the presuppositions of (*a*) the newer psychology of volition, (*b*) the philosophy of volition inaugurated by Schopenhauer, (*c*) the logical doctrine of different 'universes of reality,' in regard to which the expert or 'believer' in question is the first court of appeal, (*d*) the fact that into the formation of belief elements at first non-intellectual undoubtedly enter, (*e*) the fact that theologians as well as psychologists are expounding beliefs from the point of view of *dynamogenesis*.

II. Both will and belief have *retrospective* and *prospective* aspects. (*a*) Retrospectively considered, a man's *will* represents the sum of tendencies to act, that his experience has led him to regard as in conformity with the tendency of things; while a man's *belief* is his active sense of the *realities* with which his experience has brought him into contact. (*b*) Prospectively considered, a man's will or tendency-to-act (like an 'apperceptive system') is always slightly in advance of the matter of his present or formulated knowledge. And as to *belief*, a man has the power of testing by conscious experience the action-value or organization-value (*i. e.*, the value so far as the systematizing and developing of his own nature and tendencies are concerned) of the highest religious or moral practices and ideals of his time. Only, a man's adoption of this 'social' or 'organization' standpoint is far more matter of unconscious and inevitable volition than of conscious and arbitrary volition. We are practically necessitated (and not merely 'free') to believe in that which furthers our development.

III. We are still too close to Cartesianism and Hegelianism and 'faculty-psychology' and 'presentationism' and to external views of the realities of belief, to be able to accept the doctrine that, in the individual and in the race, volition comes first and knowledge and belief afterwards, without feeling that some kind of injustice is done to knowledge. We really *believe* not in things 'beyond' knowledge, but *only* in that which we know—only in those things which we know to constitute the reality and the conditions of our experience.

By A. C. ARMSTRONG, JR.

The historical consideration of this question demands a broad interpretation of it. It concerns belief (*a*) as affected not merely by 'will' in the technical sense but by the whole 'passional' or 'non-intellectual nature'; (*b*) as meaning assent to propositions not demonstrably established. One root of the 'faith-philosophy' is found by Miller (*International Journal of Ethics*, Jan., 1899, p. 169) in the egoistic and adventurous spirit of the Revolution and the romantic movement. A second, more widely spread and more important, source is the tendency shown in periods of *Aufklärung* to appeal from the head to the heart in support of the imperiled foundations of the ideal life. Hence the positions of Pascal and Bayle in the seventeenth century; of Rousseau, Kant, Schleiermacher and others at the end of the eighteenth and the beginning of the nineteenth; of Balfour, Romanes, James, of the neo-Kantian and Ritschlian philosophers of religion in the present age. But there is a counter-argument, also historical and recurrent, in behalf of pure reason: that the faith-philosophy is obscurantism and unreason, even that it is dishonest and unworthy (cf. Miller, *op. cit.*, pp. 172, 173). If the defenders of the 'will to believe' cite history, therefore, it is competent to their opponents to demand consideration for the historical elements in their own contention. The solution appears to be: (*a*) the faith-philosophy, moderately stated, occupies a defensible position; (*b*) nevertheless, the criticism of the 'rationalists' shows the point where it is most open to attack and where further development must begin. This is the slackness in determining the grounds and, especially, the criteria of

belief. The task has been attempted, indeed, *e. g.*, by Kant (the faith of *reason*); by James (the mingled psychology, noëtics and ethics of 'genuine options' etc.); by those who base belief on judgments of ideal worth. But the bearing of the history is not to urge any one or any combination of these as correct, but to evidence the necessity of some such development of the doctrine in question.

*Development of Voluntary Motion.* By E. A. KIRKPATRICK.  
(To appear in full in THE PSYCHOLOGICAL REVIEW.)

The case of a child of seventeen months that had never tried to stand or walk alone, who, upon seeing some cuffs on a table, crawled to it, pulled herself up, put on the cuffs, then walked and ran all over the house, was reported. The claim was made that movements, such as walking, that seem to be learned, are in reality largely inherited, and that other nervous and muscular connections are less a matter of experience than is usually thought. It was shown that Professor Baldwin's principle of reproduction of favorable stimuli by an organism must depend upon certain physiological connections, and that chance is a less important factor in the selection of movements for repetition than Baldwin has indicated. It was claimed that there is a physiological space relation between different tactual and visual stimulations and the movements they call forth. In learning movements the attention of the child is concentrated upon the stimulus and the end to be gained, and there is little or no consciousness of the movements themselves. Therefore, to analyze any manual task to be learned into its elementary movements and requiring each to be learned separately, then combined with others, and finally all used for an end, is contrary to the natural order, and a partial undoing of inherited connections that should simply be completed and perfected.

*Report on the Effects of Cannabis Indica.* By E. B. DELA-  
BARRE.

The effects of *Cannabis Indica* as determined by eleven tests on myself, in doses of 0.5 to 1.5 grains of the solid extract, may all be attributed to an induced hyperexcitability of the nervous



system—sensory, associational and motor. The particular effects are very variable, depending on a large number of factors. Many further tests are necessary. In general, a gradual increase in sensory, intellectual, emotional and motor activity occurs, lasting about half the total duration of the main influence; followed by a gradual decrease to normal or below. The increase is not continuous, but intermittent or rhythmical. The duration is from five to nine hours or more, though the influence is measurable for several days. No depressive reaction has occurred.

In the first half there is a tendency to hyperæsthesia, to increase in delicacy of discrimination, in rapidity of association and intellectual work, in richness of imagery and thought, in rate of pulse and breathing, with diminished depth of both; to decrease in muscular strength and steadiness, in secretions, in expansive but not in contractive reactions. In the second half, in case no fatigue occurs, there is a gradual intermittent return toward normal; if fatigue, a reversal beyond normal.

No noteworthy increase in illusions of suggestibility has occurred. Introspection has been trustworthy and valuable, largely increased in power. The state appears to be an exaggeration of normal states, tendencies and rhythms. Hence its enormous value in analysis. Besides the careful attention to gaining exact experimental results, which covered a much wider field than can be indicated here, interesting analyses were made of emotions, of motor influences in emotion, in discrimination, in geometrical illusions, of attention, association and expression, and of philosophical concepts.

In larger doses, or on other persons, the results might in some respects be different from those thus far obtained.

*The Psychological Imagination.* By DICKINSON S. MILLER.  
(Read by title.)

*Certain Hindrances to the Progress of Psychology in America.*  
By GEORGE TRUMBULL LADD. (Printed in full in THE PSYCHOLOGICAL REVIEW, March, 1899.)

Starting from the assumed truth that the progress of any positive science depends largely upon the quality of the men

who chiefly cultivate it, the opinion was ventured that psychology is not at present making in this country the progress which may be reasonably expected. It would seem, then, that the hindrances are partly, at least, matters of *personnel* rather than of *matériel*.

If we inquire more carefully into the particular hindrances of this order, the following seem to be among the more prominent: First, a certain aloofness of psychologists from, and a consequent ignorance of, the mental life and mental development of the common people. Without depreciating the value of any of the forms of specialization in laboratory or other allied work, the nature of psychology is such as to make a wide acquaintance with average human nature desirable, if not indispensable. In contrast with this hindrance, due to an excess of the scholastic spirit and method, is, second, the excessive amount of popular publications written by authors of insufficient scientific training. Connected with this is, third, the injury done to the science of psychology, in the estimate of the intelligent laity, by mannerisms of discussion and of the expression of tenets and discoveries, such as appear unfit for any body of men that are penetrated with the genuine spirit of science. The confession seems forced upon us that there is too little of reserve and dignity in controversy among psychologists as a class, and too much concession to popular demands that tend to lower our scientific standard.

But, in the fourth place, a certain invasion of the wide-spreading 'commercial spirit' seems likely to work harm to the science of psychology. The fear is not wholly unfounded, that this will cause an increase of personal and institutional rivalries and jealousies, of premature publication, of a somewhat disingenuous way of seeking for personal reputation rather than for the progress of science and for the welfare of mankind. On the other hand, fifth, psychologists do not, on the whole, maintain a sufficiently independent, yet teachable and friendly attitude to the other most closely allied positive sciences. An increase of a courageous but modest self-respect, and a determination to merit the respect of workmen in allied sciences, will undoubtedly do much to remove this hindrance.

The intention of this paper being only to speak of hindrances in so far as they belong to the *personnel* of psychology, reference to favoring conditions and to encouragements is, of course, omitted.

*Reason a Mode of Instinct.* By HENRY RUTGERS MARSHALL.

Instinct is commonly opposed to Reason.

The objective mark of an Instinct is that it determines in an organism typical reactions of biological significance to the organism. Subjectively we have 'Instinct feelings' when the reactions take place; when they are inhibited we have 'impulses.' The physical and psychical aspects of Instinct are as wide as life. Turning to the opposition to Instinct we find its objective mark in variation from typical reactions; this is indicated by hesitancy and then choice. Subjectively choice is represented by Will, and in our complex life the antecedent to choice is reasoning. As variation is, so choice and will are conceded to be, as wide as life; but so also, if analogy is any guide, must be the physical process antecedent to choice, and Reason the psychic coincident of this antecedent process.

Variation and reasoning both appear as reactions of a part of a complex physical and psychical system, as though it were an isolated entity out of relation to the whole system to which it properly belongs.

Variation is thus statable in terms of Instinct; and hence Reason itself must be looked upon as a mode of Instinct, the observed opposition between the two being due to the fact that Reason and Variation as we experience them are phenomena appearing in connection with psychical and physical activities of very complex organisms formed of complex systems integrated with still more complex systems.

Reason is thus referred back to Instinct. But Instinct, in its turn, is referable to the simplest of all phenomena of life—the reaction of a living cell to a stimulus. To this simplest of all reactions we therefore finally trace back both Reason and Instinct. The problems connected with the difference between Reason and Instinct are thus resolved into those connected with the determination of the relations between parts of systems—



of the nature of what we call the integration of psychical and physical systems.

*Animal Intelligence and the Methods of Investigating it.* By WESLEY MILLS. (To appear in full in the PSYCHOLOGICAL REVIEW.)

Comparative Psychology is advanced rather by systematic observations and experiments than by anecdotes, nevertheless the latter when strictly true are not valueless.

The study of the development of the animal mind (genetic psychology) is of the highest importance.

Insufficient attention has been paid to distinguishing between normal, subnormal and supernormal comparative psychology, an objection, however, which applies with a certain degree of force to human psychology. In making experiments on animals it is especially important that they be placed under conditions as natural as possible.

The neglect of this is a fatal objection to the work of the author of 'Animal Intelligence,' published as a monograph supplement to the PSYCH. REVIEW, Vol. II., No. 8, June, 1898.

The portion of this research referring to chicks is the most reliable, and the suggestions as to pedagogics, etc., valuable.

This investigator's experiments show that certain associations may be formed under highly unnatural conditions, which associations, etc., however, bear about the same relations to the normal psychic evolution of animals that the behavior of more or less panic-stricken or otherwise abnormal human beings does to their natural conduct.

It is not proven, as asserted in the publication in question, that animals do not imitate, remember, have social consciousness, imagination, association and perception; nor that their consciousness is only comparable to that of a human being during swimming or when playing outdoor games as understood by this writer. It is highly probable that animals, even the highest below man, have only rarely and at the best but a feeble self-consciousness, if it exists at all.

But on this point and on the question of inference, reasoning, etc., the time is not yet ripe for positive assertions.

It seems more than probable that the mental processes of the highest animals are not radically different from those of man so far as they go, but that the human mind has capabilities in the realms both of feeling and intellection to which animals cannot attain. While it is desirable to push analysis as far as possible, it is safer to remain in the region of the indefinite, and to refrain from making very precise and positive statements as to whether the animal mind does or does not possess certain powers, till we are in possession of a larger storehouse of facts, especially of the nature of exact and systematic observations (or experiments). *Festinate lente* is a good rule in regard to drawing conclusions in Comparative Psychology.

*Psychological Classification.* By MARY WHITON CALKINS.  
(To appear in full in THE PSYCHOLOGICAL REVIEW.)

The traditional theory that sensations have the 'attributes' of quality, intensity, extent and duration is unjustifiable, if the conception of sensations as unanalyzable and irreducible elements of consciousness is at the same time upheld, since the possession of attributes is synonymous with complexity. The admission of attributes is only possible on the theory that the sensation is not abstractly unanalyzable, but that it is rather the simplest possible concrete experience. But this hypothesis ignores the fact that percepts, images, emotions or volitions—never sensations, even in this looser conception of them—are the simplest elements of actual experience. Only as an abstract and hypothesized and unanalyzable element has the term 'sensation' any valid meaning in psychology; and on this definition there is no longer room for attributes of this irreducible datum, which, rather, is itself an attribute.

Scrutinized, each for each, the so-called attributes are readily classified on other principles. Duration distinguishes itself from all the rest in that it is attribute of physical as well as psychological phenomena, and, therefore, not attribute at all in a psychological sense—not an elementary content of the fact of consciousness, but a *reflection about* facts, physical and psychical.

Quality is identified with sensation-element by most writers, even by those who teach the attribute-theory. Similarly, in the

opinion of the writer, intensities can be shown to be sensation-elements, distinguishable and unanalyzable factors of consciousness; and extent, if not a sensational element, is a complex of such elements.

*Exhibition of Instruments for the Study of Movement and Fatigue.* By J. MCKEEN CATTELL.

Apparatus devised for special research on movement and fatigue in the Columbia Laboratory was shown and described. Several of the instruments have been already published, but these are now being used for experiments in new directions. The instruments were as follows: (1) Apparatus for measuring the time of discrimination and movement. In addition to the arrangements for exact determination of a single reaction, instruments and methods were shown by which a series of processes can be measured by simple means. Experiments by Mr. Germann on the formation of motor habits were mentioned. (2) An ordinary grindstone was arranged so that reaction-times can be measured without chronoscope or chronograph. (3) Instruments and methods for studying the accuracy of movement, its force, time, extent and localization were exhibited, and experiments in progress by Mr. Woodworth were described. (4) An automatograph giving a continuous curve for extensor and flexor movements. (5) A spring ergometer intended to replace the Mosso ergograph. (6) A dynamometer in which the pressures are continually added and counted, making the study of muscular fatigue and the effect of mental conditions on fatigue possible without elaborate apparatus. All the instruments were shown in working order, and attention was called to the use in many cases of simple means in place of more complicated apparatus, and to the improvement of the latter in several ways, such as the avoidance of batteries, mercury for contacts, and smoked paper.

*The Physiological Basis of Mental Life.* By HUGO MÜNSTERBERG.

The psychologist must demand that the physiological theory of the brain processes shows a manifoldness of factors which cor-



responds to the manifoldness of the psychical elements. The theories of to-day are not satisfactory in that respect. Every psychophysiological excitement is considered as variable with regard to locality and amount of the centripetal stimulation. On the psychical side the quality of the sensation corresponds to the local variation of the sensory end station and the intensity of the sensation to its quantity of excitement. But this is an abstract scheme which makes the central process simpler than it is, as it does not take account of the fact that every central sensory process is at the same time the starting point of a centrifugal process which depends upon the disposition of the whole centrifugal system. This central discharge varies, of course, also in quantity and locality, depending upon the openness of the channels. If we consider the sensation as the accompaniment of the physiological process which transforms the centripetal stimulation into a centrifugal discharge, we have then a fourfold multiplicity of the central process. This allows us to account for two more factors of the sensation which cannot be reduced to differences of quality and intensity: the different degrees of vividness, down to the unconscious states, and the different subjective values, as, for instance, the time values, the feeling tones, the belief tone and many others. The vividness must be considered as dependent upon the quantitative amount of the discharge and the values dependent upon the local character of the discharging paths. Every sensation is thus by principle an innervation feeling and its physiological basis is equally dependent upon the processes in the sensory system and in the central motor apparatus.

*On the Confusion of Tastes and Odors.* By G. T. W. PATRICK.

This was a preliminary report upon some experiments upon taste and smell made at the Iowa Laboratory. The experiments were made upon a subject having complete congenital anosmia and upon normal subjects acting as controls. The subject was first tested with about one hundred odorous substances, including those from all the nine classes of odors given by Zwaardemaker. None of these gave any sensation or reaction whatever. Two other classes of substances, however, gave

reactions, the first being sensations of touch and the second sensations of taste. As examples of the former, are menthol, sulphurous oxide, acetic acid, ammonia and various ammonia compounds. As examples of the latter are chloroform, ether and pyridin.

The subject's sense of taste was then tested, and the sensibility to simple tastes found to be about normal.

Then followed a series of experiments upon taste made upon the anosmic subject and simultaneously upon two normal subjects to determine so far as possible the part played by sensations of smell, touch, temperature and sight in so-called taste sensations as given in ordinary foods and drinks. These experiments extended over about ten weeks and included one hundred and eighty-five such substances, sensations of sight and temperature being eliminated as far as possible. About half a teaspoonful of each substance was given to each subject, who was allowed to smell it and taste it as much as she wished and finally to swallow it. The substances were divided, according to the results, into three classes. The first were those recognized both by the anosmic and the normal subjects. These would be presumably the foods and drinks recognized by the sense of taste alone, but an examination of the list which included the various spices, different kinds of syrups and molasses, cherry juice, etc., offered some grounds for the conclusion that, with the exception of typical simple tastes, such as sugar, tartaric acid, quinine, etc., the recognition depended in every case upon the senses of touch and temperature. The second class included those substances recognized by the blindfolded normal subjects but not by the anosmic. Presumably they would depend upon their odor for their recognition. They were as follows: coffee, tea, normal alcohol, port wine, claret, vinegar, spirits of almond, tincture of rhubarb, vanilla extract, absolute alcohol, tincture of ginger, chocolate, cocoa, milk, milk and water, sour milk, nearly all the common fruits, boiled turnip, raw and boiled onion, yoke of boiled egg, white of raw egg, oil of rose, and kerosene. A third list of substances included those recognized by one of the normal subjects but not by the other nor by the anosmic. A fourth list included the substances

recognized by none of the blindfolded subjects. Among other conclusions, the following was drawn: what commonly passes for taste sensations, so far as their discriminative or intellectual value is concerned, is the composite result of the mingling of sensations of smell, touch, temperature, sight and taste, the latter, however, playing little or no part in the discrimination of our common foods and drinks. Taste sensations proper furnish rather the emotional element in the total conscious effect. Sweet things we call 'good,' and bitter things we call 'bad,' while salt and sour, if, indeed, they are simple taste sensations, add a certain piquancy which is pleasing when they are not excessive.

*Methods of Demonstrating the Physiology and Psychology of Color.* By E. W. SCRIPTURE.

The most complete and effective method of teaching color is by means of the tricolor lantern. This is a special kind of triple lantern which I now show you. The idea of color projection in this way originated, I believe, with Du Hauron; this special lantern is the invention of R. D. Gray. It is arranged for lime-light, as the color work cannot be done with electric or acetylene light. The three jets are packed closely into one lantern-body. The three condensers are as close together as possible. Three lenses exactly alike are mounted on the front board. The jets have all adjustments for regulating the gas, manipulating the lime, etc. Limes turned in the lathe are used in order not to disturb the focus as they are rotated in the lantern. Regulators are placed on the cylinders.

Three colored films, red, green and blue, are placed in the triple lantern. I now show you a slide which gives on the screen the elementary colors singly with their combinations in pairs and in triple. Shades are shown by slowly turning the light down. The various hues and the laws of combination are illustrated by varying the intensities of the jets. The properties of the color triangle and the color pyramid are thus illustrated. Hues, tints, shades and complementaries are readily explained. When the laws of color have been thoroughly impressed by this method, slides of concrete objects are used for study. Thus,



a group of flowers affords an illustration of the automatic solution of color equations.

The phenomena of color blindness can also be represented with the tricolor lantern. The usual theory of color blindness, according to which the defect arose by the failure of one of the three fundamental colors, can be illustrated by covering up one of the lenses. For red blindness the red lens is covered, and the resulting picture appears in combinations of green and blue; for green blindness the green lens is covered, and for the hypothetical blue blindness the blue one is covered. To illustrate the newer theory, the blue slide is left unchanged, but two slides are made for red and two for green. For the dichromats of the first class—the red-blue persons—the two slides taken through the red ray filter are placed in the red and green lanterns.

The method also furnishes a remarkable analogy to the decomposition of the colors by the eye into three fundamentals and their mental recomposition into sensations of color. The tricolor views are taken by a camera used three times in succession with a differently colored screen each time. The red rays impress one of the plates, the green rays the second and the blue rays the third. The three negatives differ in their shading. Three positives are made which differ likewise. The three positives produce views appropriately shaded when projected on the screen by the colored lights. The result is a recomposition in natural colors. The approximation to the original colors is close if the slides are properly made and manipulated.

Finally I will call your attention to the latest development of lantern projection in color. Only one lantern is used. Several methods have been tried; this one—which is not original with me—seems to be the only thoroughly successful one. It is here shown publicly for the first time. Three views are taken of the original object in the usual way through color screens. The three negatives are then used to produce three positives at the same place between two glass plates. These three positives are separately colored in red, yellow and blue dyes. The light transmitted through the slide then shows the original colors of the object photographed. As these views can be used with an electric lantern, the most brilliant and beautiful effects can be produced.

*Rates of Breathing and Degree of Mental Activity.* By J. E. LOUGH.

The paper is a report of an experiment performed in the Psychological Laboratory of Wellesley College upon thirty women, students and teachers in the college. Visual stimuli, consisting of water-colors, printed pages to be read silently, and series of indicated simple mathematical operations to be carried out silently, were given to the subjects, always under exactly similar conditions, each stimulation lasting about forty seconds. Records of the rate of breathing were taken during stimulation and for the forty seconds preceding and for the forty seconds following stimulation. The average rate for the forty seconds preceding a given stimulation is taken as the basis of comparison for that experiment, and the average rate during the stimulation and following the stimulation is always reduced to a ratio of this standard. By this method only changes of rate are shown when such a change takes place within one of given periods, thus eliminating all changes not produced by the one variation of the subject—that of the presence or removal of the stimulus. And since only the relative changes are shown, it is possible to make direct comparisons of the effect of stimulation without regard to the absolute rate of breathing.

The experiments show a rather wide range in the effect of a given stimulus. But the average of the effect the stimulus has had upon all subjects eliminates the individual differences and shares its general influence. There is in every case an increase in the rate during stimulation and a return to the standard afterwards. But the amount of this increase, produced by a given stimulus, corresponds in a general way to the degree of mental activity produced.

*Recent Investigations at the Harvard Laboratory.* By ROBERT MACDOUGALL.

*Recent Investigations at the Yale Laboratory.* By E. W. SCRIPTURE.

(a) *Investigations in the Psychology of Speech.* Gramophone plate records of prose, poetry or music are obtained in

such a way that time, stress, quality, etc. of the elements of speech can be accurately measured. Results were reported in brief.

(b) *Cross Education.* Experiments showing that training of a digit on one side of body is followed by increase in ability of all the others, even though not practiced. Hypothesis is, physiologically speaking, that the development of the center governing a particular member causes at the same time the development of higher centers connected with groups of members. Psychologically speaking, development of will power in connection with any activity is accompanied by a development of will power as a whole.

(c) *Investigations with Currents of High Frequency.* Sinusoidal alternating currents of varying frequencies were produced by a Kennelley generator. The generator was run by a motor, which was started at slow speed. The speed was gradually increased, alternation becoming more frequent. Electrodes were applied to the finger. The sensations were as follows: at low frequency there was no sensation; as the frequency was increased the threshold of sensation was reached. At a higher frequency the threshold of disagreeableness appeared. At a still higher frequency pain appeared. At a still higher frequency the pain ceased and an agreeable numbness was perceived; at a still higher frequency there was a faint sensation only. I was not able to run the generator high enough to cause sensation to disappear totally, but it would undoubtedly have done so, as we can infer from Tesla's experiments.

In the course of these experiments observation was made that rapidly alternating currents could be used to produce anæsthesia and analgesia to touch and cold (though apparently not to heat). We are now developing an apparatus to apply this discovery practically. Our latest attempt—not yet completed—consists in running a light arm with a contact around a rim containing 1,000 saw cuts filled with hard rubber. The arm revolves about 100 times per second, giving 100,000 interruptions per second. The results will be announced shortly. The importance of such a convenient method of producing anæsthesia without any of the dangers or inconveniences of ether, chloro-



form, nitrous oxide gas or cocaine, makes us hope that the method may soon be made practical.

*Recent Investigations at the Illinois Laboratory.* By J. P. HYLAN.

I. *The Division of Attention.* The object of the following experiments was to throw some light upon the problem as to whether the attention can, as is generally believed, be divided.

A screen placed before a revolving kymograph drum has in it a small opening so placed that a series of lines passes immediately behind it. Without knowing the number of lines, the subject is directed to fixate his gaze upon a fixation point half a centimeter from the opening, and count the lines as rapidly as possible, the drum first rotating rapidly and then being gradually slowed down until the correct number can be given with a fair degree of certainty. The time needed for counting a single line was calculated. Besides the series with a single screen opening, series with two, with three, and with four openings respectively were tried, each opening having its series of lines, and the fixation mark being used as at first. The method of the experiment was also the same, except that with more than one opening the subject was directed to divide his attention if possible and count all the lines that appeared in the several openings.

The problem was also approached by means of auditory sensations. Single, double, triple and quadruple series of musical clicks were used, and the time for counting a single click in the different series was obtained, as in the last experiment. In other than the single series the clicks came in succession and each was of a distinctly different pitch.

Results were given which argue, in the main, against the division of attention.

II. *Effect of Amount of Motor Impulse on Motor Memory.* In this experiment Münsterberg's muscle apparatus was used, with one of the pans weighted with 200 grs., 500 grs., 1,000 grs. and 1,500 grs., in order to make the carriage move with varying degrees of difficulty. Each series with a weight was followed by a series without a weight to act as control. When

each subject had gone through each series, the experiments were repeated with the carriage pushed instead of pulled as at first, to vary the effect of the joint sensations, and also repeated with an interval of 10 and of 30 seconds between the first and the repeated movement.

The results show that the repeated movements were distinctly more accurate in pushing than in pulling, and that this difference was greater with the weights than without them. The weights seem to have been a disturbing factor, but much less so for pushing than for pulling. In pushing, the tendency was constantly to underestimate the distance, but to do so by a fairly constant amount.

*Recognition under Objective Reversal.* By GEORGE V. DEARBORN. (To appear in full in THE PSYCHOLOGICAL REVIEW.)

This research, conducted in the Harvard Psychological Laboratory during the first five months of 1898, is a study of the relative recognizability of objects turned or reversed in each of the four quadrants and in the mirror-reversal and always in a plane at right angles to the line of sight. The essential apparatus employed was a set of 368 chance blots of ink made on white cards 4 cm. square, and arranged in series of ten with three reversals in each series. These objects were exhibited to each of the nine subjects, and their judgments as to recognition recorded by means of precise electrical mechanism and a kymograph. It was found that the repeated characters when one-quarter reversed over toward the left were recognized 61.4% as often as were those unturned; inverted, 72.8%; three-quarter reversed, 47.1%; erect mirror-reversed, 65.7%; and inverted mirror-reversed, 45.7%. In other words, it appears that an object is recognized more readily when inverted than when in either of the two intermediate positions, and more readily also than in the erect mirror-reversal or in that position inverted.

It is suggested that these empirical results may be in part explained respectively by the law of habit; the optical conditions of vision; the fact that we habitually perceive the upper left-hand corner of a flat object first; and by our familiarity with mirrors, natural and artificial.

*Further Measurements of Pain.* By ARTHUR MACDONALD.

Tables of measurements made by the writer were distributed, and the conclusions reached were as follows :

1. In general the sensibility to pain decreases as age increases. The left temple is more sensitive than the right. This accords with former experiments that the left hand is more sensitive to pain than the right hand. There is an increase of obtuseness to pain from ages 10 to 11 ; then a decrease from 11 to 12 ; then an increase from 12 to 13. From 13 to 17, while the right temple increases in obtuseness, the left temple increases in acuteness. This is in the post-pubertal period. There is a general variation, which experiments on larger numbers might modify.

2. Girls in private schools, who are generally of wealthy parents, are much more sensitive to pain than girls in the public schools. It would appear that refinements and luxuries tend to increase sensitiveness to pain. The hardihood which the great majority must experience seems advantageous. This also accords with our previous measurements that the non-laboring classes are more sensitive to pain than the laboring classes.

3. University women are more sensitive than washerwomen, but less sensitive than business women. There seems to be no necessary relation between intellectual development and pain sensitiveness. Obtuseness to pain seems to be due more to hardihood in early life.

4. Self-educated women, who are not trained in universities, are more sensitive than business women. Giving, then, the divisions in the order of their acuteness to the sense of pain, they would stand as follows : 1st, girls of the wealthy classes ; 2d, self-educated women ; 3d, business women ; 4th, university women ; 5th, washerwomen. The greater sensitiveness of self-educated women as compared with university women may be due to the overtaxing of the nervous system of the former in their unequal struggle after knowledge.

5. The girls in the public schools are more sensitive at all ages than the boys. This agrees with the results of our previous measurements that women are more sensitive to pain than men.

These measurements of least disagreeableness, or of thresh-



old of pain, are approximate measurements of the combination of nerve, feeling and idea. The temple algometer designed by the writer was also described and exhibited.

*Theory of the Will in Aristotle's Ethics.* By WM. A. HAMMOND.

The two component elements in the ethical will are the Practical Reason (*νοῦς πρακτικός*, *De an.* 433<sup>a</sup>14, 16; 404<sup>b</sup>5; *Eth. Nic.* 1142<sup>a</sup> 23-30; *διάνοια πρακτική*, *De an.* 433<sup>a</sup> 18) and Desire (*ἐπιθυμία*, *De an.* 432<sup>b</sup>6 seq.; 433<sup>a</sup> 2; *ὄρεξις*, 433<sup>a</sup> 15 seqq.). Desire, as Aristotle employs it, is not a purely pathic element or a mere feeling of pleasure or pain. He describes it as an effort after what is pleasant; *i. e.*, he includes in it an activity element (433<sup>a</sup>24 seqq.). It is feeling with an added quality of impulse or *Trieb*. It involves further an idea or presentative element. The details of Aristotle's analysis of desire are, therefore, (1) Idea. There can be no desire without an image (*ὄρεκτικὸν δὲ οὐκ ἄνευ φαντασίας*, 433<sup>b</sup>28). (2) Feeling. In every desire the element of pleasure or pain constitutes the stimulating force to action (433<sup>a</sup> 21; 1111<sup>b</sup>17). (3) Effort or activity, the actual impulse to pursue or avoid (433<sup>a</sup>9, 13; 1139<sup>a</sup>22). Between the practical and theoretical reason Aristotle draws a distinction. The theoretical or speculative reason deals with necessary truths (432<sup>a</sup>24 seqq.; 1139<sup>a</sup>10 seqq.), the practical reason operates in the sphere of choice and of the variable; the theoretical reason does not command, is concerned with the true and false; practical reason is imperative, is concerned with good and bad, judges, weighs, determines; its sphere of activity is the sphere of conduct. The characteristic virtue of the practical reason is prudence (*φρόνησις*). While prudence describes the moral quality of the practical reason, the method of its operation is described by the practical syllogism (434<sup>a</sup>16-20; 1147<sup>a</sup>1-7; 1144<sup>a</sup>31 seqq.). The Practical Reason contains a jussive or epitactic force; the desire contains an active quality of impulse. Aristotle defines the moral will, therefore, as reason stimulated by desire or desire penetrated by reason (1139<sup>b</sup>4). The moral will, as this complex of reason and desire, functions under the modes of (a) deliberate choice and (b) freedom, and issues by means of par-

ticular acts in (*c*) fixed habit or the persistent character. As Aristotle regards the whole of psychical life as impulse or activity tending towards the realization of a potentiality, one may find in this doctrine of *ἐνέργεια* the correspondent of the non-moral or metaphysical will of the moderns. The will of Ethics, however, the *voluntas intellectivus* of Aquinas, is conceived of as feeling acting under the forms imposed by reason.

*Psychology and Ethical Scepticism.* By W. G. EVERETT.

Is a science of ethics possible? Ethical scepticism has denied its possibility. Each individual, by virtue of a peculiar nature, is a law unto himself. Moral laws are the rough compromise which convention effects to render some sort of social life possible. They have no natural or rational sanctions. To escape these difficulties appeal has been made to religion with its supernatural sanctions. But this is fatal to a science of ethics and is practically unsatisfactory, as large numbers are untouched by supernatural sanctions. Like difficulties confront a metaphysical ethic. A metaphysic of ethics is valid and necessary, but as a complement, not a substitute, for a science of ethics. The existence of natural and rational sanctions is presupposed by such a science. Many profoundly believe that there are adequate sanctions in human experience. Can they be reduced to scientific form? If so, we must have an adequate psychology of moral experience. Ethics requires not only a social psychology but also a psychology of the subtler phases of individual moral experience. What then is the relation of the ethical elements of consciousness to the total conscious life of the individual? What part do these elements play in mental development? How do they stand related to mental deficiencies and excellencies? What are the results upon the affective states of such vices as envy, jealousy and self-seeking? In states of *ennui*, despair and pessimism are there elements which result from ethical deficiencies? Is it true psychologically that lust and greed are the sure seeds of uneasiness and dissatisfaction? To answer these and similar questions satisfactorily would be to ground moral law and its sanctions in man's own nature, and at the same time securely to establish the science of ethics.

*Professor Baldwin's "Social and Ethical Interpretations in Mental Development."* By W. CALDWELL.

This paper was an attempt at a positive and appreciative criticism of Professor Baldwin's second volume on Mental Development, the *Social and Ethical Interpretations*. Mr. Baldwin's book achieves the object of its endeavor—the exhibition of social organization and personal (mental and moral) organization under the same psychological principles. Its classical recognition of epistemological principles is one of its merits, and it confines itself, on the whole, to the genetic point of view. The apparently ever-recurring circular process from the 'self' to 'society' and from society to the self and then back again, instead of being, as some critics maintain, a drawback or defect of the book, must be studied as part of its central lesson and main contention. Mr. Baldwin does not exactly assume, without any explanation, the self and society; he assumes the fact of mental development, and then, when studying its beginnings and its different phases, finds that it involves the conception and the reality of the self as a *socius*—as one term or another in a related thought or action content. The concluding sections of the volume, about the final and irreducible conflict between the moral man and society, are proof positive that its author believes in the reality of the human personality as something more than a mere phase of a 'social situation.' Nor is Mr. Baldwin's use of the genetic point of view a mere arbitrary procedure; he justifies his use of that method by letting us see that consciousness and conscious process cannot be understood apart from it.

The positive value of the work lies in the fullness of detail with which the relations of the thought-process to the movement-process are worked out. Its teaching about *imitation* as the social method *par excellence* must be taken along with what is taught in Mr. Baldwin's first volume upon that process. And then, lastly, the genetic point of view fully justifies the contention that society is a psychological organization. The book has a high general value at the present time, tracing, as it does, many important scientific and philosophic tendencies to their psychological roots.



*The Genetic Determination of the Self.* By J. MARK BALDWIN. (Read by title.)

Consists of sections added in the second edition of the author's 'Social and Ethical Interpretations,' in which some of the applications of the theory of the 'Dialectic of Personal Growth' are brought together more explicitly in view of criticism. It is to appear in full, under the title 'The Social and the Extra-Social,' in the *Amer. Journal of Sociology*, March, 1899.

*Art in the Light of Modern Psychology.* By G. TOSTI. (Read by title.)

*A Study of Geometrical Illusions.* By CHAS. H. JUDD. (To appear in full in THE PSYCHOLOGICAL REVIEW, May, 1899.)

The main thesis of the paper is that the underestimation of acute angles and overestimation of obtuse angles, which is a common feature of many illusions, is not a fundamental fact, but is to be explained as due to the false estimation of the length of the sides of the angles.

Overestimation and underestimation of linear distances are always accompanied by false judgments in the opposite direction in the surrounding field of vision. It is possible to find illusions in the surrounding field even when the figure suffers no internal illusion. These facts lead to the conclusion that such illusions are in general due to the shifting of points in their spatial relations.

The simplest form of the angle illusions, the Poggendorff illusion, is due to underestimation of the distance between the interrupted ends of the oblique lines, not to false estimation of the angles. This is supported negatively by comparison of the angles under a variety of conditions, under some of which the illusion appears, and under others of which it does not. Positive evidence is produced in quantitative determinations of the error in the estimation of the distance in question.

As in the case of the Poggendorff illusion, so in the estimation of all angles, whether subject to illusion or not, the estimated length of the sides is a most important factor. When the side is overestimated the angle is underestimated and when the side is underestimated the angle is overestimated.

*Subjective Colors and the After-Image.* By MARGARET FLOY WASHBURN.

This paper describes experiments designed to show the effect on the series of colors produced in the 'ringing off' of an after-image, by efforts to call up subjective color sensations of red, green and blue. It was found that the color changes of the image were very materially influenced by this process; traces of a given color in the image being intensified by central excitation until the entire image was tinged with the visualized color. Since the process thus investigated does not differ essentially from the process of voluntary attention to a given color, the experiments furnish evidence that attention has a positive, intensifying function, and they suggest that the increase of intensity on the part of the conscious state attended to comes not from a single 'attention center' but from associated centers of the same order as that which gives rise to the conscious state in question.

*Three New Cases of Total Color-Blindness.* By CHRISTINE LADD FRANKLIN. (Read by title.)

*A New Color Illusion.* By GEORGE TRUMBULL LADD.

A diagram on p. 50 of Fick's *Lehrbuch der Augenheilkunde*, used by the author to illustrate 'red-green blindness,' became the point of starting for an investigation not yet completed. The illusion, consisting of the complete disappearance of the red-colored letter *E* and the substitution of the color of the green background, was first noticed by Dr. George T. Stevens, of New York City.

Investigation has shown that strips of red, blue and orange, on a great variety of backgrounds, give the same illusion, although the orange seems to have a different way of disappearing from that followed by the other two colors. A marked difference was discovered between two classes of backgrounds. Green of two different shades, dark violet, dark blue, and black gave the illusion readily. But yellow, orange, gray, white, and a lighter blue or green, and a reddish violet gave the illusion with greatly increased difficulty or not at all. These colors

for the strips and backgrounds seem to retain their peculiarities even when combined on the same general field of vision.

Explanations of this illusion are not yet clearly made out. It may be partly due to the production of a temporary blind-spot. It seems to be connected, also, with the rhythm of attention in fixation. And there is some evidence that the substituted color is of a complicated cerebral origin. Further investigation will follow.

#### REPORT OF THE COMMITTEE ON PHYSICAL AND MENTAL TESTS.

Professor Cattell, the Chairman, stated that, owing to the absence abroad during a considerable portion of the year of four of the five members of the committee, no regular meeting had been held. In any case, the committee needed trials and reports of the tests already recommended before further joint action could be taken to advantage. Individually the members of the committee had continued its work. In this regard each must speak for himself; at Columbia the tests had been regularly made and extended to include students of Barnard College, several new tests had been devised and tried, and a gymnasium examination had been arranged that added to the value of the tests made in the Psychological Laboratory. The sum of \$100 appropriated at the Ithaca meeting had been in part distributed as follows: \$25 to Professor Jastrow toward the cost of his card-sorting apparatus in order that it might be sold at a lower rate; \$25 to Professor Cattell for record blanks, the blanks to be distributed, at the cost of press-work, to members of the Association wishing to use them; and \$25 to Professor Warren for an investigation of individual differences in memory and imagery.

#### *Tests for Sense-Type.* By HOWARD C. WARREN.

The following set of tests is proposed as a means of studying the comparative value of the several senses in the life of the individual. The object of the series as a whole is to determine the respective rôle of each sense in perception, association, memory, imagination, etc. The different tests take these functions up separately.



I. PERCEPTION TEST.—A simple passage of 400 words, containing 40 misprints, some of which appeal to the eye and others to the ear. The subject is asked to read the passage and mark the misprints. The proportion of misprints as a whole and of each kind is noted.

II. PERCEPTION-ASSOCIATION TEST.—A list of 50 words, chosen for their richness of sense-connotation, and so arranged that those in immediate succession are not likely to suggest images from the same senses. They are read to the subject, who is requested to state the kind of sense imagery suggested first, later, and most prominently, in each case.

III. MEMORY TEST.—Six series of nine figures each; to be dictated or shown to the subject, who is to repeat or write each series from memory after it has been completed.

IV. MOTOR-CO-ORDINATION TEST.—1. Writing on the forehead; the direction of the writing showing the relative importance of the visual and muscular factors in this function. 2. Five separate tests in mirror-writing (*i. e.*, looking into the mirror and writing so that the phrase shall read correctly in the mirror); the hand and paper are not seen directly—merely their reflection in the mirror; the mirror is placed at the right of the paper in two cases, and above it (*i. e.*, in front) in the rest.

V. MENTAL IMAGERY TEST.—The subject is asked to call up a vivid image of a definite sensation described to him; there are 11 tests; the time consumed in the effort is noted, together with the degree of success or failure.

VI. REACTION TEST.—Six series of ten reactions each; with natural, sensory and motor attention, and on sound and light stimuli respectively.

VII. QUESTIONNAIRE.—A set of questions to bring out the absolute and relative importance, for the subject, of various senses (including muscular) in certain respects.

These tests are expected to yield some useful data in the field of individual psychology. They should help in the final determination of a certain number of the general tests that your committee is seeking to formulate; and it is on this ground that the application is made for an appropriation from the fund allotted to that object.

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## DISCUSSIONS.

### THE MATERIAL VERSUS THE DYNAMIC PSYCHOLOGY.

It is not a little remarkable that in this era of dynamism in physical science and of various attempts at the application of dynamic concepts in psychology there have been so few really consistent presentations of the material of modern psychology from the dynamic standpoint. In this respect we recall hardly a recent book besides that of Jodl in which the old garment has not suffered from the addition of the new cloth. Titchener's text-book and, to a certain extent, that of Ebbinghaus are exasperatingly inconsequent in this regard. The net result of the experimental psychology, industriously cultivated during ten years, is admittedly so small, so far as facts are concerned, that it is a pity if it cannot at least give us a point of view.

The reason for this condition is not far to seek. It is because our psychologists have been narrow in their preparation and are consequently uninfluenced by the recent change of base on the part of molecular physics and higher mathematical concepts. It may be asserted that the student of psychology as well as of biology cannot hope to take a comprehensive view of his own domain without at least following the results of recent physical speculation.

A psychology which is so largely occupied with waves of air and pulsations of ether may be forgiven for speaking of atoms and molecules, but, inasmuch as a system of psychology is profoundly influenced by the ontological notions underlying it, one should have a care that the use of the convenient terms of conventional physics does not interfere with a logical development of the science. Still more pernicious is the effect of the 'matter idea' on current epistemology.

While it may seem incongruous for one who touches psychology from the side of neurology to present the claims of an immaterial psychology, it is hoped that it may not be without interest to psychologists by profession to receive the testimony of a worker in the so-called material substrate. Proceeding from the statement that psychology is the science of experience—of consciousness, in which all will agree, for the purpose of this discussion at least, it is apparent that the material presented to consciousness is in the form of activity. It does



not matter much that we admit with Professor Caldwell that consciousness is activity, for all we know of activity is based on its experience and on the result of inferences formed on such experience. The condition of passive receptivity is one which the neurologist cannot accept as in accord with anything we know regarding the nervous mechanism of thought. The elements of experience are all acts. Why is not psychology content to start with the actual dynamic units of experience and to use them till they are found wanting or until proof is forthcoming of the existence of material units in place of them? But, it is replied, it is one of the necessary laws of thought that forces must reside in some substance, that forces, in fine, are properties. We might be content to rejoin that this is an excursion into other than psychological fields, but we were but now complaining of the restricted range which psychology allows itself. The experimental psychologist of all men should be distrustful of hard and fast laws of thought. It is not long since he has had beautiful illustration of the possibility of living in a world where everything is wrong side up and yet acquiring the power to adjust his habitual way of seeing things to the new conditions most completely. It is impossible to say why it is any more necessary to the mind to conceive of forces as properties of matter than it is to think of matter as a convenient inferential classification form for force. Psychology would be more logical, more 'genetic,' if it would begin with the impersonal dynamic form of statement 'it rains,' 'there is a noise,' 'it mews,' 'it hurts,' etc., in describing experience. The fact that the various forces of experience are combined into secondary units of apparently simple experience does not impair the propriety of such usage. Excentric projection does seem to place our sensation in the end of the pointer, but we do not oppose this fact of experience to our knowledge that the mechanism of sensation is in the several discrete points in the fingers. We are reconciled to live in a world of illusions and realize that, so far as materiality is concerned, the rainbow is as real an object as the mountain over which its brilliant banner is unfurled.

A careful examination of the field will convince any one that the greater number of difficulties at present perplexing psychology are due directly or indirectly to the influence of the 'matter idea.' What reams of good paper have been spoiled in the attempt to explain or explain away the doctrine of psychophysical parallelism. What absurdities in the name of anatomy have been perpetrated in the effort to find a seat for the soul. How incompatible has it not seemed that there should be such a multiplicity of activities, but a single thread of consciousness.

But it may be clearly shown that physical science, which formulated the matter idea as a scientific postulate, has found it inadequate for its own purposes and contradictory of the facts in the more recondite applications in molecular physics. In support of this statement the reader is referred to the literature of the vortex atom, especially the mathematical physical discussions of Lord Kelvin, also to the address of the President of the Mathematical Section of the British Association for 1895. Dr. Hicks suggests that, when all is known, "it will be found that all phenomena are manifestations of motion (energy) in one continuous medium." The postulate of 'one continuous medium' is a concession to the supposed necessity of mediating between one form of activity and the next following, and this necessity is felt to be due in turn to the idea that forces are properties of matter. Having gotten rid of the material units, it were simpler to part with this idea of medium and accept the fact of interaction, which is quite as simple and intelligible as any machinery that can be devised wherewithal to explain it.

Among those who have attacked this question from the side of physical science no one has more illuminated it than Professor Wilhelm Ostwald, professor of chemistry at Leipzig. In his well-known address before the naturalists, at Lübeck, he called attention to the unjustifiable extension of the physical law of the conservation of energy to form the metaphysical axiom of the conservation of matter. He says: "It is important to notice that by this extension a multitude of hypothetical elements have been introduced into the notion which was at first quite free of hypotheses. Particularly, in the light of this theory, chemical processes are construed in opposition to appearance in such a way that in case of chemical combination, instead of a new substance with new properties resulting from the process, the old substances remain in the new. Thus, in the combination of iron and oxygen to form ferrous oxide, although the familiar properties disappear and new ones take their place, we are yet to believe that the iron and the oxygen are somehow concealed in the compound and simply have taken on new properties. We have become so accustomed to this view that it is difficult for us to realize its peculiarity or rather its absurdity. But when we reflect that all that we know of any substance is the sum of its properties it appears that the assumption that a given substance is indeed present, but no longer possesses any of its properties, is not far removed from nonsense."

"As a matter of fact, this purely formal assumption serves merely to combine the commonplace facts of chemical processes, particularly stoichiometric laws, with the notion of an invariable matter."

“The assumption that all natural phenomena must be referable ultimately to mechanical forms is not entitled even to be regarded as a serviceable working hypothesis—it is a simple error.”

“The most promising scientific legacy of the departing century is the substitution for the old mechanical view of nature of the energetic view.” “If all that we know of the external world is in the form of relations of energy, what reason have we to assume in this external world something of which we have no experience? Yes, it is replied, but energy is simply a product of thought—an abstraction, while matter is the real. I reply, just the contrary. Matter is the theoretical thing which we have constructed, and that very imperfectly, in order to represent the permanent in the changeable of the phenomena. Now that we begin to understand that the actual, *i. e.*, that which acts upon us, is simply energy, we have to determine what the relative position of the two ideas is, and the outcome is not doubtful; it is that the predicate of reality can only apply to energy.”

I have quoted these passages with the more pleasure because similar ideas have been uttered in my own class room for many years from the standpoint of biology and psychology. It may be permitted to repeat words used years ago but printed again in the *Denison Quarterly* in 1896. “The separation of force as distinct from matter is a secondary analysis—curious from its psychological side and wholly illusory and connected with limited conceptions of volition—and hence of causation. With all this sophistication we have forgotten or neglected that primitive naïve method of nature contemplation which views phenomena as phenomena, *i. e.*, as events, not as the apotheosis of something else—say of matter. There is no reason why we should discriminate against this view which involves the immediate acceptance of the data of consciousness as real.” “The attempt to introduce the element or idea of cause at this point is to misapprehend the sphere of causation, which as limited and of subjective origin, has no place in ontology. What then is ‘substance’ which forms the ground of the phenomenal? We have seen that it is not necessarily matter. It might be shown, to mention one difficulty out of many, that no explanation of the molecular conditions of matter has been able to dispense with the postulate of a non-material ether. Our own answer to the question propounded is briefly: Substance is pure spontaneous energy. Energy is used to imply a doing without the implication of resistance, thus of effort. \* \* To speak of energy as *residing in* something is to introduce an utterly incongruous concept, for it continues our quest *ad infinitum*.”



Now we venture to suggest that such a view of substance is of the greatest service to the psychologist, even though it may seem to the metaphysician to cut rather than to untie the Gordian knot. Accepting the data of experience as the foundation, a dynamic psychology, like a dynamic physics, seeks to construct its superstructure, not by the introduction of metaphysically postulated matter, but by combining the elements of experience. The organ of consciousness is adapted to analyze the various forces impinging on it by recognizing differences in the form and rate, in the kind and intensity of the stimuli, and these differences are the material of consciousness. It is idle to object that differences in mere form of vibration or mode of action are insufficient to account for the diversity of the essences making up the universe. That is exactly the question. Light waves of one rate produce upon the mind an impression totally different in kind from those vibrating at a slightly different rate, and the effect of electrical waves in one phase is quite unlike that of similar waves in another phase. In fact, the whole world of physics is a sphere in which observed differences are reduced to variations in rate and form of motions. It is this same idea that must make a permanent place for itself in psychology before further progress toward the simplicity of truth can be made. Nor should we feel that the dignity and worth of the psychical are lost by the recognition that it is one in essence with humbler activities. The sap that feeds the rough bark also nourishes the blossom. From the above point of view one may approach the vexed question of psychophysical parallelism with more confidence. It is not true that the body and soul are disparate and wholly incapable of interaction, for they are different expressions of the same force associated as parts of one system. It is not true that the two are identical, for they are different in form, and this difference is sufficient to distinguish physical and psychical *toto cælo*. It is not true that one is the outside and the other the inside of the same curve; they are not different aspects of identity, but they are parts of the same system and so intimately related, but, being different in form, they are, in fact, different in essence. It is to be expected that these ideas will seem obscure to many by reason of their unfamiliarity, but we believe that the method is that of unsophisticated experience and that the results conform to the highest criteria of modern science. The application of this theory to the search for an organ of consciousness has formed the subject for a paragraph in an article in the *Journal of Comparative Anatomy* for March of the present year.

From this article I may be permitted to quote :

“The search for the organ of consciousness has remained unfruitful by reason of the total disparity between the conscious and any conceivable form of purely neural activity. Nevertheless it is plain that some sort of neurosis does, in every case, form the immediate preliminary to consciousness, and it is equally clear that not every sort of nervous excitation is an adequate occasion for the emergence of consciousness. The metaphysical nature as well as the peculiar unity and continuity of consciousness has militated against the idea of localizing this power, and has disposed to a dynamic view, viz. : that the condition of consciousness is not topographical but consists in the form of *activity*.

“It is plain that, in the nature of the case, it is impossible to discover a specific portion or a definite *kind* of matter in which consciousness resides, for no complexity of the material unit could make intelligible the diversity in consciousness, while *any* complexity destroys the objective grounds of unity. It is equally hard to discover any physiological basis for the continuity of consciousness. The idea of consciousness as a property is accordingly abandoned, and it remains to conceive of it as a *form of energy*. Pure energy with the attribute of spontaneity it could only be if it were in the mode of absolute equilibrium, in which its activities should be wholly reflected into themselves. This can only be predicated of infinite essence, and it is necessary to substitute the conditions of *relative* equilibrium in a sphere of interfering activities. The last few years have revealed in the cerebrum a mechanism of neural equilibration of unsuspected complexity, and all that we have recently learned of the physiology of the nerve stimulus only emphasizes the belief that the whole of the cortical complex is adapted to act as a unit though not as an invariable unit.”

Nor does the energetic point of view simplify alone the problems of psychology. There has been a notable tendency in America of late to return to a vitalistic hypothesis in biology. It is no answer to those who advocate a vital force to state that such a force has not been isolated. It seems, when stated, a self-evident proposition that the receptivity of man, which by the limitations necessitated by the conditions of its development is adapted to admit certain segments of the infinite sphere of energy and no others, could not be expected suddenly to develop an ‘organ’ by which vital forces could be apprehended in other terms than those applied to physical forces. This is much the same difficulty as that experienced by the chemist when required by the biologist to afford him an analysis of living matter. The chemist

must first kill the protoplasm, and it will never be possible for him to affirm that the formula he determines expresses the state of aggregation of the living organism. Biologists, accordingly, have become modest, and admit that no forces have been found in living things not also common to inanimate objects. It is added that the peculiarity of living matter that distinguishes it from all inorganic substances is its power of coördinating the physical forces for the perpetuation of the individual existence or a chain of such lives. To say that the various physical forces are coördinated might mean that there is some outside force restraining them, or it might mean that by some means the several forces concerned are brought into a state of relative equilibrium by reason of interaction among themselves. Now, this state of equilibrium is simply a modified *form* of motion, and, as the differences between the several forces are, as we have seen, simply matters of form, we here have just what is necessary to constitute a new or different force as a result of the fusion of its components. When this form of action is disturbed, the force breaks up into the familiar modes of ordinary experience, and 'vital force' as such eludes our search. This does not prove that there is no such thing as vital force. In fact, we learn of vital force, as we do of any other of the forces for which we have no special 'organ,' by inferential processes, and this fact does not invalidate the inference. In view of the present tendency toward vitalism, it is simply desired to show that a form of this idea is justified from the dynamic point of view. There is just as much evidence for the existence of a vital force as there is of the existence of a force called electricity. Electricity and vital force are both forms of activity, and all activity has its roots in a common energy, the several so-called forces being simply various forms of its expression. Very much of what has been said of vital force applies, *mutatis mutandis*, to the psychical manifestations.

To the student of metaphysics who knows his Schopenhauer these conceptions are familiar. The author of 'The World as Will and Idea' said: "Matter is nothing more than causation. Its true being is its action." This is built upon a psychological foundation; but it must be admitted that the effect of metaphysics upon psychology has apparently been wholly insignificant. In conclusion, it may be noted that by the use of the term 'energy' for the universal, and 'force' for the limited manifestation of action, we reverse the usage of German writers, as may be seen, *e. g.*, in the recent work of Adolf Wagner, *Grundprobleme der Naturwissenschaft*, a work, by the way, that presents the metaphysical aspects of energetics in an attractive manner.



The writer's apology for thus trespassing on this field is his desire to promote a frank adoption of a dynamic method in psychology.

C. L. HERRICK.

UNIVERSITY OF NEW MEXICO, September, 1898.

## THE POSTULATES OF A STRUCTURAL PSYCHOLOGY.<sup>1</sup>

This article is a *manifesto* of importance to all students of psychology and philosophy. Its question has become one of general importance,<sup>2</sup> and Professor Titchener, by virtue of his recognized achievements in his chosen field of psychology, and by virtue of his general official prominence, is more than entitled to deal authoritatively with conceptions about the scope and method and material results of experimental psychology.

I. The chief gains that accrue from this paper are due to what may naturally be called its *epistemological*<sup>3</sup> point of view. I do not altogether like to put the matter thus, for I do not wish to lose light of the positive psychology that it contains, or of its author's statement of its 'main object.' And I also wish to do all I can to remove the erroneous impression that, 'of course, philosophers never will make any serious attempt to get really inside the psychological point of view.' Mr. Titchener's epistemological point of view is defined in the first third of his paper in regard to the *scope* and the *divisions* of psychological science, and in the second two-thirds in regard to what he

<sup>1</sup>Cf. the article by E. B. Titchener. *Philosophical Review*, September, 1898, pp. 449-465.

<sup>2</sup>Professor Titchener informs us in a note, occupying the half of his first page, that his article 'contains a part' of his 'reply' to a criticism (published in this REVIEW, July, 1898) of his 'view of the psychological self,' made by me at the 1897 meeting of the American Psychological Association. His article has a value independently of that criticism of mine, and I shall not in the main speak of it as a reply to my criticism. My criticism was not so much of his 'psychological self,' as such, as of the fact that he did not seem to me to allow, in his treatment of the 'psychological self,' for some admissions that he made in certain general portions of the book. His present article opens up some important epistemological considerations which at once generalize and dignify our 'discussion.' It is, at the present moment, idle to deplore or ignore methodological and plain statements regarding psychology and psychological facts. There are not wanting signs, in a recent article (this REVIEW for November, 1898) by Professor Münsterberg, that he too has felt their necessity in dealing with some of his 'English' and 'foreign' co-workers and critics. The 'discussion,' too, of Professors Baldwin and Dewey in the November number of the *Philosophical Review* certainly turns upon epistemological considerations regarding psychology.

<sup>3</sup>Professor Münsterberg uses this word, *loc. cit.*

himself calls the *nature* and *number* of the structural elements of mind. As an outcome of the first part we recognize how true were the words of Mr. Stout, in his preface to his *Analytic Psychology*, about the coming of the time when no one man would any more think of writing a book upon psychology in general than he would think of writing a book upon mathematics in general. (A) Mr. Titchener distinguishes for us, with the help of biological considerations, (1) the psychology of structure or structural psychology, (2) the psychology of function or functional (descriptive) psychology, (3) ontogenetic psychology, (4) taxonomic psychology, (5) social psychology, (6) phylogenetic psychology. A 'very large portion of experimental psychology' is really structural or morphological psychology; it is a 'vivisection which shall yield structural, not functional results.' This is Mr. Titchener's chosen domain—the discovery of "what is *there* [in 'mind'] and in what quantity, not what it is there for." His own *Outline*, he would have us infer, deals with the first of the six different brands of psychology.

(B) "There can be no doubt that much of the criticism passed upon the new psychology depends upon the critic's failure to recognize its morphological character." Surely, then, no one in the future will criticise experimental psychology for not giving us what it does not profess to give. Indeed, we shall not do so if the said experimental psychology keep rigidly to its own point of view. (C) Mr. Titchener again tells us that structural psychology has not yet come to an agreement about more than the psychology of sensation and the constitution of the sensation element. He himself regards, as we know, the affection process to be also an elemental process. A majority of psychologists do this, he says, there being a minority who do not. "It is natural, in view of the intrinsic difficulty of the subject, that the psychology of feeling should be in a less settled state than the psychology of sensation." Going up higher, the 'anatomy of functional complexes,' *i. e.*, the structural study of the 'higher [mental] process,' the 'perceptions and emotions and actions handed down in popular and psychological tradition,' is as yet \* \* \* a 'mere plan of arrangement.' (D) As to the second way in which the epistemological point of view is applied: "The elements of the experimentalists, as they themselves have been the first to acknowledge, are artifacts, abstractions, usefully isolated for scientific ends, but not found in experience save as connected with their like." This is emphatic enough. Let us not any more go to experimentalists and say: 'Your *sensations* and *affections* and *volitions* and *emotions* are very different things

from what we actually experience, are just so many poor, thin, caricatures of the organic experiences we feel in daily life.' He can reply to us that he is dealing with the structural phases of these processes, and that for more than that we had better betake ourselves to some of his colleagues. Now I think that I understand these four points. Let me look at *some* of their consequences, for I must be brief.

II. (1) As far as in him lies, should not a structural psychologist observe that accuracy of confinement within his own proper sphere that he request his critics to think about before attacking him? Mr. Titchener says things about functional psychology that may be questioned. (a) "It cannot be said that this functional psychology, despite what we may call its greater obviousness to investigation, has been worked out with as much patient enthusiasm or with as much scientific accuracy as has the psychology of mind structure." Far be it from me to compare the zeal or the patience of a body of men from the time of Aristotle to that of the English associationists with that of the heroic pioneers and workers in the experimental psychology of this century, but I shrug my shoulders and ask about the standard of 'scientific accuracy' implied in the preceding and the following sentence. "But it is also true that the methods of descriptive psychology cannot, in the nature of the case, lead to results of scientific finality." Finality on any one plane of investigation is a different thing from finality along some other plane. *Λέφοιτο δ' ἂν ἰκανῶς, εἰ κατὰ τὴν ὑποκειμένην ὕλην διασαφηθεῖη.* Mr. Titchener's conception of *science* in this article is, I think, to be inferred from his phrase about the arrival of the 'time' for 'the transformation [of psychology] from philosophy to science.' He means experimental science, as that is ordinarily understood, consequently he has no right to judge of functional psychology *merely* from his standpoint. And if some of his words in this article (to which I shall immediately refer) about the *last things* of mind were true, some of his other four psychological disciplines would also be 'in the air,'—be absolutely unscientific.

(β) Ought not a structural psychologist—and this point is even more vital—to be able to adhere rigidly to his 'structural' point of view, *at least within the realm of his own observation and scientific disputation?* I will adduce one or two reasons for saying that I do not find Mr. Titchener to do this. (i) He uses the expressions 'elemental processes' (457), and 'elements' (455, 462), and 'last things of mind'—I will not say *interchangeably*, but at least in a manner that makes it difficult for the reader to keep the 'structural' view persistently in



sight. I am only too painfully aware of the imperfections of language to press this point, although I cannot overlook it nor fail to see its influence in wrecking his own argument. I will, therefore, supplement it by saying that perhaps he ought to be held responsible ( $\beta$ ) only for the use of the two 'elements' called with admirable precision the *sensation-element* and the *affection-element*. These two things bear the weight of his whole article. He is endeavoring to set forth the structural 'elements' (450, 453) in the elementary mental processes. And his result is (462): "The affection-element is constituted of quality, intensity and duration; the sense-element (sensation or idea) of quality, intensity, duration, clearness, and (in some cases) extent." But *quality, intensity, duration, etc.*, are not *elements*; they are *characteristics* or *categories* [of sensation and affection]. That is, despite his words *constituted* (and *constituents* (p. 450)), he does not analyze the sensation-element or the affection-element into simpler elements. Nor are the sensation-element and the affection-element themselves *elements*; they are *processes* or phases of *processes*. (Mr. Titchener uses the word *processes* again and again on pp. 457-8-9, and he compares his elementary processes to other alleged 'processes,' such as will-processes, etc.) Now are *processes*, or phases of processes, facts of structure or facts of sequence? I think that they are facts of sequence. Indeed, the very fact of *process* is not a fact of 'structure,' but something more than this. In short, Mr. Titchener does not succeed in maintaining the structural point of view throughout the central sections of his article. ( $\gamma$ ) Terminology and statement apart, Mr. Titchener does not, in disputation, keep to his own confession that the 'elements of the experimentalists are artifacts, abstractions.' He *uses them as if they were real things*, and does battle with them against all other 'candidates' for 'elemental rank,' such as alleged will-process. He uses them not merely 'for scientific ends' but for dogmatic and ontological purposes. "What (459) is our justification for looking upon them ['these different processes,' preceding sentence] as last things of mind?" How, I ask, can an 'artifact' be a *last thing* of mind? A *last thing* of mind might, *e. g.*, be the *connection* which Mr. Titchener tells us always exists between these elements, but not the element as an 'artifact.'<sup>1</sup>

<sup>1</sup> I purposely overlook Mr. Titchener's 'anatomical' reasons for regarding sensation and affection as last things of mind. The 'irreducibility' test and the physiology test yield different results to different psychologists, and would yield different results to Mr. Titchener's six psychologists. Mr. Baldwin, *e. g.*, representing Mr. Titchener's *fifth* kind of psychology, claims that the mind cannot think of itself save as one term of a social relation. The inability of mind, if

III. What I do find in Mr. Titchener's article is a double point of view about structural psychology. (1) The conception of structural psychology as denoting the accredited results of a certain point of view regarding mental process or processes, to wit, the point of view characterized by the categories of quality, intensity, quantity, duration, etc. (2) The conception of structural psychology as depending upon certain peculiarities in its object-matter, to wit, that its object-matter is mental 'elements,' irreducibles of some kind or other. I think that the first point of view is successfully set forth by Mr. Titchener as the point of view adopted by experimental psychology, and, in general, it is my opinion that experimental psychology should seek to differentiate itself from the other five psychologies, not by its subject-matter (for surely its hope is to treat *all* mental processes experimentally), but by its point of view—its 'categories.' And I think that the second point of view breaks down in Mr. Titchener's own hands. This is enough for my purpose. Of course, I believe that it will break down in anybody's hands.

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### PSYCHOLOGICAL METHOD.<sup>1</sup>

These three articles of Mr. McDougall (read as papers before the *Aristotelian Society*, London) seem to have an undoubted relevancy and utility at the present time, when the subject of psychological method is for various reasons being actively discussed. They accomplish their purpose, if by *method* we mean (with Mr. McDougall) not so much ways and means of going to work in psychology, but the *method of conceiving the scope of psychology and of psychological processes*. By their contention that there can be 'no complete science of *conscious* processes' as such, they will be welcome to the experimentalists; while, by their view of the dynamic function of consciousness and of the *efficient* or active relation sustained by 'conscious' to 'neural' process, they will gratify the opponents of what, in the real, is a *last thing* about the mind, just as much as the perception of color. In one regard it is a 'complex' fact; in another it is a simple and *irreducible* fact. The physiology test, again, yields the fact of *function* as a last thing about mind. A physiological expert, *e. g.*, Mr. J. S. Haldane, insists (*Nin. Cent.*, Sept., '98) on the difference between physiological and mechanical process, by holding that physiology studies vital functions. All this shows that no one kind of psychology is entitled to talk about the last things of mind.

<sup>1</sup> *A Contribution towards an Improvement in Psychological Method*. W. McDougall. *Mind*, New Series, Nos. 25, 26, 27, January, April, July, 1898.

gion of mind, since the days of Leibnitz, has been called *automatism*. They provide, in other words, for the unification of our conceptions regarding the relation of 'conscious' to 'neural' process. Let me outline the argument.

Leading psychologists of to-day (*e. g.*, Ward, James, Stout), of course, recognize in their writings *functions of the mind* instead of *faculties of the soul*, and they also allow of the existence of processes not easily recognizable by direct introspection and, in fact, usually discoverable only by inference. They do not, however, set forth clearly the relation of the 'unconscious constituent' (Stout) to the flow of mental activity or to the ordinary *presentations* (Ward) of the mind. The doctrine of the simple concomitance of mental and neural process as represented by various writers is objectionable for two reasons: (1) it cannot be brought into relation with our general conception of causal sequence; (2) the teleological reason that any phenomenon (consciousness, in the present instance) that constantly appears in any group of animals has a part to play in the development of the individual or the species. There are various forms of the concomitance or the parallelism doctrine, but they are all unsatisfactory.

What, then, to simplify matters, are the conditions of the occurrence of consciousness *in terms of neural process*? The answer is that consciousness seems to occur wherever *new experience* has to be acquired. Experience means the establishment of new relations among nerve cells and their processes; it is the establishment of new relations among neurons. This view is tested by careful analysis and illustration in Mr. McDougall's second article. The nervous system of a mammal seems to consist of superposed systems of reflex paths, together with a great mass of neurons (*i. e.*, nerve cells and their processes) at the top of the systems, not yet or only imperfectly organized into reflex paths. His biological and physiological theory of conscious process is in agreement with Stout's doctrine of *apperception* as the process by which a mental system appropriates a new element or otherwise receives a fresh determination. In terms of neural process apperception must be conceived as an extension and further complication of a mental system by the incorporation into it of other neurons and systems of neurons, so that the complex path leads to a modified efferent outflow.

The logical inference from the foregoing is that "all adaptation of nervous reaction to environment has been accompanied by consciousness," and that "the organization of the simple reflexes and instincts of the lower animals was accompanied by a consciousness of a very



low kind." In man the reflexes of the cord and lower part of the brain were organized long ago, before man was man, and some low form of consciousness accompanied their organization. The highest organism is not the one whose nervous system is most completely organized for reaction upon a limited environment, but the one whose nervous system affords the greatest possibility of new adaptations, and so of the most complex and intense consciousness. Consciousness, then, is the force that makes mind, that makes of neural process *experiences*.

All this is set forth in Mr. McDougall's articles with admirable clearness and competency, on the basis, too, of much exact scientific and philosophical scholarship. They are a kind of touchstone on which the psychologist of 'structure' or the psychologist of 'function' or the philosophical psychologist may try the validity and the actuality and the relevancy of his ideas on mental processes or on the 'positive science of mental process' (Stout). And there are a few metaphysical thoughts, at the close of the third paper, on the relation of consciousness to 'other existents' (Shadworth Hodgson). I should much enjoy discussing these in another place. The upshot of the exposition is that no psychologist should allow himself to think of any line of absolute separation between psychical and neural process. This is the simple outcome of the teaching of biology and physiology—sciences from which, in my opinion (and it is obviously Mr. McDougall's), both the psychologist of function and the psychologist of structure have still much to learn. It is manifestly absurd to think of neural process as a thing altogether complete in itself before the irruption of consciousness, for, as Mr. McDougall points out, we are warranted in crediting the simplest organisms with a kind of consciousness. From the *genetic* point of view in psychology (represented, for example, in Professor Baldwin's recent work) we may see in our neural and automatic and instinctive and 'unconscious' processes the organization of experience that was slowly and consciously acquired in the past. The whole difficulty in which psychologists find themselves concerning the relation of conscious to 'unconscious' process is due, I think, to the old tendency of thinking of consciousness in the Cartesian way as the representation of 'representations,' *altogether apart from the fact of the activity or experience process of the organism*, or apart from the fact that our consciousness of the self is not static but dynamic, and that our consciousness makes us aware only of the thought relations of an experience process or content.<sup>1</sup> All this has

<sup>1</sup>I have tried to show this in my '*Schopenhauer's System in its Philosoph-*

become most apparent from recent psychological discussion.<sup>1</sup> But to Mr. McDougall we must express our obligations for formulating the question of the relation of conscious to neural process as one lying at the threshold of our conception of the scope and province of psychology.

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*ical Significance*' (Blackwood. Scribner's, 1896. Reviewed in this REVIEW Nov. 1, 1896).

<sup>1</sup>Even Stout, *e. g.* (*Analytic Psychology*), argues for the idea that psychology may be considered as 'the science of the development of mind.' Elsewhere he says that the 'individual's consciousness is but a fragment of the general system of the world.' And Baldwin (*Mental Development*, p. 3) expresses the same surprise that I have already referred to, that the 'new psychology has hitherto made so little use of the genetic (or biological) point of view.' James, in his *Principles of Psychology*, talks of the 'efficiency of consciousness' in a way that is altogether in advance of Cartesianism or presentationism.

## PSYCHOLOGICAL LITERATURE.

### PSYCHOPHYSICAL AND PHYSIOLOGICAL.

*The Reaction Time of the Heart, of the Vaso-motor Nerves and of the Blood Pressure.*

*Some Reflections and a Hypothesis upon the Form of the Capillary Pulse.* A. BINET. *L'Année Psychologique*, 1898, pp. 316-336.

*The Influence of Prolonged Intellectual Work upon the Rate of the Pulse.* N. VASCHIDE. *Ibid.*, pp. 356.

*The Application of the Graphic Method to the Study of the Intensity of the Voice.* B. BOURDON. *Ibid.*, p. 369.

In his first article M. Binet reports some experiments made with plethysmographic methods in the attempt to determine the time required for reaction by the various organs indicated in his title. Thus, for example, with a plethysmograph attached to one hand, his subject was required to press with the other upon a dynamometer. The point at which the heart-beat accelerated was then determined, and the time between this and the pressure of the dynamometer gives the duration of the heart reaction. M. Binet discusses a number of sources of error, which are numerous, and gives as an approximate figure for the average of such reactions 1.5 seconds. The vaso-motor reaction averages 3.5 seconds. Other observers have shown that the small vessels react much more slowly than the large ones, so this average is somewhat equivocal. The experiments in pressure were not successful. The difficulty encountered [using Mosso's sphygmomanometer] rests on the inter-relations of changes in pressure with changes in vaso-constriction. It is very difficult to isolate these factors from one another. The second article is dedicated to showing the inadequacy of the generally accepted statement that increase of pressure causes a decrease in the distinctness of the dicrotic. The vascular tonus, the force of the heart-beat, the quantity of blood and the rate of blood-flow all play a part in the determination of the form of the pulse. A series of facts is cited to show the contradictions involved, if change in pressure is alone invoked to explain the changes in the dicrotism. Vascular tonicity and high pressure apparently are antithetic to one



another, the former resulting in increase, the latter in decrease of the dirotic. M. Vaschide reports his observations, extending over a number of days, showing that intense intellectual labor, when continuous throughout such a period (the most striking results occur in nine consecutive days), produces relatively regular, progressive decrease in the average rate of the heart-beat. This agrees with the observations of previous investigators, but the periods concerned are longer than those hitherto studied.

M. Bourdon's article is in large measure given over to a presentation of the difficulties involved in his problem of measuring the intensity of the voice. Apparatus is described for determining the force of the breath, both from the nose and the mouth, the vibrations of the walls of the throat and the movement of the lips. A table shows the results, measured by tambour tracings on a smoked drum, of combining each of the consonants with the eleven chief vowel sounds in French. Both methods and results are apparently still in the pioneer stage, and the latter do not as yet lend themselves to any very sweeping generalizations.

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*Studies from the Yale Psychological Laboratory.* Edited by E. W. SCRIPTURE. Yale University, New Haven, Conn. 1898. Vol. V. Pp. 105.

The major portion of the present number of the Yale Studies is devoted to acoustic space. Mr. Matsumoto publishes his thesis on this subject presented to the University of Tokyo for the doctorate. The value of the paper lies not so much in the presentation of new facts, as in the systematic and thorough treatment of the various factors involved.

The apparatus employed in most of the experiments consisted of a spherical cage in which the subject was seated and upon which telephones or small metallic hammers could be adjusted at certain points. In part of the tests a cloth chamber was substituted for the cage, in order to eliminate the effects of reflected sound. When desired, two sounds could be given simultaneously from different directions and with like or different intensities.

Like other observers, Mr. Matsumoto finds that the localization of sound depends on the difference between the sensations arising from the two ears. A sound is localized on the side of the ear from which the most intense sensation is received. When both ears are stimu-

lated with equal intensity the sound is localized in the median plane. The great uncertainty attaching to the localization as front or back in this plane is lessened by anything tending to afford differentiation for sounds from the two directions: *e. g.*, diminished intensity or modification of overtones of sounds from the rear, owing to the effects of the pinnae and the conformation of the external meatus. Two sounds at different levels will, if of equal intensity, be localized at a point midway between the two. When two such sounds move from a common starting-point in different directions, the localizations will take a direction representing the resultant of the two movements. The confusion of front and back leads to occasional exceptions to this rule. Judgments of distance apparently conform to the relations involved in Weber's law, *i. e.*, geometrical increase of sound intensity is accompanied by arithmetical decrease of estimated distance.<sup>1</sup> The judgment of distance thus depends upon absolute sound intensity, whereas direction depends upon the relative intensity of the component factors from the two ears. The discrimination for change in the direction of sound is most delicate when the point of departure is in the median plane and the movement is away from this.

The concluding part of the paper is devoted to a very brief critical review of typical theories of acoustic space. The author adopts what he calls a motor theory, in accordance with which acoustic space seems to resolve itself into a series of motor impulses flowing from auditory stimulations. These impulses develop according to the general laws of association and natural selection. Space is primarily visual, tactual and motor. The connection of this theoretical matter with the experimental observations is the least successful portion of the thesis, which gives evidence of a deal of painstaking labor. It is to be hoped that Mr. Matsumoto will publish a fuller account of the introspective observations of his subjects, and also some statement of the method by which the localizations were indicated, *e. g.*, whether by pointing, and the errors thus involved.

Dr. Scripture, in a brief article on binaural space, proposes a formula for the general expression of the dependence of the localization of a sound upon the intensities of the respective components from the two ears. He supposes (1) that "the distance right or left of the median plane is proportional to the difference between the intensities of the two components, *i. e.*,  $x = cd$ , when  $c$  is the factor of proportionality" and  $d$  the difference in intensity between the right and left

<sup>1</sup>A similar relation between intensity of light and judgments of distance has just been observed. Cf. Ashley, PSYCHOLOGICAL REVIEW, 1898.

components; (2) \* \* \* "the relation between the distance from the median plane and the distance from the auditory axis is expressed by  $y = me - \frac{x^2}{am}$ , where  $m$  is the distance of the sound when  $x = 0$  (*i. e.*,  $d = 0$ ) and  $a$  is a proportionality factor." A complete expression must provide for reference to a system of coördinates in which the three principal planes of the body shall be represented, *e. g.*,  $X$ ,  $Y$ ,  $Z$ , so that a localization in any direction may be accounted for. Then  $x = cd$ ,  $y = me - \frac{x^2}{am} \cdot \sin a$ ,  $z = me - \frac{x^2}{am} \cdot \cos a$ . A series of curves plotted in accordance with these expressions agree with Mr. Matsumoto's observations, which do not, however, furnish a perfect demonstration because of the lack of accurate measurements for the intensities of sound employed.

Dr. Scripture also reprints from *Science* (1897) his note on 'cerebral light,' maintaining from several observations that the so-called 'retinal light' is of cortical origin. The field for such light is single, not double; the figures do not move when the position of the eyes is changed, whether by ordinary rotation or by actual displacement under pressure. The appearance of visual memory images with these figures leads the author to ascribe their origin to some of the higher brain centers.

W. C. Cooke and C. M. Warren join with Dr. Scripture in a very brief report of some tests upon the memory for arm movements. They find the constant error as related to the elapsed interval variable for different individuals, for different absolute distances and for different modes of experimentation.

Dr. Scripture contributes a longer article upon the principles of laboratory economy, which contains a number of ingenious and useful suggestions upon the arrangement of laboratory and lecture rooms and upon general equipment. He also expresses his views as to the organization of psychological work in the university and the ideals which should be kept in view. Laboratory instruction, as distinct from research work, he would employ simply for developing the powers of observation and technical facility, reserving for lecture courses the conveying of the facts of psychology. The whole article reflects Dr. Scripture's well-known tendency to emphasize strongly the mechanical, physical and mathematical sides of experimental psychology, and it is no doubt well that we should have among us so able a prophet of technique. The following sentence deserves to be quoted in this connection [p. 95]: \* \* \* \* "If the other depart-



ments, such as physics, can show better, brighter and more numerous pieces of apparatus, the students are apt to draw disparaging conclusions. The students are no longer a 'class' to be taught; they are an audience that must be led." This is not the place to discuss the merits of Dr. Scripture's attitude upon this question. It may, however, be safely asserted that many of his colleagues will feel that, with all which is admirable in his own work and the work of his students, there is constant danger, not always successfully avoided, of missing the forest for the trees.

The size-weight illusion among the blind is the subject of a brief report by J. A. Rice. The illusion is found to obtain among the blind, and follows the same general laws as among the seeing, but for touch and the muscle sense it is less marked than with normal persons. A few notes offering blue-prints of Yale apparatus, explaining the Yale color-tester and correcting misprints in the previous volume of studies, conclude the number.

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*Subconscious Homicide and Suicide; their Physiological Psychology.* CHARLES P. BANCROFT. Am. Jnl. Insanity, Vol. LV., No. 2, pp. 263-273.

Dr. Bancroft, Superintendent of the New Hampshire Asylum at Concord, in this interesting though brief paper (read before the American Medico-Psychologic Association in St. Louis, May, 1898), has touched upon timely topics of interest both to medical jurisprudence and to psychology. It offers theoretical explanations illustrated by two similar cases, one attempted suicide and the other homicide, both committed in what the writer considers a condition of split-off consciousness, induced most likely by mild toxic agencies, and accompanied or followed by amnesia.

The writer recognizes the importance to psychiatry of admitting the practical parallelism postulated to exist between consciousness and neural function. It is suggested that the physiological neural dissociation, which the work of Dr. Sidis has shown to be present in hypnosis, may obtain at other times, as in these two instances, through the agency of poisons, and, disintegrating the sum of tendencies of which a personality is composed, cause just such an aberration of the real purpose of the self as suicide and homicide often present. He considers that the action is conducted on precisely the principle of reflex action, the normal inhibitory faculties of the indi-

vidual being then in abeyance, like his judgment. In short, Dr. Bancroft aptly concludes, the organism has then for the time become an automaton, uncontrolled, and actuated by suggestions more or less subconsciously received in normal hours.

GEORGE V. DEARBORN.

*On the Measurement of Mental Activity through Muscular Activity and the Determination of a Constant of Attention.* JEANNETTE C. WELCH. Am. Jnl. of Physiology, Vol. I., No. 3, May, 1898, pp. 263-306.

This is an article, timely and concise, describing work done in the Hull Physiological Laboratory of the University of Chicago. It is a 'continuation' of a research reported by Professor Loeb in a preliminary communication published in 1886 ('Muskelthätigkeiten als Maass Psychischer Thätigkeit,' 'Arch. f. d. ges. Physiol.,' XXXIX., p. 592). An important part of the report is that wherein the dynamograph of Loeb, with which the work was done, is illustrated and described. This piece of apparatus will doubtless become common in psychological laboratories, it having some advantages over those now generally in use. It consists of an axle to which is attached below a short flat bar of spring-steel, which when in use impinges against an iron wedge, adjustable in position by a screw. To the upper side of the axle is attached a rod connected to the handle in which the subject's fingers are placed, while the palm of the hand gets its purchase from a small iron post. From the upper portion of the axle's surface a writing lever projects to the surface of a kymograph record-drum.

Miss Welch found that, when various sorts of mental activity were practiced simultaneously with the static maximum contraction of the hand, the physical force decreased in proportion to the attention required upon the mental efforts. The mental work was various in kind, and comprised such exertions as counting the conflicting rhythms of pendulums, strained visual perception, reading, writing, adding, multiplying, etc. By measuring the ordinates and abscissas of the curves traced by the dynamograph the 'constant of attention' was determined in each case, and this afforded the means for determining the concentration of attention required in the various sets of experiments.

This constant of attention was found according to the formula  $\frac{P - \hat{p}}{P}$ .

In this  $P$  represents the maximum pressure of the dynamograph, *i. e.*, its records when the attention was wholly upon the muscular work;  $\hat{p}$  represents the maximum of the muscular effort with concomitant mental work.

The determination of this constant was found to be no easy task, but in case of some subjects quite satisfactory results were obtained. The attention-constant in the case of one subject was found to range from 0.22 during the 'registration of the vibration of a pendulum by pressing one tube, the perception being visual,' to 0.585 obtained as a mean while 'counting the register of the fifth vibration of a metronome and the second vibration of a pendulum'—the most difficult of the mental tasks imposed.

It was found that the constant of attention for any activity increases with (1) the effort of accommodation of the special sense-organs; (2) the effort in coördination of the muscles; (3) the effort of the memory, and (4) the number of simultaneous 'activities.' It seems likely to the experimenter that all control of the body depends upon inhibition-impulses. "After a certain amount of practice," says Miss Welch, "and with care to have like conditions in every case, I believe that the mean constant of attention for any mental activity can be determined for every subject with as slight variation as the personal equation in time-reaction."

GEORGE V. DEARBORN.

*The Reinforcement of Voluntary Muscular Contractions.* ALLEN CLEGHORN, M.D. Am. Jnl. Physiology, Vol. I., No. 3, May, 1898, pp. 336-345.

This is a report of a research conducted in the Harvard Physiological Laboratory to determine what effect sensory stimuli have upon voluntary muscular movements. The stimuli employed were light (from a 32 c.p. glow-lamp), a sudden sound (hammer falling on a tin disk), and induction shocks on the skin (of the arm). These were applied by electrical mechanism at the instigation of the movements studied. A Mosso's ergograph was employed, the resistance being two kilograms.

It was found that a sensory stimulus applied just as the muscles began to contract caused an increase in the recorded contraction, as other experimenters have reported. On the other hand, the relaxation phase of the phenomenon is shortened by a sensory stimulus applied at the beginning of the contraction.

The substance of the experimental portion of this report may be given briefly thus: The average duration in seconds of a voluntary muscular *contraction* with simultaneous sensory stimulation is with light 0.49; with sound 0.47; and with induction shock 0.44, in contrast to 0.51, 0.43 and 0.38, respectively, without sensory stimulation.



The average duration of the *relaxation* is with light 0.29; with sound 0.29; and with induction shock 0.33, as compared with 0.61, 0.49 and 0.51 in cases without stimulus—a decided and constant decrease in the time.

As to the causes of these phenomena, Dr. Cleghorn suggests that the “acceleration of the relaxation is not due to augmentation of the contraction of the antagonistic muscles, for the relaxation of the extensors does not visibly differ in rapidity and extent from the relaxation of the flexors.” And, again, “the acceleration of the relaxation cannot be ascribed to the sensory stimulus inhibiting the discharge of motor neurons and thus permitting the rapid passive extension of the muscles by the load of the ergograph, for the acceleration does not increase with an increase of the load.” The experimenter concludes that in the present state of knowledge the acceleration is best explained as “an augmentation of an active relaxation-process by sensory stimuli.”

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*Inhibition.* H. S. CURTIS. Pedagogical Seminary, Vol. VI., No. 1, October, 1898.

The author does not explain why, in his ‘summary of the chief theories and facts of inhibition,’ with which he introduces the paper, he confines himself to the psychologies of Herbart, Beneke, Taine and Roux. Are these the only psychologists that have contributed anything of value on inhibition?

Four pages are given to Wundt’s *Mechanik der Nerven* and the physiology of the vagus nerve. Section II. is a discussion of the effect of one activity upon another: Excessive mental work may diminish the strength of the muscles and otherwise interfere with physiological function. On the other hand, hard and long-continued physical labor may be the cause of mental stupidity. One activity may inhibit another, (1) by its waste material, (2) by decreasing the blood supply, (3) by absorbing nutrition directly from resting tissue, and (4) by draining energy from other brain areas.

The higher areas are connected with the motor areas by associational fibers, which are at first more or less impermeable. If mental activity has lowered the energy in one of the higher centers, the energy of other centers tends to press in upon the area of low pressure, and by overcoming the resistance make permeable inter-connecting paths. Education has to do with the formation of these paths whereby one area may use the energy of others.

The explanation of restlessness in children is found in the theory that the lower brain areas of the child have not yet developed paths to the 'rational or associational' areas. The energy of the motor centers has no chance to drain off into the higher centers, consequently it discharges into the motor nerves. By means of pedometer records taken from 34 persons of various ages, the writer finds that country children are more active than city children, and that activity is greatest below six years of age. It reaches its maximum on Saturday. Monday and Tuesday are days of high activity, but on Wednesday, Thursday and Friday it falls off abruptly and makes the minimum on Sunday. By the questionnaire method an attempt is made to get some data on such topics as 'Sitting Still,' 'Restlessness in Sleep,' the physical and mental characteristics of the 'Restless' and the 'Quiet Child,' 'Sickness in its relation to activity' and 'Excitement.' The results are of little value.

The author closes with a section which he calls 'Pedagogical Inferences.' He thinks that, since the motor areas are to furnish so much energy for intellectual activities, "physical health and development must be the first interest of education." This section contains much irrelevant matter.

A serious defect in the whole paper is that the theory, unprofitable in itself, is unsupported by facts. What few experiments are given are wholly unrelated to the theoretical part.

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### FATIGUE.

1. *Arbeitshygiene der Schule auf Grund von Ermüdungsmessen.* F. KEMSIES. Berlin, Reuther und Reichard. 1898.
2. *Unterricht und Ermüdung.* L. WAGNER. Berlin, Reuther und Reichard. 1898.
3. *Zur Ueberbürdungsfrage.* DR. KRAEPELIN. Jena, Fischer. 1898.

Since Professor Mosso's work appeared, half a dozen years ago (*Die Ermüdung*, Leipzig, 1892), a mass of German monographs upon the phenomena of fatigue have accumulated. Most of these bear directly upon the practical question of school work and its exhaustive effect upon children, yet the work has been done chiefly by physiologists, psychologists and alienists. After Mosso the names of Griesbach in Mühlhausen, Ebbinghaus in Breslau, Burgerstein in Vienna, Kraepelin in Heidelberg, and Schulze in Leipzig are most

familiar to us as investigators along these lines. Little has been done by practical school-teachers, who, with directors and school boards, are in the end the persons to be reached if such experiments are to have effect in the abolition of unhygienic conditions and reform in methods of work.

Scientific interest in such matters has now spread to the school-room, and busy masters make time for long series of observations upon the pupils under their charge. During the past year two numbers of Schiller and Ziehen's *Abhandlungen aus dem Gebiete der Pädagogischen Psychologie* have been given up to reports of such studies; the one (*supra* No. 1) consisting of observations on pupils of the third year in a Berlin grammar school, the other (*supra* No. 2), of observations on the scholars in the gymnasium at Darmstadt. Next spring it is proposed to unite the scattered investigators of Germany into an association for the study of children, in the work of which the problems of fatigue will assume an important place. The next years, therefore, are likely to see appreciable advances in our application of the principles of hygiene to school work. Of the above-mentioned brochures I have shortly to speak, together with the recent work of Dr. Kraepelin, professor of psychiatry at Heidelberg (*supra* No. 3), which is one of the most discerning and suggestive among contributions to the study of fatigue in the school-room.

A sharp distinction must be drawn between objective and subjective exhaustion, between fatigue (*Ermüdung*) and weariness (*Müdigkeit*). The one is a constant factor and corresponds to the amount of work done; the other is fluctuating and depends upon the degree of interest in the work. Weariness is a superficial fact of attention, which may appear, disappear and reappear many times in a day; fatigue is a deep-seated phenomenon of nervous exhaustion, which steadily increases with continuance of work, and its results cannot be averted, though they may be obscured. Weariness can be induced in a fresh subject by dull work, monotony, stale familiarity, while the capacity for work, as expressed in the ergographic record, or by refinement of skin sensibility, continues unabated. Change the topic of study, substitute another teacher, adopt a picturesque method, and the pupil's weariness vanishes. No such easy recovery from fatigue is possible; it can be removed only by rest, food, sleep, and light exercise in the open air. Weariness may abate as fatigue increases; the subject frequently appears fresher at the end of a piece of work than in the middle, and his zest of accomplishment is manifested in a reduction in the fatigue curve itself. But that it is not a permanent refreshment



is shown by the rapid and extensive increase in fatigue subsequent to the completion of the work. Fatigue means a reduction in the total effective force of the individual, which can be discriminated and measured; weariness is a fluctuating personal attitude which is scarcely susceptible of record in any form. In the study of fatigue, therefore, we have to seek for the phenomena of actual reduction in capacity for productive work; and from its effects we must discriminate the factors of interest and weariness.

This makes the question of method an important one. The phenomenon to be measured is the reduction in central nervous tonicity which marks successive periods of school work. There is no practical way of observing this directly; indirect forms of measurement must, therefore, be resorted to. These fall into two general groups, depending upon the different ways in which central nervous activity is expressed: first, *mental*, in fixation of the attention, with its complications in memory, judgment and the like; and second, *physical*, in muscular innervation. The physical method of measuring fatigue was adopted by the pioneer in the study, Professor Mosso, whose records were taken in the form of dynamometric tracings of the ergograph. This test, while it is open to the objection that it involves too cumbersome apparatus for wide use, yet continues to be one of the most reliable methods which have yet been devised.

The mental test, on the other hand, has been extensively applied. It is Kraepelin's method and the method of Burgerstein, Haser, Kemsies and many others. The form may vary widely: firstly, in the character of the work required, which may be either a long series of simple examples (v. Laser, Holmes, Richter), or a few pieces of more difficult work (v. Sikorsky, Friedrich, Kemsies); and secondly, in the method of measuring fatigue, which may be either by the decrease in the rapidity with which the work is done or by the increase in the number of errors which occur. A test which has been called the *combination method* was devised by Ebbinghaus, who used paragraphs of text from which here and there words had been erased. The subjects were required to fill in all the blanks, within a given time, with words which made sense with the context. Measurement was by the number of errors occurring. The apparatus for all such mental tests is simple; it requires only the preparation of a set of arithmetical problems or the mutilating of a printed page. Its method of reading results is likewise easy, since it consists in a mere counting and averaging of errors. The truth of its interpretations is, however, by no means so certain. The test does not get at the phenomenon to be

studied at all directly or unequivocally, unless the distinction between fatigue and weariness is to be overlooked altogether. The material from which the results are read is the product of the total set of mental conditions obtaining at the time of the investigation, and the number of errors in any given case will as readily be affected by a feeling of rivalry between the pupils or by a momentary distraction as by the influence of fatigue itself. These influences cannot unconditionally be set down as constant factors, which are, therefore, eliminable. The anticipation of recess or the conclusion of work may very well be potent in establishing a law of rhythmical increase and decrease in the number of errors, which will well combine with the actual exhaustion effects to produce a curve which does not at all truly represent the rise in fatigue. The results of practice, likewise, interfere with the purity of the fatigue curve when it is determined by the number of errors occurring.

In view of such sources of error in the purely mental test Griesbach has employed an *æsthesiometer* to determine the amount of fatigue, a method which has been adopted by Wagner throughout his experiments in the Darmstadt schools. An area was selected upon the cheek and jaw of which the normal discrimination distance for two touch impressions was taken before school work began. The amount of fatigue was measured by the decline in discriminative acuteness which appeared after each successive hour of school work. This form of test has apparently proved decidedly satisfactory; it unites the simplicity of a physical method with a direct psychological fatigue factor.

I shall speak of the results of the various investigations in common. The best work of the school day is done in the first two hours; the last two are the most unfavorable. Within the first two hours the majority of pupils reach their maximum; only on Monday do the third and fourth hours show better work. If work of equal quality is to be obtained during these last two hours it must be done more slowly. For children under twelve years the school day should not exceed four hours; from twelve to sixteen a maximum of five may be imposed.

The best work of the school week is done on Monday and Tuesday; the worst is met with on Saturday; the intervening days show a fairly steady decline from maximum to minimum. After every holiday a return to the initial freshness of Monday occurs. There is no significant increase of fatigue during the course of the school term, apart from the increase of pressure toward its close. There appears thus an incomplete recovery as the week progresses, from the strain of

the preceding day, a grave item to be considered in the arrangement of school curricula.

The employment of any given hour in work which taxes the pupil heavily is marked by a falling off in quality of work during the hour succeeding it. This holds true in spite of change of work, which is designed to relieve the pupil by calling new powers into play. Change of work is held to be rest; but the fatigue persists, no matter what the new subject of study may be. Change of work is recreation only when the new occupation calls into activity a fresh set of powers, that is, only in so far as it is *rest*. Such a method may easily be applied to obtain relief from physical labor; it is to be doubted if it can be successfully employed as a means of release from mental strain. The facts cited point to the conclusion that mental work of every kind is accompanied by general and not localized central fatigue, and it is questionable if new faculties can be appealed to. The fact of clearly defined fatigue, upon which Wagner lays especial stress, bears directly against such a conclusion. All mental work involves fatigue of all and every part of the pupil's faculties, and there appears no absolute escape from it by variation of studies. Only rest will reach it, and to this end both Wagner and Kemsies recommend the introduction of ten-minute pauses at the conclusion of each hour's work.

Four types of subject are to be discriminated: (I.) What may be called the normal, marked by a relatively brief rise in efficiency attributable to elimination of initial distraction, which is followed by a continuing depression, the curve falling away steadily through increasing fatigue. In this type a second maximum sometimes occurs towards the close of the forenoon's work.

(II.) In this type there is an initial maximum from which the curve falls away rapidly and continuously throughout the day. Such children are of weak resistance and must be carefully considered with referencē to the problem of fatigue. They have little elasticity, and rest is absolutely necessary to effective work.

(III.) The third type is characterized by a depression, often excessive, in the middle of the school period, the curve declining sharply from the beginning till mid-forenoon, and there rising toward a secondary maximum during the following period. This type resembles the preceding in its low resistance, but differs from it in its elasticity. The recess almost restores its initial tonicity. Easily fatigued, it readily recovers from depression, and the problem of fatigue is here less grave than with those in whom the depression, more slowly manifested, is much more permanent. In this type the second maximum



is usually reached before the close of a four-hour school session, and the last working period is marked by a rapid increase of fatigue.

(IV.) This type, which Dr. Wagner calls the neuropathic group, is marked by a uniform initial depression, from which the curve of efficiency rises irregularly and rather slowly towards a maximum, which in most cases is reached before the close of the forenoon's work. In all four types the maximum is followed by a rapid increase in fatigue, which reaches its extreme form in the neuropathic group. Here the breaking strain comes swiftly, and if work be continued for any length of time after the reaction sets in it is liable to issue in sudden extreme exhaustion. This type is deceptive and needs the closest care.

The relation of individual studies to the problem of fatigue is of minor importance. Every discipline in turn may be made refreshing or fatiguing. The personality of the teacher counts for immensely more than the nature of the material. Nevertheless, considered in themselves a scale of values may be made out. Mathematics and Classics stand high in all the lists; singing, drawing and religion come far down, as does also the study of German. That is, studies which demand close application tax the pupil heavily, while those in which practice and mechanical routine can play a part are marked by slight fatigue. Gymnastic exercise, instead of being recuperative, ranks among the most fatiguing forms of school work. Only light exercise is recreation. Even the recess period is marked by deep fatigue in those who indulge in violent exercise. Instead of the customary intervention, the various investigators agree in recommending a shorter pause after each hour's work, during which noisy games shall be discouraged and the children taught to seek rest, fresh air and gentle movement. In these lies the solution of the problem of fatigue in school.

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#### TIME-SENSE.

*Zur Psychologie der Zeitanschauung.* F. SCHUMANN. *Zeitsch. f. Psych. und Phys. d. Sinnesorgane*, Vol. XVII., pp. 106-148.

*Zur Schätzung leerer, von einfachen Schalleindrücken begrenzter Zeiten.* F. SCHUMANN. *Zeitsch. f. Psych. und Phys. d. Sinnesorgane*, Vol. XVIII., pp. 1-48.

The first article opens with six pages of quotation from Professor G. E. Müller's dictata on time-perception. A brief review of the general theory there announced is as follows: All sensation qualities

are capable of certain modifications. For example, a given tonal quality may undergo modifications in intensity and in clang-color. Some of these modifications are of such a character that they cannot appear at the same time in the same quality: for example, a high and a low intensity. The result is that we form a complex idea of the quality, which contains a series of possible modifications in intensity. In the case of a second quality we form a similar complex idea, which also contains a series of modifications in intensity which are the same as those in the first case. We come in this way to isolate the notion of intensity. It is in reality only a kind of modification of sensation quality; it is not independent in the mind. Time is analogous to intensity. The difference is that in the case of time we must deal not with a single sensation, but with a complex idea including two or more sensations, and we have in this case not a uniform mode of modification, but a uniform relation between qualities. The relation may be recognized because more than one set of qualities appear in the same relation. But the knowledge of temporal changes is not the result of a special relating activity which is outside of the ideas themselves. The whole group of sensations is held together by the mind in a certain relation, and this relation is essential to the formation of the idea. Its recognition is due to its recurrence with different groups of qualities.

This general theory of Müller's, Schumann attempts to elaborate and defend. First, time is not a special content distinct from the sensation factors. In general, the discovery of differences between simple ideas, as, for example, the difference between two shades of gray, is not due to a separate content corresponding to the difference. The whole process is made up of two sensations and the immediately resulting judgment of difference. Introspection reveals nothing further. Even the assumption of a comparing activity finds no justification through introspection. The comparing activity is not something different from the joint apprehension. In cases where the difference is difficult to perceive and conscious effort is present, the apparent comparison is merely a higher degree of attention, unless, indeed, the mind makes use of secondary aids in the formation of the judgment.

Secondly, there is no need of assuming that for the formation of a temporal judgment both sensational elements must be simultaneously present in consciousness. Of course, the first impression must leave its trace, but this is not a conscious idea; it is much rather to be thought of as a physiological trace. There is no other possibility of immediate perception of duration, for if we require a conscious comparison then the judgment becomes mediate. Thirdly, the psychical present is never

a point; it is a line, and as a psychical process must be regarded as a complex of greater or less duration.

The assumption of a form-quality or of a form-feeling is opposed, on the ground that introspection shows no such factors and it is not required. The remainder of this article and the whole of the second are devoted to a defense of the author's earlier results and explanations against the attacks of other investigators, particularly Meumann.

Estimation of intervals depends on the secondary data of expectation and surprise which arise when the given interval does not correspond to that for which attention is 'set.' This position is supported by introspection, by the phenomena of contact, the underestimation of intervals observed with lax attention, the overestimation of intervals following a pause, and the parallelism between the rise and fall of expectation and bodily movements.

It has been reported that when the last of three stimuli marking off two equal intervals of time is intensified the second interval is overestimated. Now, the intensification of a stimulus results in surprise, and this should, according to Schumann's principle, have just the opposite effect to that reported. In answer, the author reports experiments in which he shows that the common, simple result is underestimation rather than overestimation. In the cases where the intensification led to a conscious introduction of rhythm the result was overestimation. The normal results are in agreement with the theory; the rhythmical complications explain the others.

The constant errors of judgment at intervals of different lengths are not fundamental facts, but are all to be explained by one or more of a variety of influences, such as contrast with previously given intervals, accompanying sensations of strain, rhythmical apprehension, and possibly others.

Meumann finds that, in a long series, sounds heard at a given rate seem more rapid than only two or three sounds at the same rate. Schumann finds that in all cases where this error in judgment appears it is based on a relaxing of the attention during the longer series. Finally, in regard to Meumann's results that a series of strong sounds which succeed each other at short intervals seems more rapid than a series of weaker sounds given at the same rate, Schumann finds that his subjects either perceive no difference at all or else observe the exact opposite of that reported by Meumann. The contradiction may be due to subjective conditions or to the objective conditions under which the experiments were tried.

The investigations of time-perception are among the most difficult



undertaken in experimental psychology. If one overlooks the earlier, rather crude experiments, the number of valuable treatments of this subject reduces to five or six. Unfortunately, the results thus far obtained are frequently of such a conflicting character that one feels that the whole ground must be gone over anew before any interpretation can be commenced. The articles have come to take on a polemical and too often personal character that does not tend to stimulate unqualified acceptance of the statements of either party. The personal equation doubtless plays a very large part in estimation of time intervals, and perhaps it would be well for investigators to recognize this fact once for all, and not to be too hasty in generalizing from their own individual observations or from those of their own 'school.'

The theories, too, are hypothetical in the last degree, and must be so regarded. It seems clear to the present reviewer that both space and time must ultimately be explained as recognitions of relations and not as contents. It is difficult, however, to give such a theory an intelligible statement, and the difficulty of finding a psychical process in which a relation is apprehended is always difficult. This latter difficulty can certainly not be disposed of by calling it an 'immediate' process or by referring it to some undefined physiological traces. The close relation between the unanalyzed processes of immediate perception and the more complex processes of mediate judgment furnishes perhaps a clue that will aid in the solution. The simpler process must be like the more complex in kind, if not in degree.

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*Zwei Beiträge zur Psychologie des Rhythmus und des Tempo.*

KURT EBHARDT. *Zeitsch. für Psych. und Phys. der Sinnesorgane*,  
Bd. XVIII., Hf. 2.

The general result of this investigation may be briefly summarized in the following principle: Whenever a subject who is producing a series of sounds by means of movements of the hand is required to increase in any way the effort necessary to produce the proper movement, his attention will be in part absorbed by the increase in effort and he will not perceive fully the lapse of time. The result will be that he will judge to be equal intervals which are in reality different—longer intervals with greater effort being judged as equal to shorter intervals with less effort.

In the first group of experiments the subject produced series of

movements at regular intervals. The interval was left to the subject's choice and varied between 0.3 and 0.6 of a second. In a first series all the movements were of equal intensity. In a second series every second movement, in a third, every third movement, was emphasized. The result was that all intervals following accented movements were longer than those following unaccented movements.

In the second group of experiments it was found that a musical composition played without its full accompaniment required more time than one played with the accompaniment. Or a piece of music executed on a set of keys not connected with strings, and thus producing no sounds, required more time than one executed on the ordinary instrument. In both of these cases the subject is forced to supply the part omitted by an increased effort of imagination. This increased effort absorbs the attention and causes him to underestimate the duration. Ebhardt holds that in all these cases we have to do with phenomena of activity and with the sensations most directly associated with activity rather than with the more passively received sensations. But even from this point of view it is not easy to see how greater concentration of attention on the sensations is to obscure the duration. The duration can not be regarded as a separate content which can be pushed out of consciousness by strong sensations of movement. Duration is an attribute of sensations or complexes of sensations. That concentration on these sensations is to obscure their temporal attributes certainly requires some explanation rather than mere dogmatic assertion.

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## VISION.

S. Ramon y Cajal's *Neuere Beiträge zur Histologie der Retina*.

R. GREEF. *Ztsch. f. Psych. u. Physiol. der Sinnesorgane*, Vol. XVI., 1898, pp. 161-187.

Dr. Greef gives an interesting summary of the most recent additions made by Ramon y Cajal to our knowledge of the structure of the retina; the subject, up to the stage of its development here described, has been made most accessible to the non-specialist reader in *Die Retina der Wirbelthiere* (Wiesbaden, 1894) by Dr. Greef, who has also made contributions of his own to the work of the Spanish author. The most interesting points now made out (which may be added, for the English reader, to the excellent account in the *System of Diseases of the Eye*, by Norris and Oliver) are the following:

Most important of all, it can now be affirmed, without doubt, that the cones are, quite simply, rods in a higher stage of development. This fact is very much to the favor of those theories of the light sense which regard the color function of the cones as a developed form of the rod function, the latter affording no means of discriminating between lights of different periodicity. That the cones are intrusted with the conveyance of some more complicated form of excitation (whatever the nature of the excitation may be) is indicated by the fact that the knob-like basilar ending of the rods is replaced in them by numerous thread-like expansions. If our subjectively acquired belief regarding the different functions of the rods and cones had happened to be the reverse of what it is (if we had been induced to attribute the color sense to the rods and the undiscriminated light sense to the cones), knowledge on this subject would now be at a standing-still point of contradiction; as it is, it can go on its way rejoicing in one more of those mutual confirmations of reasoning processes proceeding by different routes which are, in general, the source of the confidence we feel in our interpretation of the phenomena of the natural world. Even the latest writers on the histogenesis of the retina have had little to say on the early stages of the infra-limitant portion of the rods and cones. It has only now been made out, by the Golgi method, and especially by means of the double impregnation and the rolling-up of the retina, that the rods and cones pass through a period (in the newborn cat, for instance) when they exhibit no difference in structure (so far as structure is preserved in these methods), and can only be distinguished from each other by the circumstance that the nucleus of one is surrounded by a somewhat thicker layer of protoplasm than that of the others, and so stains darker. (This is a stage in which the end members are wholly undeveloped, and so can give no means of orientation.) The question of their embryonal identity—a question which Cajal himself was formerly obliged to give up—he has now, therefore, been able to solve in the affirmative sense.

Other points which may be noticed in this summary of results are these: There have been many reasons for regarding the rods and cones as differentiated epithelial cells and not as nerve cells or as neuroglia cells—the epithelioid appearance of their outer members, their position as limiting cells in the interior of the primitive optic cup, etc. That they are, in fact, such is now established by the circumstance that in their development they pass through, like nerve cells, a monopolar phase, but that, unlike the neuroblasts of His, the cellulipetal process is first developed, and not the cellulifugal. If



R. y Cajal is right, we have now a criterion by which to distinguish between the three classes of cells which are capable of conducting nervous currents: (1) cells in which the cellulipetal process is formed first (rods and cones, taste-cells, etc.); (2) those which begin their development with the sending-out of a cellulifugal process (the great majority of the multipolar cells of the nervous centers); (3) cells which seem to form both processes at the same time (bipolar cells of the retina, of Corti's organ, etc.). The difference between the bipolar cells intended for the rods and those intended for the cones is much greater in mammals the fourth day after birth than it is later, which confirms Cajal's discovery that these cells are distinctly different.—Recent studies of the retina of the sparrow (in which this organ has reached an extremely high development) disclose a new form of cell (later detected also in the retinae of reptiles and of some mammals) which resembles both in shape and in position the amacrine cells, but which differs from them in having an immensely long (sometimes a millimeter long) axis-cylinder process. Their function seems to be to act as association-fibres between distant amacrine cells. They are extremely numerous, and it is very probable that the ramifications of the centrifugal nervous fibres are spread out around these cells. They may be called the amacrine-association cells.—The retinae of birds offer the best field for the study of the centrifugal fibres; for the finch, sparrow, etc., the Golgi method is best; for the thicker retina of the dove, that of Ehrlich. Cajal is now thoroughly convinced that these fibres all terminate in close contiguity with amacrine cells, and that the function of the latter is to form an important member in a conducting chain between the brain and the junction of the bipolar with the ganglion cells.—Among the regular cells of the ganglion layer are certain others which are now made out to be true amacrine cells, but not in their proper place—dislocated amacrine cells. R. y Cajal has before laid down the rule that for the recognition of the nature of a nervous cell one should not attend so much to the position of the cell-body, for that may vary greatly, but rather to the position and the relations of the protoplasmic processes and the axis-cylinder. By means of this principle Lénhossek has been able to discover the bipolar cells of Cephalopods, although they are on top of, instead of beneath the feet of, the rods and cones, and also their amacrine cells, although the bodies of these cells are quite out of their natural position.—It is more than ever certain that there are nowhere in the retina either nervous reticulations or protoplasmic anastomoses.

C. LADD FRANKLIN.

*Ueber Raumwahrnehmung beim monocularen Sehen.* ROBERT MÜLLER. Philosophische Studien, Vol. XIV., No. 3, pp. 402-470.

This is another investigation of monocular depth-perception by Hering's method of dropping marbles through the field of vision. Only indirect vision was investigated, the aim being to find whether empirical evidence could be educed in support of Kirschmann's theory of the importance of parallax in indirect vision. The results are held to indicate the presence of a means of judging vaguely monocular depth, even when movements are excluded. The absolute localization is attributed to binocular factors that can never be eliminated.

Like all investigations by this method, the results require a good deal of interpreting before the conclusion can be put in its accepted form. Interpretation and historical review make up the main part of the article.

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*Die stroboskopischen Erscheinungen.* KARL MARBE. Philosophische Studien, Vol. XIV., No. 3, pp. 376-401.

The article opens with a summary of all the references to stroboscopic phenomena and a criticism of the results thus reviewed. Then follows a restatement of Talbot's law on which it is held that these phenomena depend. The only part of this section that is particularly new is the description of an experiment suggested by the work of Grünbaum in the *Journal of Physiology*, to show that the slower the contours of surfaces which are to fuse, vary, the less will be the fusion at a given slow rate of movement. A mirror is covered with black paper in which there are two small openings. These openings are so arranged that in one there will be seen the reflection of the periphery of a rotating disk made of white and black sectors. The other opening reflects a part of the same disk nearer the center. If the rotation is just fast enough to produce fusion at the periphery, there will not be fusion at the center.

The apparent movement of stroboscopic figures is due to a failure of the subject to detect the omitted phases of the movement. These omitted phases may be brought out by especially arranged conditions. Such apparent movement is accordingly due to a central process.

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## GENERAL.

*The Origin and Growth of the Moral Instinct.* ALEXANDER SUTHERLAND, M.A. London and New York, Longmans. 1898. 2 vols. Pp. xiii + 797.

These two handsome volumes are well worth reading. The language of the author is lucid and non-technical. His thought is simply presented, although it has involved many years of thought and labor. The author claims full half of the book to be a detailed expansion of the fourth and fifth chapters of Darwin's *Descent of Man*. The whole work, however, is independent in its thought and execution.

Adam Smith and Darwin are the godfathers of the present child. Adam Smith, it is claimed, needed only a suspicion of what Darwin established to have revealed the true origin of our 'moral instincts.' He founded morality on sympathy, but knew not whence the sympathy arose.

Mr. Sutherland's position may be summed up somewhat as follows: The 'moral instinct' is of an emotional nature. Intelligence is a concomitant. A moral action is one which is founded on sympathy. While philosophers, as Schiller says, are disputing about the government of the world, Hunger and Love are performing the task. These two great masses of reactions and tendencies, in the form of self-preservation, selfishness, ambition, will-power, etc., on the one hand, and sympathy, generosity, altruism, love, etc., on the other, are present as potentialities in each human being. The latter sum of tendencies Mr. Sutherland calls moral. In a far more inductive and thorough spirit than that of Herbert Spencer or Drummond, he traces the growth of the sympathetic or altruistic feelings, showing how absolutely necessary they are biologically considered. Parental care must have made its beneficent appearance as an agency essential to the survival of the better fitted, of the higher gifted. Sympathy and altruism arose thus as advantageous variations, securing by their presence longer play and developing periods (Karl Groos) so necessary in phylogenetic development. In the second volume the suggestive and interesting experiments on the temperature of different animals during hatching and incubation are given more fully than in the author's first communications.

Many facts are adduced of the rise and growth of parental care, first in the cold-blooded animals, then in the warm-blooded types, then among mankind. This growth is also well described among the savages, civilized and cultured races and classes. Conjugal tenderness and fidelity begin only on the level of the warm-blooded animals. As the sympathetic tendencies increase in the human races there dawn the



feelings of chastity, modesty, etc. He combats with right Westermarck's doctrine that fear and hatred of incest are instinctive. Parental care, sympathy and solicitude extend gradually to the members of the family and finally to the members of the community, guild and race. Thus the stoic cosmopolite is, in some respects at least, the acme of civilization. This feeling of sympathy, so helpful and advantageous, biologically speaking, is strongly evidenced in the multitudinous hospitals, asylums and similar institutions of this century. (Parenthetically it may be mentioned that Ohio in 1894 contributed \$1,146,721 to feed the paupers of the State and \$4,175,915 for all charitable purposes; the sum-total of the incomes of all the colleges and universities in the State amounted to less than \$1,000,000.)

The suggestive point is made that morality grows out of the family (perihestic), while public law springs out of the uniform usages outside the family (aphestic). Public law has its origin rather in the *hunger* tendencies of the race, in feuds, retaliation, arbitration, police necessities, etc. Thus public law never gave rise to any moral feeling, but moral feeling gave rise to corresponding public laws.

Since sympathy is emotional, an examination is made of the nature of the emotions and of their physical basis or concomitance. The James-Lange theory is presented, though worked out independently of them both. Mind is the continuous consciousness of sensations and emotions, the former arising from variations in sense organs, the latter from variations in the general vascular tone of the body. The biological necessity of fear, joy, etc., and their connections with the action of the vascular system are pointed out.

In criticism several points may with advantage be raised. In the love and hunger series of tendencies the implication is that if one (sympathy) is moral the other is either immoral or non-moral. The judgment of the race affirms that both are necessary, useful, good and moral when they are conducive to the race in general. That is the biological test of the ages. The speculative theories of the past were brilliant guesses. As a general rule they mixed up gloriously the ought with the actual. The motive, for example, does not justify and make an action moral unless it is conducive to the greatest good of that particular species or of the race. Neither is the hedonistic doctrine any better. The test of the age is not is he happy or is he not. That is not what history and science call the fittest in survival. The fittest has been of various kinds and forms. At one time it is lying, stealing, might, courage, at another their opposites. At one time it is intellectual in its nature (Kidd's Social Evolution), at another emo-

tional and at another volitional. Moral (human) conduct is that sum of habitual human actions which are conducive to the general good. This is no speculation, but the general test of the ages. Ethics as a science deals with the 'oughts' of history, their rise and fall, the causes of their success and failure. It deals with uniformities in the actions of men, their causes and probable continuance. As such it is a part of psychology and rests ultimately upon biological principles.

The measure of morality is the measure of fitness and accuracy of adaptation or attention. It is not a question as to whether a certain action produces pain or pleasure—upon that subject the success and progress of the world have not hinged—nor is it a question as to complexity, coördination or apparent goodness in itself of an action—for a jewelled watch is of no value or 'no good' in sunless obscurity; but it is an important issue as to whether an action has been of some value or of some advantage to the needs of the environment, present or future. Appropriation of property may be painful or pleasurable; it is in itself, however, neither right nor wrong. When appropriated by the State, whether from a willing or an unwilling subject, it is considered a good act on this condition, that it is for the greatest good of the greatest number. Appropriation of property for an evil purpose is theft. *Suum cuique* is a false definition of justice.

"Thou shalt not kill" either thy neighbor or thyself. But we praise the action of a Winkelried or a Christ, who marched to voluntary deaths. The man whose voluntary death is of disadvantage to his family and to the community at large we call a suicide. Moreover, that which was useful once may not be useful now. The tiger's fierce passions may be hurtful possessions to-day. In short, morality is measured and always has been measured in the great world's perspective by the degree of advantageous adaptation. Ethics as a science leaves the narrow anthropocentric attitude of Kant and the Hedonists and assumes the wider view of an objective, biological standpoint.

The author has, I believe, failed to analyze this important point, which has such a vital connection with his subject-matter. He says, "Right conduct arises from the moral instinct, after due allowance has been made for the reasonable exercise of the self-preserving instinct," p. 19. Again, "As a rule, moral conduct is right conduct; in other words, our moral or sympathetic instincts in general impel us to what is for the good of our race as a whole, but not always," p. 18. That is to say, Mr. Sutherland does not fully identify moral conduct with right conduct. Sympathy (his morality) does not constitute in his opinion the highest and truest test of right conduct. This point

certainly needs more careful analysis. Furthermore, the hunger side of our tendencies, or as Benjamin Kidd in his much and rightly abused book would say, the rational or intellectual side of our nature, should not be burdened with the evilly significant term 'selfish.' The modern world of inventions and culture can ill afford to endure a speculative philosopher's assertion that such are selfish, or are not conducive to the good of the race.

Mr. Sutherland fails again to make a clear and firm analysis of the terms mentioned in the following extracts: "that moral instinct which, with its concomitant intelligence, forms the noblest feature as yet visible on this ancient earth of ours" (p. 1); "the moral instinct with all its accompanying accessories, the sense of duty, the feeling of self-respect, the enthusiasm of both the tender and the manly ideal of ethic beauty" (p. 2). One of the clearest expositions the writer has seen of these phenomena is that of Dr. J. H. Leuba's, 'The Psychophysiology of the Moral Imperative' in the *American Journal of Psychology*, Vol. VIII., No. 4.

In a word, the author has given us an excellent treatise on the rise and growth of sympathetic reflex actions. The closing words of his introductory remarks are significant: "Hence the moral instinct is not an instinct of right conduct, a thing which has no existence, but an instinct, mainly sympathetic, which we find it conducive to man's highest good to encourage, by giving to the actions which it prompts the approving name of right conduct" (p. 19).

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*The Instincts and Habits of the Solitary Wasps.* GEORGE W. PECKHAM and ELIZABETH G. PECKHAM. Madison, Wis., Published by the State. 1898. Pp. 245. With Plates.

This work, as the record of careful and patient observation of forty-five species of solitary wasps, is of considerable interest to the comparative psychologist. The authors find these wasps to have large variability, individuality and intelligence. "The social hymenoptera are born into a community, and their mental processes may be modified and assisted by education and imitation, but the solitary wasp (with rare exceptions) comes into the world absolutely alone. It has no knowledge of its progenitors, which have perished long before, and no relations with others of its kind. It must then depend entirely upon its inherited instincts to determine the form of its activities, and although these instincts are much more flexible than has been generally sup-



posed, and are often modified by individual judgment and experience, they are still so complex and remarkable as to offer a wide field for study and speculation." The remark about absence of imitation rather conflicts with page 228, where the imitative acts are made a distinct variety. The most striking observation on intelligence is that of one *Ammophila* (p. 23) which picked up a pebble in her mandibles and used it to hammer the earth smooth over her nest. This seemed a case of improvising a tool and making an intelligent use of it. The account is corroborated by a report from an independent observer. Another remarkable instance of intelligence was the hanging of spiders in plants to keep them out of the way of ants, which was noted in several individuals. The comparative activity (pp. 150, 158) is also notable. The luring (p. 115) may have been either self-deception or confusion.

The authors' studies do not uphold the idea as to the wonderful stinging instinct by which the wasp is supposed always to hit the nerve center of spiders, etc., in such a way as to produce permanent paralysis, and so give fresh food to its future offspring. They show that the results of stinging are extremely variable, and that larvæ subsist healthily upon dead material. The authors do not make it clear how far the wasp consciously uses its sting as subjugating weapon, aiming at large and vital parts, or whether it be mere imperfect instinct (cf. pp. 203, 227, 232). The writers find no sense of direction in wasps, but their numerous observations show that wasps carefully study localities and note landmarks, and yet frequently lose their nests. (Yet here again there is not complete consistency, cf. pp. 60 and 8.) The psychological analysis is not as clear and thorough as we could wish. There is too much careless writing of this sort: "Just here must be told the story of one little wasp whose individuality stands out in our minds more distinctly than that of any of the others. We remember her as the most fastidious and perfect little worker of the whole season, so nice was she in her adaptation of means to ends, so busy and contented in her labor of love, and so pretty in her pride over her contemplated work" (p. 22). Our general impression from this work is that the solitary wasps, while obeying general instinctive impulses, as stinging, nesting, etc., are far more largely than is generally supposed guided by intelligence in the specific applications. It is to be hoped that the authors pursue their studies, especially in the way of experiment, and extend their work to the social wasps on the lines intimated on page 68. They might also tell us whether wasps numerate, play, and what emotions they have.

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*The Essence of Revenge.* DR. E. WESTERMARCK. Mind, July, 1898.

Dr. E. Westermarck opens this article by a sharp criticism of Dr. Steinmetz's theory that revenge is essentially a mode of enhancing the self-feeling, and in its earliest form is undirected, falling upon any convenient victim rather than the real aggressor. He points out that the instances of undirected revenge adduced by Steinmetz are irrelevant or tend the other way, and he thinks that the so-called instance of undirected revenge is merely either "sudden anger or it is the outburst of a wounded 'self-feeling,' which, when not directed against its proper object, can afford only an inadequate consolation to a revengeful man." We may observe on this matter that an oriental despot in massacring a host of non-aggressors merely to enhance his power and dignity is plainly not moved by revenge. However, the running amuck by a Malay, or the assassination of an innocent Empress by an anarchist, may be generally directed revenge and misdirected, but not undirected revenge. Certainly Dr. Westermarck's contention seems sound, namely, that revenge is not at first indiscriminating, not a purely subjective exaltation of selfhood which bears no definite action and feeling toward aggressor as such. To our modern individualism much savage revenge must appear indiscriminating when it really is directed to what it judges collective responsibility.

After remarking on cases of revenge among animals which indicate discrimination, and then touching on the close connection of anger and revenge, Dr. Westermarck briefly indicates the function of revenge in self-conservation and self-furtherance. It would, of course, require a volume to treat properly the natural history of revenge, its culmination as life factor, and its decadence in its primitive forms in the highest civilization. The duel is survival form, and I think the admiration shown for a murderer by many women in civilization probably is survival of the ancient savage feminine admiration of the most successful head-hunter. On the other hand, Britain's vengefulness protects her citizens in all parts of the world, and 'Revenge the Maine!' was lately the war-cry of a great nation. Dr. Westermarck agrees with Dr. Steinmetz that strict equivalence is not a general law of revenge. He instances Hannibal slaying 3,000 captives in revenge for the death of his grandfather, but I doubt that Hannibal would acknowledge injustice. Revenge gets even from its own point of view. Dr. Westermarck distinguishes equivalence as qualitative and quantitative, and ascribes qualitative—"the paying back in his own coin"—mainly to 'wounded pride,' but maintains that quantita-

tive is determined by public opinion. "If the offender is one with whose feelings men naturally sympathize this sympathy will keep the desire to see him punished within certain limits, and if they sympathize equally with the suffering of the offender and with that of his victim they will demand a punishment only equal to the offense. This demand, in combination with the rough ideas natural to an uncultured mind that offense and punishment are to be measured by their external aspects, lies at the foundation of the strict rule of equivalence, which is thus an expression, not of an unrestrained barbarism, but of advancement in humanity and civilization. If this explanation be the correct one the rule in question must have been originally restricted to offenses committed by fellow-tribesmen, as public opinion could not otherwise have been an impartial judge." However, is it not the natural tendency of revenge to hurt the other as bad in the same kind? And does not the regulation of revenge proceed mainly from caution, both of the revenger, who fears counter reprisal, and of his kin and friends, who have to undertake any revenge for him? No one but an absolute despot can afford to retaliate every aggression with death. In savage life an individual who always sets out on death-dealing revenge would get short shrift himself; and thus natural selection weeds out both the over-revengeful and the under-revengeful, and establishes a tit-for-tat, which comes to be natural justice, as the simplest law of socialization. Dr. Westermarck intimates and asserts that sympathy of companions as regulating revenge moulds moral consciousness, but he does not develop with any clearness or fullness a most interesting point which lies out of the direct scope of his paper. Certainly, a simpler solution of the duty-consciousness lies in the revenge become not merely impulsive, but compulsive, at urging of wife, children and relatives, a duty to be done.

With some remarks on intention in revenge and on the comparative method of study, Dr. Westermarck closes a paper which certainly advances our knowledge of the subject, but which on some points is more suggestive than conclusive.

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*An Introductory Logic.* JAMES EDWIN CREIGHTON. New York, The Macmillan Company. 1898. Pp. vii+387.

In this volume Professor Creighton has presented very clearly the essentials of the traditional logic, which at the same time he has supplemented with an admirable statement of the salient features of the



modern logic. An introductory outline of his treatment of the subject is followed by a brief historical sketch which serves to indicate the several phases through which logic in its development has passed. This gives to the student at the very beginning a point of view and a general conception of the scope of the subject. It also tends to stimulate his interest in the following portions of the book. The author divides the subject-matter into three parts: the first treats of the deductive logic; the second, of the inductive; and the third, of the general theory of logic.

The first part begins with a general account of the syllogism, reserving, however, for a subsequent chapter the detailed discussion concerning the specific rules of the syllogism and their application. This procedure seems to me to be a very happy one, as it opens up the subject at that point which is most familiar to those who have never studied logic, and yet who, in a vague way at least, have some notion of the inferential processes which, to their minds, it may be, are almost, if not altogether, synonymous with logic itself. Thus starting on somewhat familiar ground, this much is gained, that the student is not at the outset deterred by the array of definitions which in most text-books on formal logic seem to block the way to the heart of the subject. Though it may not be as strictly logical a procedure, yet for pedagogical reasons it is better to begin at the center with some idea of the meaning of inference, and thence work towards the elementary material which environs it. Throughout the treatment of the deductive logic there is a clearness of statement, and also a due sense of proportion in passing over, with but a brief mention, such portions as have historical rather than present interest either of a speculative or of a practical nature, as, for instance, the subject of reduction. Professor Creighton's illustrations and examples are not of the conventional order, and, therefore, they do not leave the impression which traditional examples in logic so often do leave, namely, that syllogistic reasoning has no counterpart in the actual inferences of everyday life.

In the author's exposition of the inductive logic we find that the practical procedure in inductive investigation is kept prominently in view, and the various methods of research are clearly explained in the concrete by giving a number of appropriate examples of actual experiments whose results have materially augmented the wealth of scientific knowledge.

Professor Creighton's classification of the inductive methods seems to me to be somewhat at fault. He divides the methods into

those of observation and those of explanation; the former embrace the methods of enumeration, of statistics and of causal determination; and the latter, the methods of explanation, are chiefly the methods of analogy and hypothesis. My criticism of this division is that causal determination is in itself an explanation. It is true that the causal relation is generally further analyzed or simplified by analogy or by some hypothesis, but for the most part the hypothesis or analogy explains by referring to some underlying causal connection. It is due to Professor Creighton to state that he himself acknowledges that in this classification the difference between the two kinds of methods is one of degree rather than of essential nature<sup>1</sup> and yet it perhaps would have been better not to make a distinction which is not one of essential nature the basis for differentiating so prominently these methods of research.

In the third section, which treats of the nature of thought, or the general theory of logic, the author's point of view is in its essential features quite in accord with that of Bosanquet. Professor Creighton insists upon the conception of thought as a living, growing phenomenon, and not a mere mechanical grouping of ready-made ideas, and also that the growth of thought is to be regarded as a process of development which proceeds ever from simpler to more complex states, according to the manner of all evolutionary processes. He regards the judgment as the unit of thought, and defines the concept as 'the series of judgments which have already been made.'<sup>2</sup> By way of comment upon this definition of concept, he adds that "to make the thought our own, to gain the real concept, it is necessary to draw out or realize to ourselves the actual set of judgments for which the word is but the shorthand expression."<sup>3</sup> The concept, however, is not merely a summation of a number of judgments. It is rather a blending of the various elements which the several judgments have furnished in such a way that these judgments which have been operative in the formation of the concept are implicitly rather than explicitly apprehended. For there is such a thing as an implicit apprehension of the significance of a whole without a conscious analysis of its component parts, as Mr. Stout has so admirably set forth in his *Analytic Psychology*. The function of the concept is essentially 'adjectival' until it is subjected to an analysis which discloses explicitly the parts which form the 'actual set of judgments' but which before such analysis were discerned implicitly.

Professor Creighton has stoutly defended the necessity of a uni-

<sup>1</sup> P. 220.

<sup>2</sup> P. 270.

<sup>3</sup> P. 271.

versal element in some form or other as the ground of inference. In this view he takes exception to Mill's contention that reasoning is from particular to particular. The author very happily sums up the argument in support of his position in the following sentences, which clearly indicate his general point of view as regards the theory of inference: "Knowledge sees the universal in the particular, or reads the particular as a case of the universal. And when thus interpreted the particular ceases to be a bare particular and becomes an individual with a permanent nature of its own. When one reasons from an individual case, then it is the universal or typical nature, not the particular or momentary existence, upon which the inference proceeds. If there were any merely particular facts in knowledge we could never reason from them. But the so-called particular facts, as elements of knowledge, possess a universal or typical aspect in virtue of which alone inference is possible."<sup>1</sup>

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*L'Éducation Rationnelle de la Volonté: Son emploi thérapeutique.*

PAUL ÉMILE LÉVY. Préface de M. le DR. BERNHEIM. Paris, Alcan. 1898. Pp. v + 234.

Dr. Lévy divides his monograph into two sections, a 'Theoretical Study' and 'Practical Applications.'

From the theoretical point of view, psychotherapy is based upon one fundamental principle: "Thought is in act in the nascent state; it is a beginning of activity. \* \* \* The transformation of the idea into an act may take place in two distinct ways. Either the idea becomes a *positive* act, *i. e.*, feeling, volition, sensation, movement, or it becomes a *negative* act—in other words, it neutralizes the act, prevents the feeling, the volition, the sensation, the movement from being realized (*se produire*). Dynamogeny, inhibition—these are, briefly, two aspects of the same process." (Pp. 13, 14.)

Granting this principle, all that is needed to bring about any given change is to awaken in the mind an idea of the change in question, *i. e.*, to give a suggestion. The suggestion may be administered by another (81-97) (hetero-suggestion), or by the patient himself (auto-suggestion). It is with the latter only that Dr. Lévy concerns himself. To make the auto-suggestion most effective it should be administered in a state of mental repose (49) (*recueillement*), and should be reinforced, when possible, by directing upon it any available emotion

<sup>1</sup>P. 344.



(55), and by the deliberate adoption of such conduct as the suggestion would itself, if realized, inspire (63-79) (*entrainement actif*). The suggestion should not be in the form of an effort to will its realization; it should rather be put as a categorical statement (52), but voluntary attention should be concentrated upon it to ensure its taking root (32) (52, note) (128).<sup>4</sup>

The same principle may be applied to the realization of a highly abstract ideal (Mental Hygiene, 99-120). By fixing the concept of the ideal in mind as an auto-suggestion its influence will be felt at unexpected moments, modifying the details of conduct.

By *will* we mean nothing more than the resultant (125), as expressed in conduct, of all the active tendencies of simultaneously co-existing ideas, but it must be remembered that a small part only of these (126) are conscious, the larger part being subconscious. The weakness of will which prevails in modern society (French society?) is due to the fact that the intellectual, social and political upheavals of the last century have undermined the definite convictions which ruled the conduct of our ancestors and have given us nothing in their place (121-125). It is obvious (127 ff.) that the theory of auto-suggestion, supplemented by hetero-suggestion, is the only scientific method of training the will and developing character.

Such is Dr. Lévy's theory. It embraces much that is true—although nothing new—but it suffers from a lack of what may be termed the quantitative sense, and an indifference to the niceties of conceptional discrimination. To take the last first, surely it is only by doing violence to the plain meaning of words that the tendency spontaneously to adopt the beliefs of others, the tendency on the part of certain ideas to give rise to the corresponding sensation, and on the part of other ideas to give rise to a movement, are all subsumed under the concept of 'nascent act,' which is strictly applicable to the last only of the three. There is an analogy between these three tendencies, but they are far from identical.

The quantitative sense is the ability to discriminate tendencies from necessary causal relations, possibilities from probabilities, and this Dr. Lévy seems to lack. The facts now in hand go far towards rendering plausible the hypothesis that every mental state possesses—perhaps vicariously, as the representative of cortical process—certain intrinsic tendencies, which, when the inhibition of other states is removed, would result in such determinate events as the development of an illusion, the suggestion of an associated idea, the production of a movement, the modification of metabolism. But to base upon such a

doctrine the elaborate system of practice proposed by Dr. Lévy and to hold forth such confident hopes to those who stand in need of aid is a very different matter. Such a leap should be taken only after the practicability of the proposed method has been demonstrated. What has Dr. Lévy done towards demonstrating it?

He has conducted experiments for four (218) months upon himself and six other persons. We are told nothing of the physical or mental traits of these other persons, although it would appear (p. 185) that one at least, 'C,' was an advanced hysteric. Such details are quite irrelevant to the question, Dr. Lévy thinks (217). Nor are we told anything of their reliability as witnesses; merely that (145, note) each one must have known whether the results he reports were due to suggestion or to coincidence. We are, however, told that no one of them received more than ten or fifteen minutes' instruction from Dr. Lévy (218) as to the method of experimenting and of recording experiments. No record whatever is made of the negative results, though we are told that two subjects, whether two of the above mentioned six or not is not stated, could get no positive results (219). Of the positive results some are given. A few illustrations taken at random will give an idea of their character.

"One of my subjects, a merchant, frequently, before going to see his customers, gave himself the suggestion to succeed in the business which he had to do with them. Often, he said, the suggestion was followed by the desired result" (162). This is due, Dr. Lévy thinks, to the greater self-confidence and persuasiveness imparted by the suggestion.

Or an observation of Coste de Lagrave's—"I sing badly; I resort to the auto-suggestion that I should sing with taste, should give pleasure to the audience; I receive compliments for the first time in my life" (162).

"I try to concentrate my mind upon my work alone, to the exclusion of all else in life. I note at the end of the afternoon that my time has been much better spent than usual" (160).

"After having grippe, though a light attack, I used to lie thenceforward quite a while before I could go to sleep, and woke two or three times every night. These disturbances lasted about a week; in about the same time I succeeded in making them disappear by auto-suggestion" (172).

"Trembling of the right hand had lasted about two months when the patient thought of treating it by auto-suggestion. Was improved after several attempts, then definitely cured in a period of time the duration of which is not clearly marked" (189).

“Very violent headache, only in temple and forehead, with throbs. Cure very rapid by auto-suggestion” (190).

Severe itching in three middle fingers of right hand, lasting a week, cured in four or five days by auto-suggestion (201).

“Being dyspeptic and having no appetite I give myself the suggestion to be hungry. At the next meal I really eat with very good appetite” (206).

A few of Dr. Lévy's cases are better than these, but the great majority are marked by the same disregard of essential details, and the same reckless ascription of the result to the suggestion, when it might obviously have been due to some totally different cause, which makes these cases worthless.

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*L'Absolu et sa loi constitutive.* CYRILLE BLONDEAU. Paris, Alcan. 1897. Pp. xxv + 350.

The ‘Absolute’ is the immutable universe in which things and thoughts are alike confounded. ‘The higher law from which results nature or life’ is formulated as follows: “The mutual relation of the elements which constitute a body is in inverse ratio to the relation of these elements to the environment which contains the body” (p. 344). This is put forth, in all seriousness, as a revelation of the world's ‘most secret resources.’

The only thing in the book of any special interest to the psychologist is the author's conception of the relation of physiological process to the facts of consciousness. He speaks of this as a discovery of ‘the passage from physiology to psychology’ and regards it as an achievement of cardinal importance in his discussion (p. xxi), enabling the intelligence ‘to radiate freely in the world without finding any further obstacle to its infinite expansion’ (p. 114). What then is the fulfilment of this profession? First, a reassertion of a solution of continuity between physiology and psychology; secondly, the assertion of a similar abyss between consciousness made up of sensations and images and the pure reflecting activity of the mind; thirdly, as explanatory theory, the assertion of relatively free forces at different levels in the nervous system by virtue of which sensations are, on the one hand, perceived and, on the other, reflected on. “The sensations and images are perceived by the free force most directly related to the force subject to the external and internal action; the parts more indirectly related to the sense organs are now, in virtue of their saturation



and the law of equilibrium, able to exercise themselves on those which furnish the sensations and images" (p. 129). "Thus the inner world is broken up into two parts: one belongs provisionally to the thing whence the action emanates, the other remains outside, and it is solely by this part of the inner force not absorbed, free, therefore, relatively to that particular connection, that it is permitted to perceive the sensation, which is thus, in principle, nothing but a relation of inner to outer" (p. 132). As a purely physiological hypothesis much can be said in favor of the conception of lower- and higher-level centers with which related but relatively independent stores of disposable energy are incorporated, corresponding respectively to lower and higher processes of consciousness; but to see in such an hypothesis—which, moreover, is not new—"the passage from physiology to psychology" requires surely more than ordinary clairvoyance.

The book abounds in apothegm and paradox ('consciousness contradicts reason,' 'the love of truth is not reasonable,' etc.), and is not wholly free from contradiction ('one must be convinced of human liberty relatively to the absolute,' p. 208; 'from the absolute point of view there is no liberty,' p. 69; cf. p. 211, 'consciousness is only free relatively to things'). Its pretensions are preposterous (*e. g.*, p. xix), its style insipid, its construction loose, like the jottings of a note-book. There is much by the way to stimulate reflection, but in the end epigram on the absolute and its constitutive law through nearly four hundred closely printed pages becomes not a little tedious.

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*Ueber die Messung der Auffassungsfähigkeit.* LUDWIG CRON und EMIL KRAEPELIN. Separat Abdruck aus: Kraepelin, *Psycholog. Arbeiten*. II. Bd. 2 Heft. Pp. 203-325. Leipzig, 1897.

This is an account of experiments conducted primarily to determine the influence which the peculiarities of the individual exert upon the form of the apperceptive faculty and to discover the common factors involved in the act of apprehending. Subjects were kept at the continuous apprehension of printed words and an attempt made to estimate the precision and reliability of the performance. For this purpose the region of the threshold was used, *i. e.*, stimuli were presented to the eye only for so long a time that they could be perceived in a number of cases but not always distinctly. The number of correctly read stimuli furnished a measure for the ability of apprehending, while from the incorrect readings deductions could be made as to the

action of error processes. The experiment consisted essentially of three drums on which were pasted, in spiral form, (1) nonsense syllables, (2) one-syllable words, (3) two-syllable words. The drums revolved at a constant speed of 24 mm. per second, and were observed through an opening or slit in a screen from a fixed distance. The observation slit, 5 mm. in height, was regulated in width by means of a micrometer screw, so that the time during which the stimulus was visible could be varied. Three different widths were used, 5, 4 and 3 mm. By means of a contact arrangement, which noted the entrance of each letter into the field of vision and its disappearance, the duration of visibility could be exactly measured. Such measurements gave for the three different slit widths, 290, 230 and 170 thousandths of a second, respectively. Each subject endeavored to read aloud the words as they passed the opening of the screen and the correct readings, errors and omissions were noted down in shorthand by the experimenter. The same experiment was performed on three different days. There were six subjects, three normal and three pathological cases from a clinic.

The results show surprising differences in capacity to apprehend the matter presented. Certain facts and principles, however, were common to all.

Results of experiments with nonsense syllables show that the greater part of the syllables were correctly read, the number decreasing with the narrowing of the observation slit, yet more slowly than the visibility. On shortening the time of exposure one-fifth, correct readings showed only an insignificant decrease. In the normal subjects at 4 mm. the limit was very nearly reached beyond which a wider opening could effect little improvement. As the time of exposure was shortened, omissions became decidedly more numerous, while errors showed only a moderate increase, omissions being evidently the measure of the difficulty of apprehension. Errors in which only one letter was misread predominated. The first letter was by far the most frequently misread, the last next; the middle letter, least of all, indicating that the attention was directed especially to the middle so that the syllable was taken as a unity. The difference in apprehending the first and the last letter is ascribed by the authors to incomplete cessation of attention. The single syllables here follow one another every  $1\frac{1}{4}$  sec., but 2 sec. is the most favorable space of time between signal and stimulus for the complete cessation of attention. This would hinder the apprehension of the first letter to a greater degree than the last.

*One-syllable words.* Results show a greater facility in apprehending than for nonsense syllables, notwithstanding the greater number of letters to a word. The word-idea present in the process of apprehension is assigned as the cause. The number of correct readings sank 10% with the narrowing of the observation opening from 5 mm. to 3 mm., a smaller diminution than resulted in the case of nonsense syllable under like conditions. Single-letter errors are most numerous, two-letter errors only slightly less. The initial letter is almost always relatively well recognized here because a capital and so attracting the attention. The second letter is generally indistinctly apprehended, the third favored, the fourth neglected, indicating a two-fold rhythm of apprehension. The number of nonsense misreadings out of the total misreadings varies in the different subjects from 6% to 31%. From the number of nonsense misreadings deductions are made as to the influence of word-ideas. Such influence is maintained to be greater the smaller the number of nonsense misreadings. Two classes of misreadings are made, namely, those which occur in the same word again, and those which are repeated in different words. In the former an error once committed becomes fixed, while in the latter an idea has acquired such power that it moves the thoughts in a certain direction. This is indicated by such errors as Heft for Kost, Heil and Geist; Stadt for Sold, Stoltz, Druck, Stift, Saar, Staub, Wohl and Stern, where little connection is apparent between stimulus and error, but evidently an impulse present to express an idea whose content is from another source.

*Two-syllable words.* In apprehending two-syllable words two subjects showed a better record than in the two previous experiments; in others, however, there was a falling-off showing itself in the number of omissions, the number of errors remaining about the same. According to the view previously noted this would indicate greater difficulty in recognizing words with less liability to error. The apprehension here, unlike that of shorter words, takes place by spelling, giving little opportunity for conjecture and influence of ideas, hence greater accuracy. The initial letter is again distinctly favored. The rhythm of apprehension which was earlier expressed only in two subjects appeared very distinctly in others. One subject appeared to recognize best the first and fifth letters, indicating a grouping of greater extensity. The same misreadings were not so frequently repeated in different words as in the case of one-syllable words.

As a rule the number of correct readings increased from day to day. The improvement from the first to the second day is much more



pronounced than for the second to the third day. This the authors attribute to the influence of habit rather than to facility gained by practice, for the former exerted in the control of disturbing processes reaches its height under such conditions quicker than the latter. Practice quite generally in the experiments shortens the time of perception and, according as the inclination is to misreadings or omission, causes a decrease of the former or latter. Fatigue, which appears to stand in close relation to practice, develops the opposite effects, yet, on account of the changing conditions of work and the short time of the experiment, its effect was limited.

From the results obtained conclusions are drawn as to the different influences which determine the formation of the process of apprehension. Such influences are: (1) quickness of perception, which determines the distinctness of the impressions; (2) articulation of apprehension, which determines the clearness of the constituent parts of the impression; (3) sensuous precision of perception and influence of ideas, which affect the reliableness of apprehension; (4) the more or less striving or effort to make the best possible record, which leads to the suppression of readings felt to be erroneous; (5) practice and fatigue; (6) memory, noticeable in the frequent return of the same misreading in the same word.

The combined effect of these conditions occurring with different strength in the individual observers determines the aptness of apprehension. In closing, a characterization of the different subjects is made on the basis of the proportion and combination of those influences appearing in the investigation.

E. C. JONES.

YALE UNIVERSITY.

*Aussenwelt und Innenwelt, Leib und Seele.* JOHANNES REHMKE.

Rektorats rede Univ., Greifswald. Greifswald, 1898. Pp. 48.

*Zur Parallelismusfrage.* G. HEYMANS. Zeitschrift für Psychologie, Bd. XVII., Heft, I.-II., S. 62.

*Die erkenntnistheoretische Stellung des Psychologen.* RUD.

WEINMANN. Zeitschrift für Psychologie, Bd. XVII., Heft 3, 4, S. 215.

Professor Rehmke has given us, in his inaugural address of last spring, what is designed as an exhaustive statement of the possibilities of general theory touching the relation of mind and body, and a decision on logical grounds in favor of interaction. In a preliminary historical sketch his aim is to show in their simplicity the logical motives that have forced the development of theory. In his direct treat-

ment of the problem the distinction between the thing or (more generally) the existence, and its properties, furnishes the basis. Ancient materialism regarded the mind as a thing, modern materialism as the property or peculiarity of a thing—the body or brain. The Platonic teaching and the author regard it not as a thing but as a separate immaterial existence. Solipsism regards the body as a property or peculiarity of the mind. Spinozism (a name that Dr. Rehmke gives to modern parallelism in general) accounts mind and body as equally properties or peculiarities of a substance which manifests itself in them. Experience tells us that mind and body are connected. Experience tells us also that the connection of ‘things’ is causal. If mind and body are both ‘things,’ they interact. But the mind is not a thing; the grounds need not be here repeated. In that case, it is held, it cannot be a party to interaction, for only natures alike in kind can interact. How then shall we understand the evident connection? Shall we say with modern materialism that the body (or brain) is the thing and the mind the property? But in no case, as here, can a thing be to all appearance fully conceived without its property, and its property without the thing, and against this as against the view that the body is a mere property of the mind the testimony of experience to the distinctness of mind and body in our world of reality is final. Are the two connected then as properties of a third somewhat? The Spinozistic doctrine merely repeats the fallacy of Occasionalism; it resorts to an alien substance to do what it has pronounced impossible. Experience does not show us a quality changing punctually of itself whenever another quality of the same existence changes. The connection of quality with quality must then on this view be causal. But two qualities of the same existence do not stand in causal relation to each other, nor an existence in causal relation to its qualities. We must recognize the following pair of first principles: (1) Only a separate existence suffers change; qualities cannot suffer it. (2) An existence suffers change only when there is another existence involved as the active condition of the change. (Reference to author’s *Lehrbuch d. Psych.*, S. 41–45, and *Zeits. f. imm. Phil.*, Bd. II., S. 349 ff.). Mind and body are accordingly separate existences interacting. The rule that all interacting existences must be alike in kind is a hasty generalization from the fact that interacting *things* are alike in kind. But is not the quantity of motion (Dr. Rehmke rejects the term ‘energy’ as vague) unalterable? How then can non-physical things produce it? The answer is that the law, which is universal, applies only where thing moves thing, not where a non-physical existence causes a

thing to gain or lose motion. How far, if at all, the brain loses motion when the mind is affected, and the mind parts with power (in any sense) when the brain is affected, we are not yet in a position to determine.

Dr. Rehmke's subtle and dexterous treatment of this much agitated question may be described as scholastic, if relentless logical analysis entirely without psychological analysis of the terms is the typical scholastic tendency. The argument seems to depend wholly on the finality and exhaustiveness of the distinction, as applied for instance to the mind, between a 'separate existence' and its 'properties.' It is curious to see so assured an idealist as Dr. Rehmke so confident of this. His remarks on parallelism, in part fruitfully suggestive, are vitiated by his confounding the modern theory in all its forms with Spinozism. Were Clifford and Fechner (themselves far enough apart) believers in a 'substance' of which mind and body were the 'properties'? Moreover, when the author speaks of the hypothesis that when a pin produces a wound, and through it a cerebral discharge, the pin's psychic counterpart produces the psychic counterpart of the bodily disturbances and then the pain accompanying the cerebral discharge, as a fancy too mystical to be entertained by the modern parallelist, he is tossing aside what many regard as of the essence of their principle. To substitute 'the conservation of motion' for that of energy, as though the former would be acknowledged tantamount to the latter, or more generally regarded as true, is a serious error. It should be remarked, too, that an author who uses the term 'the solipsists' to designate one of the schools of philosophical opinion should inform us who these thinkers may be and where they are to be found.

The best comment on the artificial method of Dr. Rehmke's essay is furnished by the second on our list; the article of a fellow-idealist defending 'monastic' parallelism from the attacks of Professor Stumpf and more recent writers. Dr. Heymans calls attention to many misunderstandings of the theory, rejects Spinoza's substance, and in a lengthened exposition explains that it is only from the convenient point of view of the human mind, which makes an independent enduring world of its own perceptions, instead of conceiving the world in the true terms of outer mind-stuff, and so doubles the facts, that there is any parallelism at all. The remainder of his article consists of acute replies to the recent assailants of the theory. The objections to it on the score of evidence he does not fully face. This essay is the most closely and carefully reasoned of the three before us.

The article on the epistemological position of the psychologist is



a vigorous defence of realism by psychological arguments. The author advances to battle with a light heart, and lays about him with boyish confidence against the whole host of contemporary German phenomenists, Schuppe, Schubert-Soldern, Kaufmann, Mach, Rehmke, Leclair, Laas, Cornelius, Avenarius; that is to say, the school of 'immanente Philosophie,' the strict sect of Avenarius, and certain detached kinsfolk of these. "The psychological compulsion which drives us to apprehend that which we are conscious of, which we experience, as objective, is no mere jest which our Psyche perpetrates, but an instinctive indication of the view to which the purely philosophical and logical consideration of the matter of experience by rightful consequence leads." Various departments of psychology are drawn upon to show the underlying realistic assumption of the science. Much is made of the argument that idealism does away with the distinction between psychology and other sciences. It can hardly be said that the article exhibits a delicate sense of the besetting difficulties of the long-vexed problem or a complete grasp of the opinions it condemns. It is a philosophical instance of the illusion of simplicity.

D. A. MILLER.

PHILADELPHIA.

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## NEW BOOKS.

*Social Elements.* C. R. HENDERSON. New York, Scribners. 1898.  
Pp. ix + 405.

*Le Rôle Social de la Femme.* Mme. A. LAMPÉRIÈRE. Paris,  
Alcan. 1898. Pp. 175. Fr. 2.75.

*La Psicologia contemporanea.* GUIDO VILLA. Bibliot. di Scienze  
Moderne. Turin, Bocca. 1899. Pp. 660. Lire 14.

*Report of U. S. Commissioner of Education, 1896-7.* W. F.  
HARRIS, Vol. 2. Washington, Govt. Press. 1898. Pp. vii and  
1137-2390.

*L'Instabilité mentale.* G. L. DUPRAT. Paris, Alcan. 1898. Pp.  
310.

*Wild Animals I have Known.* E. S. THOMPSON. New York,  
Scribners. 1898. Pp. 358. \$2.50.

A delightful series of chapters on animal genius-heroes. Mr. Thompson ought to tell us more explicitly, however, just what incidents the psychologist may quote on his authority as a naturalist!

J. M. B.

*Democracy. A Study of Government.* J. H. HYSLOP. New York, Scribners. 1899. Pp. xiv + 300.

*The First Philosophers of Greece.* A. FAIRBANKS. New York, Scribners. 1898. Pp. vii + 300. \$2.00.

*Footnotes to Evolution.* D. S. JORDAN and others. New York, Appletons. 1898. Pp. viii + 392.

*Leibnitz, The Monadology and other Philosophical Writings.* R. LATTA. Oxford, Clarendon Press. 1898. Pp. 437.

*Principles of Biology.* HERBERT SPENCER. Revised and enlarged edition. Vol. I. New York, Appletons. 1898. Pp. xii + 706.

*Die Spiele der Menschen.* KARL GROOS. Jena, Fischer. 1899. Pp. viii + 538.

*Christian Science. A Sociological Study.* C. A. L. REID. Cincinnati, McClelland. 1898. Pp. 32.

A spirited negative examination of Christian Science by a physician. His paper is printed by request of the Northwestern Ohio Medical Association, before which it was read.

*Double Personality. Lenten Hysteria.* W. L. HOWARD. Two papers reprinted from the Maryland Medical Journal. Pp. 4 and 4.

*Intensitätsschwankungen eben mekliche optischer und akustischer Eindrücke.* W. H. HEINRICH. Se.; Abd. aus Anz. d. Akac. d. Wiss. in Krakau. Nov., 1898. Pp. 365-381.

*The Scottish Contribution to Moral Philosophy.* J. SETH. Inaug. Lect. Univ. of Edinburgh. Edinburgh and London, Blackwood. 1898. Pp. 43. 6d.

*Ueber unsere gegenwärtige Kenntniss vom Ursprung des Menschen.* By E. HAECKEL. Bonn, Strauss. 1898.

*Ueber die Grundvoraussetzungen der individualistischen Weltanschauung.* By W. LUTOSLAWSKI. Helsingfors, J. Simèlii Erben. 1898. Pp. 88.

*The Message of the World's Religions.* Reprinted from *The Outlook*. New York, Longmans. 1898. Pp. 125.

A series of chapters giving the point of view of each of the great religions, *i. e.*, Judaism (Rabbi Gottheil), Buddhism (Ruys Davids), Confucianism (A. H. Smith), Mohammedanism (G. Washburn), Brahmanism (Professor Lanman), Christianity (Lyman Abbott). A remarkably interesting little book.

J. M. B.

*The Beginnings of Art.* E. GROSSE. New York, Appletons. 1897. Pp. xiv+327.

The German edition of this already well-known book was noticed at length in this REVIEW (III., 1896, p. 560). We need, therefore, only repeat our commendation of it, and strongly recommend the English version to psychologists.

J. M. B.

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## CORRESPONDENCE AND NOTES.

### PROPOSED CHANGES IN THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

At the recent meeting of the Association in New York, certain members proposed the formation of a separate section to be devoted to the reading and discussion of philosophical papers. On account of the small number present when the matter was brought up, decision was postponed until the next meeting, and the Secretary was requested to send out cards to all members asking for a general expression of opinion on the point.

Certainly, every member should have an opportunity to be heard with reference to a proposition which logically involves changes in the name and constitution of the Association, but a vote taken without discussion will fail to express the thoughtful wishes of the members. I venture to suggest that questions of sufficient importance to be referred to a vote of the whole Association ought to receive some attention in the pages of the REVIEW, and beg leave to restate some of the reasons why, in the interests of philosophy, as well as psychology, the proposed action seems unwise at the present time. Most of these reasons were mentioned in the discussion at the meeting.

First, judging from the experience of the programme committee, it would be difficult to arrange a programme for such a section without interfering with the regular meetings.

Second, our best psychologists are among our best philosophers, and their withdrawal from even a part of the meetings of the Association would be a serious loss. At the same time the greatest need of psychology at present is more of sound philosophy, and the greatest need of philosophy more of sound psychology. Closer union is more to be desired than further separation.

Third, philosophical papers are already welcome whenever they offer contributions to psychology or show the bearing of psychology



on problems of philosophy. This offers a wide range of subjects for those who are interested in any branch of philosophy, and such papers always form a part of our programme. So far as possible, they are grouped together in the same sessions.

Fourth, when it comes to the making of interesting programmes, philosophical subjects are by no means equal to scientific subjects. As a rule the papers are too long. Scientific theories and results can be stated briefly, but it takes time to set forth philosophical opinions. Such are not suitable subjects for general discussion, and discussion ought to be the most important feature of these meetings. There is no object in coming together to listen to papers which can be read at home. As a matter of fact, at our own meetings philosophical papers never called forth as much interest as the scientific, while attempts in other places to hold meetings for the exclusive discussion of problems in philosophy have repeatedly ended in failure.

Finally, the Association is now making splendid progress and is becoming a source of inspiration to workers in the field of psychology. At the same time it is doing a real and lasting service for philosophy in furthering the development of scientific spirit and methods in the realm of mental phenomena. Nevertheless much remains to be done before psychology comes into right relations with the rest of the sciences. There is need of all the wisdom and energy available to improve the character of our scientific work, and it is extremely important that no step be taken which will weaken the Association.

CHARLES B. BLISS.

(THE REVIEW is ready to print other concise opinions on this or other matters pertaining to the Association. J. M. B.)

MRS. LAURENCE HUTTON, whose interest in Miss Helen Keller is well known, allows us to print the following passage from a personal letter from Miss Keller to her, written under date of January 17, 1899. Psychologists will be interested in the passage, both because it is Miss Keller's and also because of the sentiment which her project embodies: "Have you seen Kipling's 'Dreaming True,' or 'Kitchener's School'?" It is a very strong poem, and has set me to dreaming too. Of course you have read about the 'Gordon Memorial College,' which the English people are to erect at Khartoum. While I was thinking over the blessings that would come to the people of Egypt through this college, and eventually to England herself, there came into my heart the strong desire that my own dear country should in a similar way convert the terrible loss of her brave sons on the 'Maine' into a

like blessing to the people of Cuba. Would a college at Havana not be the noblest and most enduring monument that could be raised to the brave men of the 'Maine,' as well as a source of infinite good to all concerned? Imagine entering the Havana harbor, and having the pier, where the 'Maine' was anchored on that dreadful night when she was so mysteriously destroyed, pointed out to you, and being told that the great, beautiful building overlooking the spot was the 'Maine Memorial College,' erected by the American people, and having for its object the education both of Cubans and Spaniards! What a glorious triumph such a monument would be of the best and highest instincts of a Christian nation! In it there would be no suggestion of hatred or revenge, nor a trace of the old-time belief that might makes right. On the other hand, it would be a pledge to the world that we intend to stand by our declaration of war, and give Cuba to the Cubans, as soon as we have fitted them to assume the duties and responsibilities of a self-governing people."

J. M. B.

G. A. TAWNEY, Ph.D., has been promoted from Assistant to full Professor of Philosophy in Beloit College, Wis.

G. F. STOUT, editor of *Mind*, has been called from Aberdeen to the new Wilde Lectureship in Mental Philosophy, at Oxford.

PROFESSOR JAMES WARD'S Gifford Lectures on 'Naturalism and Agnosticism' are to be issued at once by the Macmillans. The same publishers are also to print Professor Royce's Gifford Lectures.

WE have received the first number of the new series of *l'Intermédiaire des Biologistes*, to the title of which the words *et des Médecins* are added by the new editor, M. L. Hallion. It has several new and interesting features. With it comes to hand also the first issue of *l'Intermédiaire des Neurologistes et des Aliénistes*, edited by Dr. Paul Sollier.

WE notice that Mr. Brooks Adams' *Law of Civilization and Decay* is to be issued in French translation by Alcan, Paris.

PROFESSOR MARTIUS, of Bonn, succeeds Professor Riehl at Kiel, the latter going to Halle.

WE regret to record the death of Robert Zimmermann, the distinguished Herbartian writer on *Æsthetics* and Philosophy.

PROFESSOR HÆCKEL'S interesting address (listed above) on *Der Ursprung des Menschen* has been brought out in book form by the Macmillans under the title 'The Last Link.'

M. SEAILLES has been appointed Professor of Philosophy in the University of Paris.

THE *Proceedings* of the National Educational Association (Washington meeting, 1898) are of more than usual interest. Papers by Royce, Krohn, Draper, Shaw, will interest psychologists, as will also the transactions of the child-study section. Taken with Commissioner Harris' admirable two-volume Report just published the educational world has an abundance of good reading.

J. M. B.

DR. E. B. MCGILVARY, Assistant Professor of Logic and the Theory of Knowledge in the University of California, has been appointed to the Sage Professorship of Moral Philosophy in Cornell University, to succeed Professor James Seth, now Professor of Moral Philosophy in the University of Edinburgh. Professor McGilvary made his chief philosophical studies at the University of California, and received there his degree of Ph.D., having previously won his M.A. at Princeton, and his A.B. at Davidson College, N. C. He will begin his duties at Cornell with the opening of the autumn term of 1899.

THE *Rivista Italiana di filosofia* so long and so successfully edited by the lamented Professor Ferri, is to be continued under the title *Rivista filosofica*. It will be directed by Professor C. Cantoni of the University of Pavia.



# THE PSYCHOLOGICAL REVIEW.

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## A STUDY OF GEOMETRICAL ILLUSIONS.

BY PROFESSOR CHARLES H. JUDD.

*New York University.*

With the single exception of Brentano's<sup>1</sup> unsuccessful attempt to explain the Müller-Lyer illusion by the general fact that acute angles are overestimated and obtuse angles are underestimated, no one has essayed to establish any direct relation between the illusions of linear distances and those in which there is false judgment of the angles. The so-called angle illusions have always been referred to the Poggendorff figure as the simplest case of the illusion. Since 1861, when Hering<sup>2</sup> first explained the Poggendorff illusion as due to the false estimation of the angles, there has been, in spite of disagreements as to the ultimate explanation of the false estimation, a universal acceptance of the statement that the angles are misjudged. It is the aim of this paper to present certain facts that seem to show that the false estimation of the angles in the Poggendorff figure is merely a secondary effect, not always present, and in no case the source of the illusion. The illusion is rather to be explained as due to the wrong estimation of certain linear distances, and may be reduced in the last analysis to the type of illusion found in the Müller-Lyer figure.

Before taking up the discussion of the Poggendorff illusion it will be necessary to point out certain facts in regard to the Müller-Lyer figure. The overestimation and underestimation characteristic of this figure are very much more comprehensive processes

<sup>1</sup> *Zeitsch. für Psych. und Phys.*, III., 349.

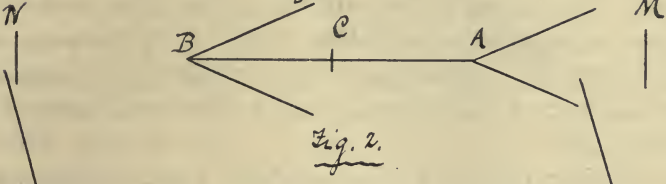
<sup>2</sup> *Beiträge zur Phys.*, p. 384.

than they are ordinarily recognized to be. The attention of the observer has always been concentrated on the lines included between the obliques. In developing his theory of boundaries Lipps<sup>1</sup> evidently sees that the concentration of the whole attention on a line within certain boundaries is a limitation which calls for some justification. He raises the question: How does it come that a line which bounds the figure within does not at the same time act as the boundary of the space without? The very important suggestion contained in this question is, however, entirely lost in the easy assumption with which Lipps dismisses the difficulty that he has raised. He assumes that when a line is in a position such that it may be regarded as the seat of the two non-contradictory functions which are relatively independent, one of these functions will appear, while the other will be swallowed up in the general perceptual process. Such a disposition of the matter does not find any justification in the empirical facts. Fig. 1 is made up of the two figures of the Müller-Lyer illusion so placed that the ends of the horizontal lines are equally distant from the short vertical line placed between them. It is evident that overestimation within the figure is accompanied by underestimation of the space outside of the figure, and, conversely, underestimation within the figure is accompanied by overestimation of the neighboring space. It may be objected that the presence of the short vertical line between the extremities of the horizontals gives us, in effect, two new Müller-Lyer figures of empty space in which the oblique lines will, of course, be directed in exactly the opposite direction to that in which they extend when regarded as parts of the original figures. The answer to this objection is that if no vertical line is used, but the subject is required to locate the point which is apparently half-way between the extremities of the horizontal lines, he will make an error indicating the presence of the illusion in its full intensity. This will be made somewhat clearer by Fig. 2. In this figure the obliques are drawn in such a way that with respect to the line as a whole they produce no illusion. The partial effects of the oblique lines are, however, by no means lost. If some neutral point of reference is marked so that direct comparison is possible,

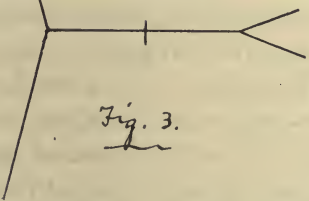
<sup>1</sup>Raumaesth. und geom. Täuschungen, III.



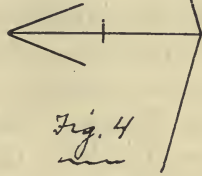
*Fig. 1.*



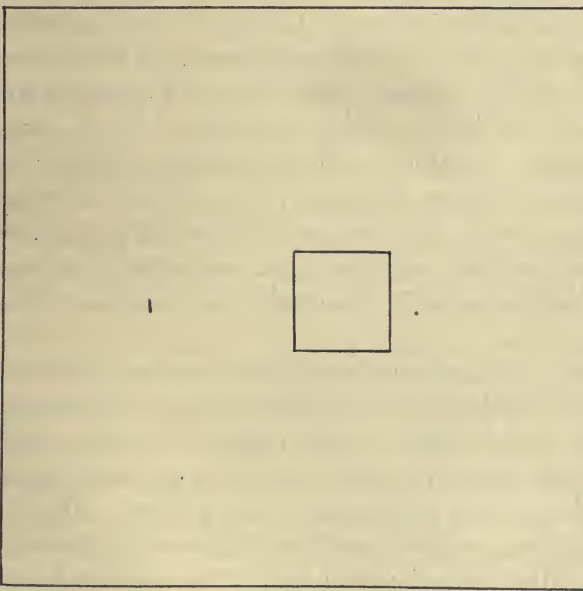
*Fig. 2.*



*Fig. 3.*



*Fig. 4.*



*Fig. 5.*



it can be seen that the partial effects are present in undiminished intensity. Thus in the figure the horizontal line as a whole suffers no illusion whatever. The middle point is marked, and it appears at once that in one-half of the line there is underestimation and in the other half there is an equal degree of overestimation. Furthermore, the position of the figure as a whole with respect to the short vertical lines, which are placed at equal distances from the extremities of the horizontal line, indicates that the influence of the oblique lines on the surrounding space is undiminished.

Other similar facts are illustrated by Figs. 3 and 4. In these figures the influences at the extremities of each of the horizontals are alike in kind, but unequal in degree. The result is, again, a shifting of the middle point. The illusion announced by Professor Baldwin in 1895<sup>1</sup> also belongs here. The illusion is in brief as follows: If two figures of unequal size, as two squares or two circles, are brought near to each other in the field of vision, the point half-way between them will be attracted towards the larger figure. The similarity between Fig. 3 and Professor Baldwin's circle figure is apparent at once. But the illusion appears when squares are used instead of circles. That even in this case the illusion belongs under the principle here developed, rather than under any principle of size-contrast, will be apparent from Fig. 5. Here the large square contains the small one, and yet the illusion is in kind and degree exactly like that described by Professor Baldwin.

All of these facts go to show that the processes of underestimation and overestimation within the figure are accompanied by far-reaching effects outside of the figures. In fact, overestimation and underestimation are wholly inadequate terms with which to describe the processes taking place. To say that points are shifted in their spatial relations with reference to all the points in the field of vision would be much nearer to the whole truth. Such shifting becomes apparent only when neutral points of reference are present in the field of vision, or when direct comparison with points which are shifted in the opposite

<sup>1</sup> PSYCHOLOGICAL REVIEW, II., 244.

direction becomes possible. When referred to this general principle it becomes evident that the phenomena of overestimation and underestimation are only occasional manifestations, under favorable conditions, of processes that are operative but undetected in every field of vision. In most cases the tendencies to false estimation of one sort or the other are lost in the complexity of the conditions or through the absence of points of comparison. One case in which such tendencies are present but unobserved is of such importance for our later discussion that we may call attention to it at this time. If an oblique line ends in a horizontal line which extends for an indefinite distance on each side of the point of meeting there will be no apparent illusion. If, however, the attempt is made to mark off in the horizontal line equal distances on each side of the point of intersection with the oblique line, it will be found that distances on the acute-angle side are underestimated and distances on the obtuse-angle side are overestimated. Or if the point of intersection is taken as the point of greatest importance, the illusion will take the form of the shifting of that point towards the extremity of the horizontal line which is on the acute-angle side.

Having thus generalized the concepts overestimation and underestimation, we turn to a consideration of the Poggendorff figure, and a criticism of the theory which regards it as the simplest case of the angle illusions. First of all, we have to consider certain negative evidences which show that the illusion is not due to a false estimation of the angles. Such negative evidence is at hand in the now generally known facts. The illusion disappears when the figure is so placed that the intercepted line is horizontal or vertical. If the illusion were due to wrong estimation of the angles it is not easy to see how these particular positions of the figure could operate to destroy the illusion. No general statement to the effect that an acute angle, one of the sides of which is vertical or horizontal, is not subject to false estimation will explain away this difficulty for the angle theory, for if the intercepting parallels, instead of the intercepted line, are brought into the vertical or horizontal position, the illusion appears in its full intensity. The negative evidence

presented in these four positions of the figure is strengthened by that given in Figs. 6, 7 and 8. In Fig. 6 the acute angles are present, but the illusion does not appear in any position of the figure. In Fig. 7 the parts of the intercepting parallels which lie between the points of interception are present, and with these the obtuse angles. The illusion is strengthened so much that it cannot be made to disappear in any position of the figure. The same is true of the case illustrated by Fig. 8, in which only a small part of each of the intercepting parallels is present. With regard to the angles, then, we must conclude that the acute angles, instead of being essential to the illusion, seem rather to weaken it, for the strongest form of the illusion appears when these angles are omitted.

Further negative evidence appears in Figs. 9 and 10. In Fig. 9 the upper and lower halves show the Poggendorff illusion in opposite directions. If the angles were misjudged, the interrupted lines should seem to diverge on the left and to converge on the right. As a matter of fact, it will be seen that all the oblique lines appear to be parallel with each other; this statement applying, of course, to the two interrupted lines when compared with each other. Furthermore, since the uninterrupted oblique lines form the same acute and obtuse angles with the intercepting parallels as do the intercepted lines, it is possible to make a direct comparison between the angles under discussion in cases in which the illusion is present and in cases in which it is absent. No inequality will be observable. On the other hand, it will be noted that the apparent width of the spaces between the oblique lines is not the same when the right and left sides of the figure are compared. Thus, the interval between the upper oblique and the interrupted line seems wider on the left than it does on the right. The space below the intercepted line seems broader on the right than on the left. The converse is true of the spaces above and below the lower intercepted line.

Figure 10 will be recognized as a complex made up of 6 and 7. The line CD shows no illusion of the Poggendorff type. The lines AB and EF show the typical illusion. At O and N the intercepting parallels are somewhat extended, and it will be observed that the acute angles thus formed, instead of seeming





Fig. 6.



Fig. 7.

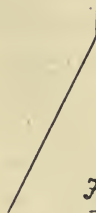


Fig. 8.

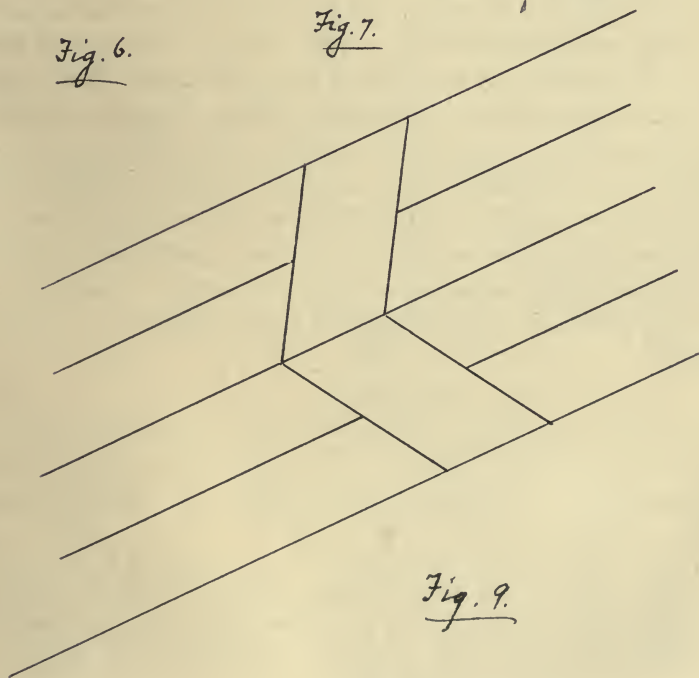


Fig. 9.

larger than the acute angles at 2 and 3, where no illusion appears, seem rather to be noticeably smaller.

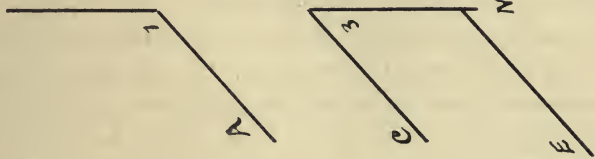
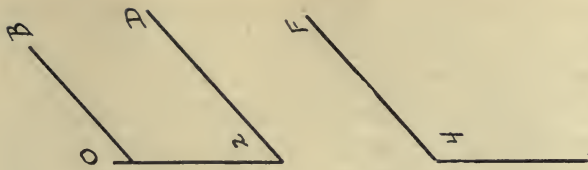
In view of this negative evidence it seems clear that the false estimation of the angles cannot be regarded as the explanation of the Poggendorff illusion. The real causes of the illusion are to be looked for in some other direction. We turn for our investigation of the figure to one of the positions in which the illusion disappears. Such a position is that in which the intercepted line is horizontal. If the apparent length of the interval between the points of interception is compared with an equal interval marked off by intercepting parallels which are perpendicular to the intercepted line, it will be observed, as seen in Fig. 11, that the interval in the Poggendorff figure is underestimated. This underestimation was subjected to quantitative determination. The method employed in these investigations was the same as that used by Heymans.<sup>1</sup> Cards were arranged so as to present a pair of parallels perpendicular to the horizontal line which they intercepted. The distance between these parallels, or the standard distance, was 50 mm. At the right of the parallels just described was a second pair of parallels also intercepting the horizontal line, but sloping obliquely from the upper right to the lower left, forming an angle of  $45^\circ$  with the horizontal line. The extreme right part of the figure, including the right oblique line and the part of the horizontal lying to the right of it, was made movable, so that the subject could easily adjust the distance between the points at which the oblique lines intercepted the horizontal. The errors for three subjects are given in Table I.

TABLE I.

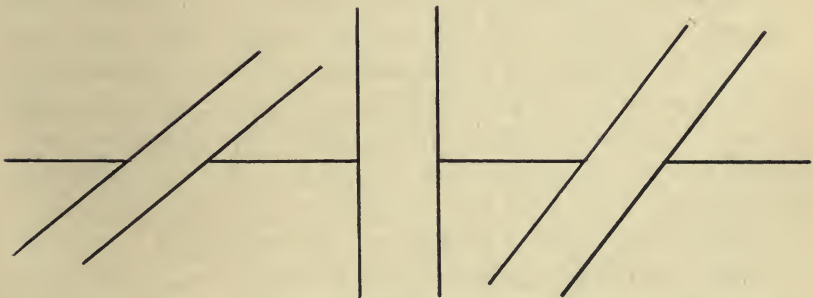
SUBJ.	NO. OF DETER.	AVG. ERROR.	M. V.
J.	10	6.7 mm.	1.1
E.	6	14.5	1.3
C.	3	6.3	1.1

The next step in the investigation was to break the figure up into its elements, with a view to discovering the importance of the different parts of the oblique parallels for the illusion.

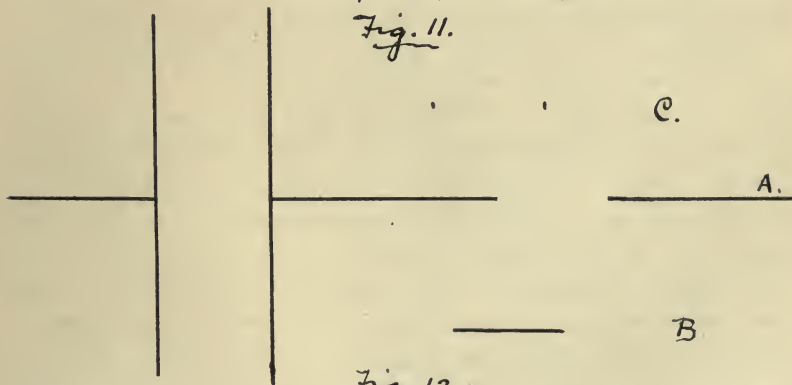
<sup>1</sup> *Zeitsch. für Psych. und Phys.*, IX., 221.



*Fig. 10.*



*Fig. 11.*



*Fig. 12.*



These oblique parallels were divided into four parts. The part of the left line above the horizontal was designated a; the part of the right oblique line above the horizontal was called b; in like manner the lower left line is c, the lower right line d. Cards were prepared which made possible all the different combinations of these lines, and measurements similar to those reported in Table I. were made with each combination. The standard distance was again 50 mm.; the angle of obliquity was as before 45°. The average errors are given in mm.

TABLE II.

NO. OF DETERM. IN EACH CASE.	SUB. J.		SUB. E.		SUB. C.		ORDER OF GREAT- EST ILLUSION.		
	10		6		3		SUB. J	SUB. E	SUB. C
	Avg.	M. V.	Avg.	M. V.	Avg.	M. V.			
a	7.5	0.9	11.8	0.6	7.7	0.9	ac	ad	cd
ab	8.8	1.0	13.0	0.5	5.3	1.1	d	abd	abd
ac	9.9	0.9	11.8	1.2	5.3	3.1	ad	abcd	abc
ad	8.9	1.0	17.5	1.2	8.7	1.1	ab	acd	ad
abc	2.4	0.4	13.3	1.3	9.0	0.6	abd	abc	d
abd	8.2	1.0	16.1	0.9	11.6	1.1	a	ab	a
acd	7.2	0.7	13.3	1.0	7.6	0.5	acd	a	acd
abcd	6.7	1.1	14.5	1.3	6.3	1.1	abcd	ac	abcd
b	3.6	0.8	5.7	0.4	2.0	0.6	bd	bed	bed
bc	1.0	0.6	4.7	0.8	1.0	0.6	none	d	ab
bd	6.0	1.4	7.4	0.7	2.0	0.0	bed	cd	ac
bcd	4.1	0.7	11.1	0.5	6.3	1.1	b	bd	b
c	2.9	0.7	5.3	0.7	1.0	0.6	abc	none	bd
cd	2.9	0.5	8.5	0.7	12.0	0.0	c	b	c
d	9.7	1.3	8.7	0.8	8.0	0.6	cd	c	bc
none	5.7	0.6	6.8	0.8	0.3	0.9	bc	bc	none

There are very noticeable personal differences in these results, but the general tendencies are common. The lines a and d are favorable to the illusion; the lines b and c are unfavorable. The figure resulting from the combination of the favorable lines is identical with the Müller-Lyer figure for underestimation; that resulting from the combination of the lines unfavorable to the illusion is identical with the Müller-Lyer figure for overestimation. Even under the last-named conditions, however, there is a slight underestimation as compared with the standard made use of in this case. This renders it necessary for us to examine at the outset of our discussion the

process by which the distance adopted as a standard in this case is estimated.

Fig. 12 makes it possible to compare the standard distance between the parallels with three other equal distances. The distance between the free ends of the interrupted horizontal (case A) is usually judged as shorter than the distance between the parallels. That this statement cannot be put in a universal form will appear when the results of the subject C, in Table II., are examined. The illusion in its general form has been observed by a large number of individuals who were not subjected to any quantitative tests. We are accordingly justified in regarding C as exceptional in this particular. This conclusion is confirmed by the fact that his judgment in case B is also exceptional. Most observers see a short unbroken horizontal line as equal to the standard distance. The subject C, on the other hand, sees the horizontal line as longer than the standard. Finally, when the interval between the parallels is compared to the interval between two dots (case C), the latter is usually seen as longer. The judgments in this case are, however, so irregular that quantitative determinations are of little value. Certain quantitative results which throw light on some of these cases are to be found in Tables III. and IV. In Table III. the results are given from comparison of the interval between the free ends of the interrupted horizontal and the interval between the parallels. The method is the same as before. The standard, that is, the distance between the parallels, was varied from 2.5 to 150 mm. Ten determinations were made in each case for J, five in each case for E.

TABLE III.

DISTANCE.		2.5	5.0	10.0	20.	50.	80.	100	150
Subj. J.	Avg.	1.4	1.9	3.3	3.8	5.7	8.2	3.0	-1.4
	M. V.	0.2	0.2	0.4	0.4	0.6	1.4	1.2	1.7
Subj. E.	Avg.	1.3	2.5	3.3	3.2	6.8	9.9	8.8	-0.4
	M. V.	0.2	0.0	0.2	0.6	0.8	1.5	2.5	1.8

Table IV. presents the results of a comparison by C of the distances marked off by a number of unbroken horizontal lines and the interval between the free ends of an interrupted hori-

zontal. The standard line in each case was the unbroken horizontal, the number of determinations five.

TABLE IV—SUBJECT C.

DISTANCE.	5.0	10.	25.	50	70	100
Avg.	2.0	5.8	6.0	10.2	11.2	14.0
M. V.	0.0	0.3	1.2	0.6	1.4	1.6

It will be noted that Tables III. and IV. are directly comparable. For J and E the interval between parallels is judged as equal to the horizontal line which is used as the standard in Table IV. The same general result is to be found in both tables. The illusion is greatest for short distances.

In connection with these facts attention is to be called to those cases in Table II. in which the free end of the horizontal is left after the withdrawal of both parts of the oblique. Take, for example, the case in which *d* alone is present. It will be seen that for J and C the illusion is about as strong as it is when *d* is combined with the most favorable line, namely, the line *a*. Again, in such cases as *a*, *ac*, and even *bd*, there are indications that the free end of the horizontal is favorable to the illusion.

If we attempt to find an explanation of the illusion which appears when the break in a horizontal line is compared with the interval between two parallels, or with the unbroken horizontal line, it is evident that we cannot appeal to the principle that filled space is overestimated when compared with empty space. The two intervals stand on a par with respect to their content. Then, again, when these intervals are compared with an interval bounded by dots, it will be seen that this last example of empty space is subject to overestimation rather than underestimation. The theory of Lipps, which attributes such illusions to the bounding activity of the various lines and points, on the general principle that the greater the bounding activity of the terminal lines or points the more the bounded interval is underestimated, does not seem adequate to explain these facts. Thus, it is difficult to see how the parallel lines can be regarded as less intense boundaries than the free ends of the interrupted horizontals. Again, in his explanation of the Müller-Lyer



figure, Lipps assumes that the oblique lines which slope outward have a bounding activity which is negative. This cannot hold when applied to case *bc* in Table II. To be sure, some of these difficulties could be avoided if the bounding activities were regarded as applying to the intercepted lines rather than to the intervals. But this brings us back to the position taken early in this paper, namely, the position that the points at the end of a line have spatial relations in all directions, and any modification of the relations in one direction involved at the same time the opposite modification in the opposite direction. Such shifting of points cannot be explained by the bounding activity of lines or points, for it is the boundaries themselves that are shifted in their spatial relations.

The movement hypothesis, on the other hand, seems to meet the demands of all the different cases. The more intense the sensation of movement, the greater will be the estimation of the distance; conversely, the less the intensity of the sensations of movement, the shorter the estimated distance. If from a given point the tendencies of movement are outward, then the movement outward will be, if it is executed, somewhat easier. In any case the tendency will result in an active tension of the muscles which favor outward movement of the eyes. The space lying in that direction will be underestimated, while space in the opposite direction will be overestimated. The point from which these tendencies emanate will, accordingly, be shifted outward. If, on the other hand, the tendencies from a given point are inward, the point will be shifted inward. Influences of one kind and another may be combined in great variety of quality and intensity. The final apparent position of a point will be determined by all of these influences operating together. The question now arises: What are some of the influences which give rise to tendencies of movement? The most important fact in this connection is that the eye tends to follow lines rather than to direct its own course through space. When lines are present in the field of vision they tend to attract and direct the eye in its movements or in its tendencies of movement. But every line has two directions, and, therefore, it cannot in itself determine the particular direction in which the eye is to move or

to tend to move in any particular case. There enters at this point of our discussion a very important and very ambiguous factor. We may designate it by the convenient term, the direction of attention. By this we mean to indicate that the particular direction of movement in any given case depends on the relation of that part of the field of space which is subjectively the most important to all other parts. Thus, in the cases reported in Table II. the important distances were the breaks in the horizontal lines. The tendencies of movement originating in all of the lines will have their direction determined by their relations to these two intervals. If we attempt now to discover which lines are favorable to movement across these intervals and which are unfavorable we shall find full agreement between our results as recorded in the tables and principles just developed.

In Table II. it will be seen that all those combinations of lines which render movement across the open interval easy, as, for example, a, ad, acd, dcb, and others, are favorable to the illusion. The fact that cb is not a case of overestimation rather than underestimation can be explained by a consideration of all the facts reported in Tables III. and IV., together with some of the subjective observations made during those tests. A characteristic observation was made by C while comparing the horizontal lines with the interval between the free ends of an interrupted horizontal (Table IV.). The ends of the lines and of the interval seemed more or less uncertain. The ends of the line seemed to run out into the surrounding space, thus making the line seem longer; the ends of the lines bounding the interval, on the other hand, seemed to close in on the interval, making it seem smaller. In both these cases there is evidently a strong tendency for the movement which has originated in the line to extend itself beyond the end of the line. This holds for those cases reported in Table II., in which the free end of the horizontal is favorable to the illusion. To return now to the case bc. While these lines alone would result in overestimation of the interval as in the normal Müller-Lyer figure, their influence is counteracted by the tendencies produced in the horizontal lines.

The principles may also be applied to the estimation of the interval between the dots as compared with the interval between

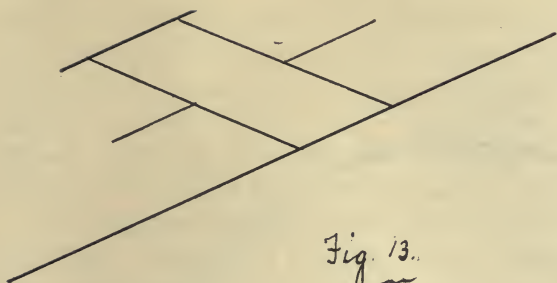


Fig. 13.

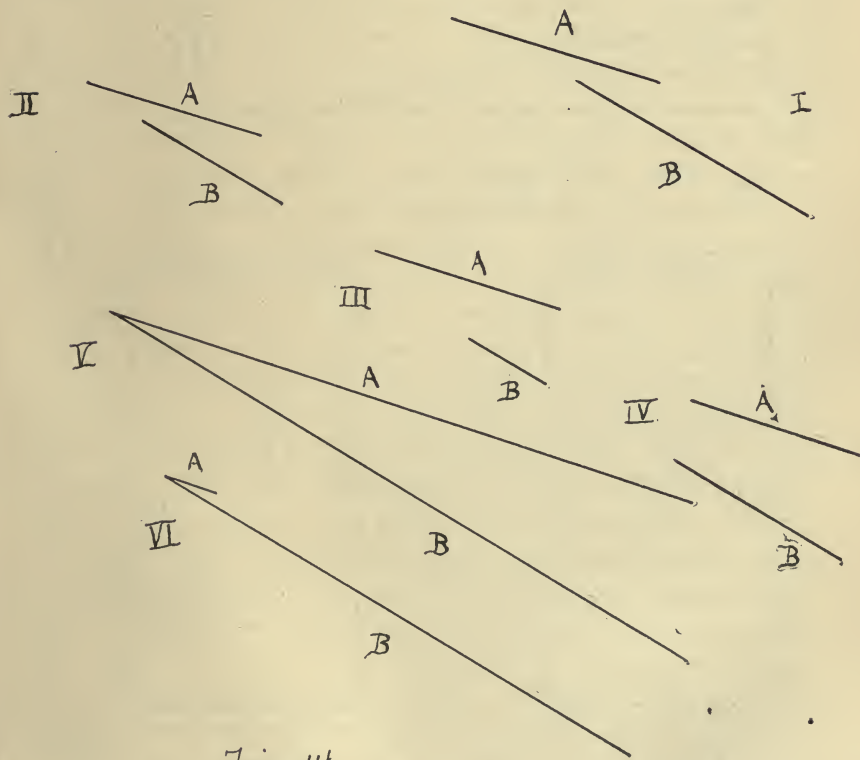
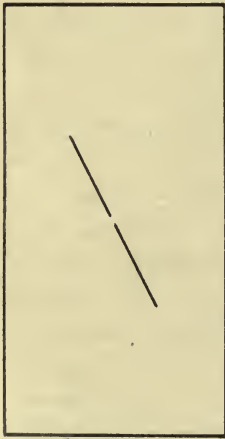
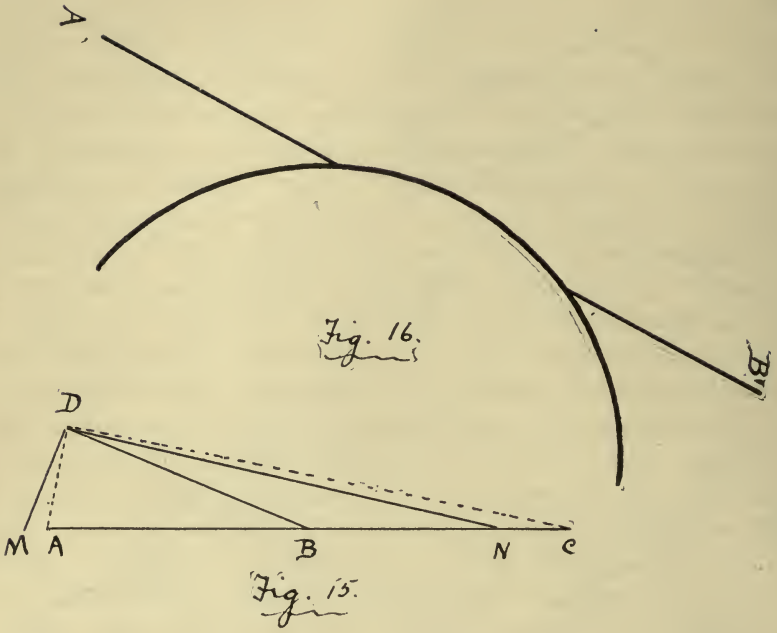
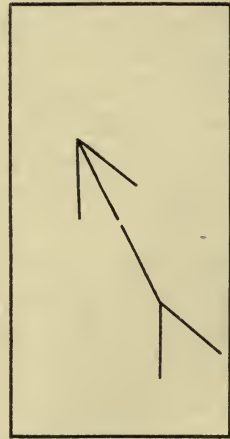


Fig. 14.





*Fig. 17.*



*Fig. 18.*

the parallels. The dots give rise to no tendencies of movement across the interval. For most observers the perpendiculars counteract the tendencies toward underestimation which originate in the horizontal lines. The influence of these horizontals is not entirely overcome. In the case of the subject C it seems to persist in its full intensity. In fact, his description of his method of estimation would seem to justify the special explanation that he neglects the parallels almost entirely.

The measurements and explanations thus far presented apply only to the exceptional positions of the Poggendorff figure; to those, namely, in which the typical illusion disappears. That the underestimation of the interval between the points of interception is present, though in a smaller degree, in other positions of the figure, can be seen by comparing those cases in which the illusion appears with those in which it does not appear, as given in Figs. 6, 7, 8 and 10. But this fact does not suffice to explain the appearance of the illusion in certain positions and its disappearance in others. There is another fact of false judgment to which attention was called in the discussion of Fig. 9. The distances along the parallels (in the case of Fig. 9 the vertical distances) are also misjudged. For the explanation of this misjudgment we have only to refer back to our earlier discussions. When horizontal or vertical distances along the parallel lines are the subjects of attention, as they are in the usual positions of the Poggendorff figure, those portions of the parallels lying on the obtuse-angle side of the intercepted line will be overestimated. The overestimation of this distance along the parallels with the underestimation of the oblique distance across the interval gives us a full explanation of the illusion. At the same time we have in the appearance and disappearance of the typical Poggendorff illusion in different positions of the figure an illustration of the fact that lines produce illusions only when they have some direct influence on the particular direction to which the attention is turned. When the Poggendorff figure is in such a position that the intercepted line is horizontal, the false estimation of distances along the parallels has no direct bearing on the distance to which the attention is directed. The whole influence of the parallels is there absorbed in aiding the

intercepted horizontal line in carrying the eye across the interval. When, on the other hand, the length of the parallel becomes itself a matter of judgment, as it does when the parallels, instead of the interrupted line, are vertical or horizontal, then the whole combination of conditions changes. The influence of the intercepted line is such that distances along the parallels are overestimated on the obtuse-angle side of the point of interception. The parallels no longer bridge over the interval so fully, and the underestimation is, therefore, much less marked.

In view of all these considerations, both negative and positive, we conclude that the Poggendorff illusion is not due to the false estimation of angles. The question now presents itself: Can the Poggendorff illusion under any conditions give rise to a false estimation of angles? Fig. 13 furnishes empirical evidence which goes to show that it can. The intercepted line in this figure does not seem parallel to the other oblique lines, but slopes in such a way that it seems to meet the lower line at the left and the upper on the right. This leads to a discussion of the general question: How do we in any case judge the size of angles?

The discussions of the estimations of angles have always confined their attention to the simplest case of such judgments, namely, those in which the vertex is expressed in the figure. Such a limitation of the discussion is obviously unwarranted and has led to theories of angles that are correspondingly short-sighted. Angles may appear, and often do appear, in concrete experiences between lines and surfaces not in direct contact. How is it possible for us to estimate these angles? Fig. 14 presents a number of cases in which lines were so drawn that each pair subtends exactly the same angle. All the lines marked A are parallel and all the lines marked B are parallel. The difference in apparent size of the angles is noticeable at once.

By means of these figures let us test Wundt's hypothesis, which is that small angles require relatively greater energy for the eye to move through them on the general principle of physical and physiological inertia. This greater energy of movement is interpreted as due to greater surface between the



sides. Compare now the two Angles IV. and VI. at the bottom of the figure. The left-hand figure shows an angle to which Wundt's theory should apply most admirably and we should have overestimation. On the other hand, the lower right figure should be beyond the range of application of Wundt's theory and we should have underestimation. The appearance is, of course, directly opposed to the theory in both cases.

Or take the Helmholtz theory of contrast in direction of movement, it is difficult to see how the greater and weaker effects of contrast could explain the differences in the estimations of the angles in Fig. 14 for which the vertices are not expressed. Yet there is a very notable difference in the judged sizes of these angles. The general criticism of perspective theories seems to the present writer to have been so fully carried out by Wundt that there is no call at this time for a repetition. The angles in Fig. 14, it may be noted, are so drawn that perspective influences play little, if any, part. All the lines are in the same direction and the lines are drawn from the vertices obliquely towards the observer.

The most obvious induction from Fig. 14 is that the judgment of the length of the sides of an angle is a very important factor in the judgment of the size of the angle. The *a priori* probability which attaches to this statement is so great that it seems hardly possible that it should have been persistently overlooked. In addition to this judgment of the length of the sides, there must also be the judgment of the distance between the sides at a certain distance from the vertex. In short, the whole process is a kind of triangulation in which the mind takes into account three factors, namely, the distances from the vertex at which the measuring arc is to be drawn, and the length of that arc with respect to the whole circumference of the circle. Thus it is possible for us to recognize an angle whether we measure the arc near its vertex or at some distance from the vertex. Such complex judgment is, however, subject to many influences that produce illusion. If, for any reason, the arc is judged too long, the angle is overestimated, or, *vice versa*, a judgment which makes the arc too short results in underestimation of the angle.

Misjudgment of the length of the sides may lead indirectly to such false estimation of angles. Thus, if the subject is asked to point out the vertex for Angle IV. in Fig. 14, he will usually place it too far from the lines, that is, he will underestimate the length of the sides. Conversely, in Angle I., he will place the vertex on A too near the left-hand extremity of B.

These facts apply with less clearness to Angles V. and VI. VI. is underestimated, but the reason in this case may be that the arc is underestimated rather than the length of the sides misjudged. Explanation of Angle VI. is difficult. The principle discovered in the other cases, however, is of very general importance. It can be brought into direct relation with the fact that all acute angles are overestimated and obtuse angles underestimated. This will appear from Fig. 15.  $AC$  is a line at the middle point of which ( $B$ ), an oblique line equal in length to  $AB$  is drawn. Since movement from  $B$  to  $A$  is favored by  $BD$ , it follows, on the principle developed, that the point  $B$  will be shifted towards  $A$ . As compared with  $BD$  the line  $BA$  will be underestimated, and the line  $BC$  will be overestimated. In comparing the two angles  $DBA$  and  $DBC$  the arcs will, therefore, be estimated as cutting  $AC$ , not at  $A$  and  $C$ , but at some points as  $M$  and  $N$ . The true arcs (or chords) of measurement are  $DA$  and  $DC$ , but the chords used are  $DM$  and  $DN$ . And since  $DM$  is longer than  $DA$ , while  $DN$  is shorter than  $DC$ , the angle  $DBA$  will be overestimated, while the angle  $DBC$  is underestimated.

In this way it is possible to explain all the angle illusions. The movements upward and downward of the lines in the Zöllner pattern, as observed and described by Helmholtz, are direct confirmations of this position. The successive fixation of certain points in the figure brings out the illusion of length, first on the side of an acute angle, then on that of an obtuse angle. The angles do not suffer any further change by such successive fixation, but the presence of a neutral point of reference gives clearness to the illusions of length, that is, the original source of the angle illusion appears in its simplest form. Furthermore, it will be found that if Wundt's and Hering's modifications of the Zöllner figure are so drawn that the extremities of the

different oblique lines are not distinctly marked (particularly the point to which all converge), the illusion of bending in the horizontals will be very much reduced, if not entirely lost.

Finally, Figs. 16, 17 and 18 present angle and distance illusions in such relation that the principles discussed may be directly applied and at the same time confirmed. In Fig. 16 the line *AB* seems to be bent inward so that its two parts if continued would form a very obtuse angle within the circle. This is due in part to the underestimation of the sides of the acute angle and the overestimation of the arc between the points of interruption. Furthermore, the rapid increase in the distance between the line and the circle beyond the points of contact may result in overestimation of the arc by which the angle would be estimated. In Figs. 17 and 18 a part of the diagonal is drawn in such a way that it extends for equal distances on each side of the middle of the rectangle. In Fig. 17 it will be noted that the part of the diagonal seems to meet the left side of the rectangle below the corner, and the right side above the corner. The illusion will be clearest if the ends of the line are fixated. It will also be observed that there is a marked tendency for the eye to pass in its movement, not through the longer distance from the ends of the line to the corners, but from the ends of the line to the nearer right and left sides of the rectangle. This tendency is a full explanation. It leads to the angle illusion through the underestimation of the side. Fig. 18 combines a number of the facts already pointed out. The detailed analysis may be left to the reader.



# THE NATURE OF ANIMAL INTELLIGENCE AND THE METHODS OF INVESTIGATING IT.

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Those interested in this subject may be classified in the main somewhat as follows perhaps :

1. Those who see in the animal mind only a sort of weaker human intellect ; who look chiefly for evidences of intelligence and take no account of the failures and stupidity of animals.

2. Those who recognize that the animal mind is not the equivalent of the human mind in all its qualities as it exists in men of superior development in the highest civilization, but who nevertheless recognize the resemblance up to a certain point to man.

3. Those who approach more or less closely to the view that animals are automata, or at all events consider animal consciousness as utterly different from human consciousness, except in a few of its lowest states. With regard to investigation or material of knowledge we recognize a class who, while suspicious with reference to the conclusions of the anecdotal school, do not consider anecdotes worthless, much less meriting the supreme contempt some writers manifest for such evidence. They believe that there is no more reason to set aside reliable anecdotes of animals than of men. Anecdotes may illustrate a normal, sub-normal or super-normal mental condition or development ; but if they set forth facts it is for the psychologist to explain, not to ignore them. Another class of investigators see little or no good in anything in comparative psychology or psychology in general, except experiment, which is for them the sole key to a reliable knowledge of the mind.

Among psychologists as among biologists there are those who are willing to shut themselves up in the narrow lane of experi-

ment—a lane with high walls on each side cutting off all view of the surrounding domain open to general observation and experience. As these people see so little themselves yet ever behold that little before them, they come to interpret everything in the light of their own limited observations. They insist on others believing as they do; they would have others wear the fetters they have put on themselves; all thinking must conform to the rigid conditions in which they are content to live and move and have their intellectual being.

The only hope of safety for the man who engages in experiment is ever to check his observations, and, above all, his conclusions, by other wider observations and those broad general principles which are like the points of the compass to the mariner; and I venture to suggest that it is the failure to do this which accounts for the greater part of the wrecks scattered along the shores and over the bottom of those seas traversed by the experimenter in biology and psychology.

As we have had what I cannot but think a recent conspicuous example of the sort of neglect referred to, I propose to criticise the methods pursued and the conclusions drawn, the more especially as this investigator claims to have swept away, at one fell swoop, almost the entire fabric of comparative psychology.<sup>1</sup> He appears to believe that he has razed the old structure to its very foundations and settled once and forever the weightiest problems with which others have been long struggling in vain.

Dr. Thorndike has not been hampered in his researches by any of that respect for workers of the past of any complexion which usually causes men to pause before differing radically from them, not to say gleefully consigning them to the psychological flames. For Dr. Thorndike the comparative psychologists are readily and simply classified—they are all insane—the only difference being the degree, for he speaks of one of them as being ‘the sanest’ of the lot.

Having thus cleared the way, this investigator proceeds to set forth, in no uncertain terms, what we should believe, and his creed is very brief and easily remembered. Animals neither

<sup>1</sup> ANIMAL INTELLIGENCE, by Ed. L. Thorndike. (Monograph Supplement to the PSYCHOLOGICAL REVIEW, Vol. II., No. 4, whole No. 8.)

imitate, feel sympathetically, reason nor remember, though about the latter point he is not quite so dogmatic.<sup>1</sup> He comes very near to the belief that they are automata pure and simple, though this he does not assert in so many words. The above mentioned views he thinks he has deduced from experiments. If so, the present writer thinks so much the worse for the experiments. At all events, with the exception of reasoning about which I wish to reserve judgment, I have come to widely different conclusions and from experiments also as well as from other sources of information.

Dr. Thorndike in criticising my book<sup>2</sup> has given the impression that I have not made experiments, or 'crucial experiments.' Now, I think it can be shown from my publications that I have recorded more experiments (not to mention scores which have not been described) than all other investigators together, if we except those working on insects. Moreover, these experiments have been invariably conducted under natural conditions, the absence of which seems to be almost a recommendation with some, but which I consider a fatal objection to Dr. Thorndike's work. Incidentally, I may remark that a laboratory as ordinarily understood is not well suited for making psychological experiments on animals.

When Dr. Thorndike charges that most of the books do not give us a psychology, but rather a eulogy of animals; that they have all been about animal intelligence, never about animal stupidity, I recognize a certain amount of truth in the imputation. But I beg to suggest that to a certain extent the same applies to works on human psychology. To what extent has the mind of the savage or semi-barbarous man been investigated? Yet to make comparisons between man and the lower animals parallel such a study is essential. I do not find Dr. Thorndike's publication any freer than others from the fallacies aris-

<sup>1</sup> In an account of his own work given by Dr. Thorndike in *Science* (Vol. VII., p. 823) he goes still further in his negations. "Conception, inference, judgment, memory, self-consciousness, social consciousness, imagination, association and perception, in the common acceptance of the terms, are all absent from the animal mind."

<sup>2</sup> *The Nature and Development of Animal Intelligence*. London, T. Fisher Unwin; New York, The Macmillan Company. 1898.



ing from considering the superior class of human minds or the civilized and educated man, and comparing him with the lower animals. Dr. Thorndike considers his experiments crucial; that individual peculiarities have been eliminated; that hunger is an adequate stimulus or condition; that no personal factor need be considered; that "the question of whether an animal does or does not form a certain association requires for an answer no higher qualification than a pair of eyes"—all of which I consider fallacious and to a large degree explanatory of the misleading psychology which he has constructed. With dogs I found several stimuli stronger than hunger, as any one really acquainted with the nature of animals must know, and such stimuli may, and frequently do, lead animals so to deport themselves that they become a perfect revelation to those who have long been associated with them.

I had that well illustrated in the case of a tame fox (vixen) that I reared. When a certain critical period (*œstrum*) was reached her whole nature took on a new character, and it became practically impossible to control her as formerly; and, unless I had ocular demonstration of the facts, I would not have believed it possible for any animal to have accomplished what this fox did. Nevertheless, in order to learn her methods of procedure it was necessary to observe unawares to her, and that I may say applies to very many studies of animals. That a pair of eyes is not all that is requisite for a complete outfit as an observer, Dr. Thorndike's work but too pointedly exemplifies. I venture to think that in all cases it is a question of whose eyes, or, in other words, the training those eyes have had, and still more of the intellect that passes judgment on what is seen.

I have all along endeavored to emphasize the importance of individual differences. They do somewhat disturb statistics, and they rather spoil curves, it is true, and experimenters have always been prone to ignore them; but they exist in nature, and when adequately recognized our explanations for many things will be found altogether too simple, and, therefore, delusive, rather than real and adequate.

Dr. Thorndike admits that 'an act of the sort likely to be attended to will be learned more quickly.' Undoubtedly, yet

this investigator has practically ignored this in his tests, for he placed cats in boxes only  $20 \times 15 \times 12$  inches, and then expected them to act naturally. As well enclose a living man in a coffin, lower him, against his will, into the earth, and attempt to deduce normal psychology from his conduct.

The present writer has pointed out distinctly that when animals are removed from even their usual, not to say natural, surroundings they may be so confused or otherwise diordered that they fail to act normally, and this I have illustrated by experiments. Dr. Thorndike found that dogs when placed under similarly improper and disturbing conditions, as I deem them, behaved in a like panicky way, except that they gave up sooner, which he attributes in part to their being insufficiently hungry. But dogs have not as much perseverance as cats, as my experiments abundantly prove. However, had Dr. Thorndike witnessed the resources of my dogs when let loose in the yard after some of their companions, which had already been set free in the adjoining fields and woods, I can believe that even one so fast bound in the grip of his experiments as he would have altered his opinions on this and many other subjects. In dogs under such circumstances we have illustrated not alone an adequate motive or stimulus, but it is shown that they have memory—can conjure up exciting pictures of the pleasure-giving scenes of the past, re-experience in some fashion the delights associated with that past, make a sort of generalized abstract of the whole—in a word, have very much the same experiences as the human being who accompanies them and delights in such things.

When the contrary is proved by adequate observations or experiments, I am ready to alter my opinions, but not on such evidence as seems to go directly counter to all that one has borne in upon him by daily observation. To do otherwise is, indeed, to bid adieu to common sense as well as to science, and to accept as proof what seems to me of no more value than counterfeit coins, but which, nevertheless, like bogus money, deceives the unwary, even among psychologists.

The experiments on chicks I consider the least misleading and most valuable part of Dr. Thorndike's work. Not only are birds much lower in the psychological scale; not only does free

association explain more in their case, but the conditions of the experiments were rather more natural. A pen  $16 \times 14 \times 10$  makes for a chick a very different thing from one  $20 \times 15 \times 12$  for a cat. Even those curves which in the case of the cats and dogs only serve to stereotype error are possibly of some value when applied to the chicks. Says Dr. Thorndike: "I hate to burden the reader with the disgusting rhetoric which would result if I had to insist on particularizations and reservations at every step." If anything, just such particulars might have somewhat redeemed these experiments. They might at least have proved helpful in some way. At the present stage of comparative psychology we are in need of observations down to the minutest details. We can better spare the rhetoric.

When we consider how widespread—indeed, almost universal—is imitation among animals of the middle and higher grades, that it is difficult so to separate it from the general psychic life of the animal as to be able fairly to analyze their mental processes and determine how much is due to independent development *per se* and how much to imitation, one cannot but marvel at the degree to which that magic word of modern science 'experiment' can blind the mind to facts thick as the leaves of the forest, and all pointing to the importance of imitation in animal life. So obvious an example of imitation as the talking of parrots is set aside or twisted out of all recognition. It is, moreover, a case of heads I win, tails you lose. Much that Dr. Thorndike has said when discussing this subject is valuable as suggesting a basis for observation and on the genesis of imitation, though this applies also to human psychology. There is one fallacy that underlies the whole of Dr. Thorndike's experimenting and vitiates his conclusions, namely, this: that he overlooks the many possible and actual inhibitions to response to a stimulus. One would have thought that the case of the cat mentioned by him (p. 59) would have given him pause. The conduct of that cat, like all the rest, only proves to him that animals do not imitate.

I find myself ever disposed to imitate in certain cases, yet do not. To illustrate—when I read a chapter on psychology written in the fascinating style of James, one exemplifying the profundity of a Ladd or a Hall, the bold constructive character of a



Baldwin, or a vigorous plea on behalf of modern psychology by Cattell—the list might be much enlarged—I am filled with admiration, and there is an impulse to imitate, but I have not as yet taken the first step. Having thus been the subject of experiment in this way over and over again, I should, according to the logic of Dr. Thorndike, be characterized as a non-imitating creature—not only as regards the subject in question, but generally. The truth is far from this. There is a strong tendency on my part to imitate, but there are stronger forces acting to inhibit the process, and, moreover, these forces are not always the same nor is each always equally potent. In truth, the whole matter is very complex even in animals. I find no difficulty whatever in explaining why the animals did not respond to the stimuli Dr. Thorndike used.

When one meets the *questionnaires* he seems at last to strike the rock bottom of common sense. The author of the experiments referred to has no high opinion of the trainers. “I would first adjust all things in connection with the surroundings of the cat so that they would be applicable to the laws of nature, and then proceed to teach the trick.” I see much saving sense in this remark, and believe that had Dr. Thorndike grasped its significance he would have given us a very different psychology. The writer seems to have totally neglected the methods and experience of the trainers of dogs for field work, and has also I believe failed to make use of the lessons the trainers of trick animals can teach us. Even to witness a performance of trick animals is enough to enable one to see how at one time the tendency to imitate assists and at another mars the performance. To be sure, there is a sort of deliberate, studied, high-class imitation possible to man, but beyond the reach of animals, but this is, after all, comparatively rarely employed in the lives of the great mass of men.

A student of McGill University has communicated to me the fact that a kitten which could not be induced to jump over an object placed before it did so only after seeing the mother do it, and after that there was no more trouble in getting it to perform the trick. The young hounds of the Montreal Hunt Club are taught by being actually put through the performance, *i. e.*,

they are attached to an old and strong dog while hunting, so that Dr. Thorndike's contention as to the uselessness of an animal's being put through a performance breaks down. Indeed, that was to be expected even from his own teaching as to the genesis of associations, to go no further. As to the inability of animals to have memory images for which Dr. Thorndike contends I find myself, in the light of my experience with animals, quite unable to agree. I believe that their memory is like our memory of the same things so far as image, etc., are concerned, but that there may be with man, owing to the complexity of his mental condition, a more varied fringe around that memory core which latter will be much alike in both the man and the animal.

To refer to but a single experiment to illustrate this: I had a greyhound that was very prone to chase cats, a habit which became with him more and more pronounced, I presume, from his success in consequence of his speed. On the occasion I wish to emphasize I had taken the dog in a certain direction, and, as a result, a cat crossing the street was so hotly pursued by him that she took to a tree. Many months after I brought the dog along this same way, but approached the scene of the exciting chase from the opposite direction. Long before the exact spot was reached the dog was all attention. It was perfectly plain that he remembered the long-past incident, and that certain feelings (which accompanying feelings Dr. Thorndike denies to animals) were also aroused; but great was my astonishment when the dog stopped at a certain tree, looked up and behaved otherwise in such a manner as left no doubt in my mind that he remembered the identical tree and every detail of the whole incident. This cannot be explained by the sort of consecutive association that Dr. Thorndike would substitute for 'memory' as ordinarily understood, for the locality was approached from the opposite direction.

The central phenomena of memory were in this case the same with the dog and his master, but the feelings and the mental fringe or associated ideas were not identical. In the one case they were appropriate to the dog, in the other to the man, his master, who was in this instance trying to draw some psychological conclusions, so the difference was considerable; but had

it been a hunting expedition in which both dog and man took an active part, the resemblance even in revival would have been altogether greater.

One finds in the end, however, that Dr. Thorndike does allow representation to animals within very narrow limits. Along with this writer's "I never succeeded in getting the animal to change its way for mine," a quotation from a recent interesting and instructive publication seems timely: "One must be familiar with the normal conditions of the insects in question before he is able to note those slight changes in the environment that offer some opportunity for an adaptation of means to ends, or before he is competent to devise experiments which test their powers in this direction."<sup>1</sup> The above seems to the present writer to be applicable in the widest sense to investigations in comparative psychology.

The experiments to which Dr. Thorndike refers under the heading 'Association by Similarity and the Formation of Concepts' only really show that animals may react to a vague stimulus, and this is quite sufficient to meet the ends of their existence in many cases; but neither these experiments nor any others show conclusively that this alone is the best of which animals are capable. The comparison of animal consciousness to human consciousness during swimming is open to the same objection. Such a mental state is possible to both man and animals, but neither is confined within such narrow limits of almost pure sensation.

I must object to Dr. Thorndike's analysis of human consciousness in playing open-air games as being inadequate. It does not correspond with my own experience nor with the accounts I have heard persons of different degrees of skill give as to what was going on in their minds during the playing of games. No doubt Dr. Thorndike's account does fit a certain portion of the mental phenomena, but the whole matter is much more complex than he seems to think, and is worthy of an analysis more accurate and comprehensive than has ever been given to it. Such views of animal consciousness as Dr. Thorndike presents seem

<sup>1</sup> *Instincts and Habits of the Solitary Wasps*, by Geo. W. Peckham and Elizabeth G. Peckham, p. 234.



to me altogether too narrow to meet the actual mental condition of, say, a dog when engaged in a fowling expedition.

From certain experiments which I made with my dogs in play, taken along with scores of others, I find myself utterly unable to agree with many of the views of the destructive or narrowly restrictive school of comparative psychologists. We should surely be very cautious in denying wholly to animals what Dr. Thorndike terms 'free floating ideas.' The believer in evolution will demand that, in this and other cases in which qualities man possesses are denied to animals, there be the clearest proofs given. The burden of proof lies with those who deny them, and this remark applies to feelings as well as intellectual processes, though to a less degree. Nor can I agree with those who maintain that we must always adopt the *simplest* explanation of an animal's action. Such does not apply to man, and why should it meet every case among animals? Though in this regard Professor C. Ll. Morgan with others seems to me to be in error, I fully agree with the views of this writer as quoted in the publication under consideration (p. 86): "Lastly, before taking leave of the subject of the chapter, I am most anxious that it should not be thought that in contending that intelligence is not reason I wish in any way to disparage intelligence," etc. But Professor Morgan is more and more in sympathy with the destructive school, so that he now seems willing to surrender anything to all and sundry who may ask him to stand and deliver. I have been myself classed by one of my reviewers<sup>1</sup> with Romanes. While I agree with much in Romanes' attitude in regard to animal intelligence, nevertheless, since this writer preferred to work upon second-hand material rather than make observations and experiments for himself, and had, moreover, a tendency to speculation rather than the accumulation and weighing of facts, I prefer to be myself considered an humble follower of Darwin, who, so far as he went in animal psychology, best illustrates the method and especially the spirit that will, I think, prove most fruitful.

The one point about which I feel like withholding an opinion till many more observations have been made is that of reasoning.

<sup>1</sup> *Science*, Vol. VIII., p. 520.

That animals can reach C by some mental process when A and B are given, and that this is to be explained either by some process of inference or by one as yet unexplained, I have little doubt. Unquestionably, association explains much in the mental structure of man and still more in animals, but that this is the whole story when we get beyond elementary chapters in instinct I cannot for a moment believe, unless the meaning of the word is greatly and unwisely extended. The subconscious must enter largely into the psychic life of animals, as of men, and one who observes animals long and closely must believe that no such naked skeleton as Dr. Thorndike presents to us can represent the animal mind.

The mental processes of an animal are generally not comparable to pure tones, but rather like those tones that abound in overtones, though this applies still more to man. Our age will probably be looked back upon as one characterized intellectually by great destructive and constructive activity, but also as one readily satisfied with unduly simple explanations put forward with a confidence and rashness that will be astounding to a later age. As showing, however, a different spirit and tendency I quote the following<sup>1</sup> with much gratification, coming, as it does, from two most patient, sympathetic and successful observers: "Our study of the activities of wasps has satisfied us that it is impossible to classify them in any simple way. The old notion that the acts of bees, wasps and ants were all varying forms of instinct is no longer tenable and must give way to a more philosophical view. It would appear to be quite certain that these are not only instinctive acts, but acts of intelligence as well, and a third variety also—acts that are probably due to imitation, although whether much or little intelligence accompanies this imitation is admittedly difficult to determine. Again, acts that are instinctive in one species may be intelligent in another, and we may even assert that there is considerable variation in the amount of intelligence displayed by different individuals of the same species."

The same may, I believe, be affirmed for animals generally; and it is work of the character described in the monograph

<sup>1</sup>Op. cit., p. 228.

from which I quote which really advances comparative psychology.

Were it possible to observe an animal, say a dog, from the moment of its birth onward continuously for one year, noting the precise conditions and all that happens under these conditions, the observer being unnoticed by the creature studied, we should, I believe, be in possession of one of the most valuable contributions it is possible to make to comparative psychology. This would imply not one, but several persons giving up their whole time, day and night, by turns, to such a task. As yet, but very imperfect approaches have been made to anything of the kind; nevertheless, such as they have been, they are the most valuable contributions thus made, in the opinion of the present writer, and the more of such we have the better.

If to such a study another were added, in which the effect of altering conditions from time to time with the special object of testing the results on an animal or animals similarly closely observed from birth onward, we should have another most valuable contribution to comparative psychology; but experiment on animals whose history is unknown must, in the nature of the case, be very much less valuable than in such an instance as that just supposed.

As Professor Groos has suggested in a private communication to me, it is important to make observations on wild animals, and there seems to be room for the worker in comparative psychology in zoological gardens as well as in the field or forest. But I must again maintain that it is fact rather than theory—observation, as ordinarily understood, and experiment—that are more needed than anything else as yet.

#### RÉSUMÉ.

Comparative psychology is advanced rather by systematic observations and experiments than by anecdotes; nevertheless, the latter, when strictly true, are not valueless.

The study of the development of the animal mind (genetic psychology) is of the highest importance.

Insufficient attention has been paid to distinguishing between normal, subnormal and super-normal comparative psychology;



an objection, however, which applies with a certain degree of force to human psychology.

In making experiments on animals it is especially important that they should be placed under conditions as natural as possible. The neglect of this is a fatal objection to the work of the author of 'Animal Intelligence,' published as a monograph supplement to the *PSYCHOLOGICAL REVIEW*, Vol II., No. 8, 1898.

The portion of this research referring to chicks is the most reliable, and the suggestions as to pedagogics, etc., valuable.

This investigator's experiments show that certain associations may be formed under conditions highly unnatural, which associations bear about the same relation to the normal psychic evolution of animals as the behavior of more or less panic-stricken or otherwise abnormal human beings does to their natural conduct.

It is not proved, as asserted in the publication referred to, that animals do not imitate, remember, have social consciousness, imagination, association, and perception; nor that their consciousness is only comparable to that of a human being during swimming or when playing out-door games, as understood by this writer.

It is highly probable that animals, even the highest below man, have only rarely and at the best but a feeble self-consciousness, if it exist at all.

But on this point and on the question of inference, reasoning, etc., the time is not yet ripe for positive assertions.

It seems more than probable that the mental processes of the highest animals are not radically different from those of men so far as they go, but that the human mind has capacities in the realms both of feeling and intellection to which animals cannot attain. While it is desirable to push analysis as far as possible it is safer to remain in the region of the indefinite, to refrain from making very precise and positive statements as to whether the animal mind does or does not possess certain powers, till we are in possession of a larger storehouse of facts, especially of the nature of exact and systematic observations (or experiments). *Festinate lente* is a good rule to observe in regard to *conclusions* in comparative psychology.

## THE DEVELOPMENT OF VOLUNTARY MOVEMENT.

BY PROFESSOR E. A. KIRKPATRICK.

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The work of a train dispatcher who has to direct the movements and stoppages of a few score of trains so that there will be no delays or collisions is justly regarded as very difficult, and the brightest minds must go through years of training before they are equal to the task. The child, however, who gets up from his play and brings us a book, and then resumes his seat, performs an act of much greater complexity and nicety of adjustment, for as large a number of muscles as trains are moved, and an impulse passes to and from each muscle; all these movements and adjustments take place in a few seconds, and a variation of a fraction of a second in the order of contraction interferes with the grace and accuracy of the movements as much as the variation of a fraction of an hour in the time of trains interferes with their successful movement.

Without previous practice, pigs, chickens and many other animals can coördinate visual sensations and movements so as to walk or run, avoiding obstacles and adapting themselves to the nature of the ground. Young chickens can move towards and pick up food with only a little less accuracy than adult chickens. The human infant has not such power of motor control at birth, and our problem is to determine how he comes into possession of it within a year or two. *First*, it is popularly thought that he learns how to make the movements; *second*, it may be claimed that the power to make such movements is inherited, just as it is in the case of the chicken, except that the mechanism is not complete for some time after birth, as is known to be the case with birds as regards flying; *third*, it may be claimed that the movements are partially provided for by the inherited mechanism and partly acquired and learned.

As to the first supposition, the evidence is overwhelmingly against the possibility of such a stupendous task being performed by a child in the short space of a few years. He has over four hundred muscles, and these may be combined in practically an infinite number of different ways. If it depended entirely upon chance or the child's ingenuity whether he should find the right combination for any movement, as reaching for a ball and passing it from one hand to the other, he might work during his whole lifetime at that one puzzle before he would be likely to solve it by getting exactly the right muscles and in the right combinations.

The second theory, though contrary to ordinary observation and opinion, has many facts to support it. For example, it is well known that children and even adults who have never learned to swim sometimes succeed in swimming ashore when left in the water with nothing to do but sink. Fathers sometimes use this method of teaching their sons to swim. Many parents have noticed that their children learned to walk and run with surprising rapidity after they began. The most striking instance of this kind coming under my notice is thus described for me by the father, Supt. Hall, of North Adams, Mass.

“In reply to yours of March 25th, I give you the following account of how my little daughter Katherine learned to walk. She was the youngest of a family of five. The other children had learned to walk soon after they were a year old, and in the normal fashion by being encouraged to put forth a series of efforts until they were able to go alone. Katherine was a normal child in other respects, bright, active and healthy, yet unable to walk a step when she was seventeen months old. Of course, we were anxious, fearing that the cause of this inefficiency might be physical, especially as she persisted in crawling and absolutely refused to try to help herself under the encouragement of any assistance.

“At last we referred the matter to a physician, who said: ‘It is a peculiar case, and I can hardly tell whether the difficulty is physical or mental. If there is no improvement in a short time, call me again.’ Shortly afterwards I came home



one day at noon, and, placing my cuffs on a table in the sitting-room, threw myself on a lounge to rest. Katherine happened to notice the cuffs from where she sat on the floor, and, crawling across the room, pulled herself up by the leg of the table, and, reaching out with one hand while she held on to the table with the other, took a cuff off from the table and slipped it on over her wrist. Of course, to do this she had to stand alone. I noticed it at once, and was surprised when she reached out her other hand for the other cuff and slipped that on, and then stood looking in a very interested way at the cuffs on both wrists. Then, to our great surprise, she turned towards me with a very pleased expression on her face and walked as confidently and easily as any child could. Not only this, but she immediately ran across the room, through another room and around through the hall-way, not simply walking, but running as rapidly as a child four or five years of age would. What surprised us most was that she did not seem to be wearied by her effort at all.

“We allowed her to keep the cuffs on for ten minutes or more, and she was on her feet all the time. At last she sat down a moment, rested, and then, strange to say, got up on both feet without assistance and commenced to run around the room again. As an experiment, I took the cuffs off, and she was as unwilling to try to walk as before. We could not possibly induce her to take a single step without the cuffs. When, however, we allowed her to put them on, she seemed to be greatly delighted and walked and ran as before. The result was that I gave her an old pair of cuffs to put on and allowed her to wear them for two days. This was the only way we could keep her from crawling. After that time she seemed to be able to get along without the cuffs, and has not crawled any since.”

Instances similar to this of sudden acquisition of control of the vocal organs are not unusual. Numerous cases of remarkable movements by somnambulists and by persons frightened or excited are so common that it is sometimes said that instinctive action is more perfect than deliberate action. The fact that such instances are rare, while most children seem to spend considerable time in learning movements, is not positive proof

that such movements as walking and swimming are not inherited movements. It may be claimed that as fast as the nervous and muscular systems develop the child begins making the movements which when combined with others constitute the movements of walking; but that those movements ordinarily looked upon as practice and regarded as the cause of nervous and muscular development are in reality merely the effect and sign of the hereditary perfectment of the nervous and muscular systems which is going on. Such movements as those of walking and swimming may, therefore, be wholly hereditary, but it seems reasonable to suppose that the development of those movements is hastened and in part produced by practice, and certainly it cannot be claimed that all the various movements of work and play which human beings perform are inherited rather than acquired, especially when it comes to the manipulation of tools.

The third theory is the one more commonly held by psychologists and physiologists, and in a general way is probably the most nearly correct, but in my judgment it needs to be modified in the direction of the last and made much more definite. Observation of young children has shown clearly that the infant inherits the power to make many reflex, instinctive, expressive and impulsive movements, and that these simpler movements are combined in performing the various voluntary movements which he afterwards performs. It seems to be the common opinion that chance and imitation are important factors in effecting such combinations, while some seem to think that the child learns the simpler movements and then by an act of constructive imagination combines them in the proper way to effect his purposes. Professor Baldwin, who has perhaps contributed more than any one else to the subject, has in part eliminated chance by showing that there is a tendency in every organism so to act as to continue, increase or repeat favorable stimuli. The performance, repetition and perfectment of a movement do not, therefore, depend entirely upon the chance production or repetition of the stimulus by the environment, but the tendency in the animal to the circular form of reaction causes the stimulus to be repeated again and again. He does not, however, make sufficiently clear



the physiological basis of this tendency, and he seems to allow too large an element of chance in the determination of the course of nervous impulses within the organism. When a child repeats again and again a sound, as children so often do in the third quarter of the first year, it must be because the auditory sensory center is in closer connection with the motor center for the vocal organs than with any other motor center; otherwise the limbs would be just as likely to move as the vocal organs. More than this, the sensory center for that sound must be more closely connected with the center for producing it, or else any other sound would be just as likely to be made. There are probably more than a score of muscles concerned in articulation, and only when just the right ones contract in just the right degree will a given sound be produced; hence the number of different combinations mathematically possible is hundreds of millions. It cannot, therefore, be a matter of chance when a child repeats, after a few trials, a sound that he has heard; but it must depend upon physiological structure that makes the path more open between certain auditory centers and corresponding motor speech centers. Again, when a child imitates a movement he sees, it must be because there is a connection between the visual sense center and the motor center for moving the part in a corresponding way. Of course, it is a familiar fact that there is an excess of motor energy set free in all attempts to make new movements, especially in the case of children, which causes many other than the necessary muscles to contract; but physiological openness of certain paths rather than chance determines which movements shall be selected for repetition.

The next point which I wish to emphasize is that there is an inherited physiological space relation between the visual stimulus of an object in a certain position and the muscles for moving to that object. A young chicken succeeds in picking up a grain of meal, not because he mentally judges the direction and distance, but because the visual sensation calls the right muscle into play. In a similar way, a child grasps an attractive object, not because he knows its direction and distance, but because the visual sensation calls the proper muscles into play. So accurate is this physiological relation between visual sensations and move-



ments that, though I experimented frequently from the time she began to grasp at about three months, I never succeeded in getting my little girl to try to grasp an object more than four or five inches beyond her reach, and rarely so far as that. She would stretch her hands towards more distant objects that she wanted, but not with the grasping movement. The direction of her movement was also from the first nearly as accurate as the fixation with her eyes. This physiological space relation of certain motor reactions to certain stimulations is, in my judgment, of an importance hitherto unappreciated in explaining not only the development of voluntary movements, but also in explaining ideas of space.

Close observation of the earliest attempts at grasping convinced me that the only element prominent in consciousness at first is the visual sensation of the object. After it has been reached, reflexively grasped when touched and instinctively brought to the mouth several times, disappointment is shown if the hand, instead of the object, comes in contact with the lips, showing that there was then expectation of a certain kind of sensation that was not realized. The young child in grasping objects has a sensation or image of the object in a certain position and an image of a sensation to be gotten; but according to my observations there is no evidence that his consciousness is concerned at all with the movements he is making in order to get hold of the object and bring it to him. The same is true of all the earlier voluntary movements of the child, and attention to the movement itself hinders rather than helps in learning the movement. In the case of Superintendent Hall's little girl there was inability to walk so long as she thought about her movements; but as soon as her attention was concentrated upon getting the cuffs on and carrying them around she succeeded perfectly, though she had never tried it before.

Every adult knows that if he thinks about how he is doing a thing he can do it much less perfectly than when he thinks merely about what he wants to do; yet it is a common belief that one in learning any act must go through a stage of quite acute consciousness of the movements involved. I maintain, on the contrary, that children do not ordinarily go through any

such stage in learning their earlier movements, and that it is not usually necessary for either children or adults to go through such a stage of consciousness of all, or even of a large proportion, of the elementary movements involved in the new act.

I have not time to give facts in support of this conclusion, nor to point out its importance in the solution of various educational problems ; but I will close with one or two general considerations. In the history of the race arts have always preceded sciences ; men have learned to do things, then reflected upon how they do them, analyzed to discover elements, then determined the general laws according to which the actions may be successfully performed, and this order of procedure is the natural one to the child. It is possible that in some cases short cuts may be taken, as Baldwin has suggested, and possibly the order may sometimes be reversed and time saved. A person who knows one language, for example, may possibly learn another language more quickly by studying its grammar first ; but I am sure that a child who knows no language could not learn one by beginning with the grammar. Adults who are able to make many movements may learn more quickly a new movement by having attention called to some of the elements, though probably never by having it called to all ; but a young child would be hindered rather than helped by such a process. This is true, not simply because of the general tendency of the mind to develop in this order, but because the past experience of the race has developed a very definite system of relations between various stimuli and various simple movements, and has probably developed less definitely various combinations of simple movements and a tendency to other combinations in the attainment of ends frequently striven for by the race. The teaching of a movement by having each of its elements learned, and then having these elements combined and used, is not only a reversal of the natural order in attaining an end and a misdirection of attention, but is an undoing of what has been partially done by the experience of our ancestors, instead of completing the process.

## THE INSTINCTIVE REACTION OF YOUNG CHICKS.

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The data to be presented in this article were obtained in the course of a series of experiments conducted in connection with the psychological laboratory of Harvard University during the year '96-'97. About sixty chicks were used as subjects. In general their experiences were entirely under my control from birth. Where this was not true the conditions of their life previous to the experiments were known, and were such as would have had no influence in determining the quality of their reactions in the particular experiments to which they were subjected. It is not worth while to recount the means taken so to regulate the chick's environment that his experience along certain lines should be in its entirety known to the observer and that consequently his inherited abilities could be surely differentiated. The nature of the experiments will, in most cases, be such that little suspicion of the influence of education by experience will be possible. In the other cases I will mention the particular means then taken to prevent such influence.

Some of my first experiments were on color vision in chicks from 18 to 30 hours old, just old enough to move about readily and to be hungry. On backgrounds of white and black cardboard were pasted pieces of colored paper about 2 mm. square. On each background there were six of these pieces,—one each of yellow, red, orange, green, blue and black (on the white ground) or white (on the black). They were in a row about half an inch apart. The chicks had been in darkness for all but three or four hours of their life so far. During those few hours the incubator had been illuminated and the chicks had that much chance to learn color.



The eight chicks were put, one at a time, on the sheet of cardboard facing the colored spots. Count was kept of the number of times that they pecked at each spot and, of course, they were watched to see whether they would peck at all at random. In the experiments with the white background all the colors were reacted to (*i. e.*, pecked at) except black (but the letters on a newspaper were pecked at by the same chicks the same day). One of the chicks pecked at all five, one at four, three at three, one at two and one at yellow only. These differences are due probably to accidental position or movements. Taking the sums of the reactions to each color-spot we get the following table :

I.	Times reacted to.	Total number of pecks. <sup>1</sup>
Red,	12	31
Yellow,	9	21
Orange,	6	34
Green,	5	11
Blue,	1	3

I should attach no importance whatever to the quantitative estimate given in the table. The only fact of value so far is the evidence that from the first the chick reacts to all colors. In no case was there any random pecking at the white surface of the cardboard.

On a black background the same chicks reacted to all the colors.

II. is a table of the results.

II.	Times reacted to.	Total number of pecks.
White,	6	19
Blue,	4	11
Red,	4	8
Green,	4	4
Orange,	2	7
Yellow,	2	4

In other experiments chicks were tried with green spots on a red ground, red spots on a green ground, yellow spots on an orange ground, green spots on a blue ground, and black spots

<sup>1</sup>This double rating is necessary because of the fact that the chick often gives several distinct pecks in a single reaction. The 'times reacted to' mean the number of different times that the chicks noticed the color.

on a white ground. All were reacted to. Thus, what is apparently a long and arduous task to the chick is heredity's gift to the chick. It is conceivable, though to me incredible, that what the chick reacts to is not the color, but the very minute elevation of the spot. My spots were made so that they were only the thickness of thin paper above pasteboard. Any one who cares to resort to the theory that this elevation caused the reaction can settle the case by using color-spots absolutely level with the surface.

#### INSTINCTIVE REACTIONS TO DISTANCE, DIRECTION, SIZE, ETC.

I have purposely chosen this awkward heading rather than the simple one Space-Perception, because I do not wish to imply that there is in the young chick such consciousness of space-facts as there is in human beings. All that will be shown here is that he reacts appropriately in the presence of space-facts, reacts in a fashion which would in the case of a man go with genuine perception of space.

If one puts a chick on top of a box in sight of his fellows below, the chick will regulate his conduct by the height of the box. To be definite, we may take the average chick of about 95 hours. If the height is less than 10 inches he will jump down as soon as you put him up. At 16 inches he will jump in from 5 seconds to 3 or 4 minutes. At 22 inches he will still jump down, but after more hesitation. At 27  $\frac{1}{2}$  inches 6 chicks out of eight at this age jumped within 5 minutes. At 39 inches the chick *will NOT jump down*. The numerical values given here would, of course, vary with the health, development, hunger and degree of lonesomeness of the chick. All that they are supposed to show is that at any given age the chick without experience of heights regulates his conduct rather accurately in accord with the space-fact of distance which confronts him. The chick does not peck at objects remote from him, does not, for instance, confuse a bird a score of feet away with a fly near by, or try to get the moon inside his bill. Moreover, he reacts in pecking with considerable accuracy at the very start. Lloyd Morgan has noted that in his very first efforts the chick often fails to see the object, though he hits it,

and on this ground has denied the perfection of the instinct. But, as a matter of fact, the pecking reaction may be as perfect at birth as it is after 10 or 12 days' experience. It certainly is not perfect then. I took nine chicks from 10 to 14 days old and placed them one at a time on a clear surface over which were scattered grains of cracked wheat (the food they had been eating in this same way for a week) and watched the accuracy of their pecking. Out of 214 objects pecked at 159 were seized, 55 were not. Out of the 159 that were seized, *only 116* were seized on the first peck, 25 on the second, 16 on the third, and the remaining two on the fourth. Of the 55 that were not successfully seized, 31 were pecked at only once, 10 twice, 10 three times, 3 four times and 1 five times. I fancy one would find that adult fowls would show by no means a perfect record. So long as chicks with ten days' experience fail to seize on the first trial 45% of the time, it is hardly fair to argue against the perfection of the instinct on the ground of failures to seize during the first day.

The chick's practical appreciation of space facts is seen further in his attempts to escape when confined. Put chicks only twenty or thirty hours old in a box with walls three or four inches high and they will react to the perpendicularity of the confining walls by trying to jump over them. In fact, in the ways he moves, the directions he takes and the objects he reacts to, the chicken has prior to experience the power of appropriate reaction to colors and facts of all three dimensions.

#### INSTINCTIVE MUSCULAR COÖRDINATIONS.

In the acts already described we see fitting coördinations at work in the chick's reactions to space facts. A few more samples may be given. In jumping down from heights the chick does not walk off or fall off (save rarely), but jumps off. He meets the situation "loneliness on a small eminence" by walking around the edge and peering down; he meets the situation "sight of fellow chicks below" by (after an amount of hesitation varying roughly with the height) jumping off, holding his stubby wings out and keeping right side up. He lands on his feet almost every time and generally very cleverly. A four days' chick



will jump down a distance eight times his own height without hurting himself a bit. If one takes a chick two or three weeks old who has never had a chance to jump up or down, and puts him in a box with walls three times the height of the chick's back, he will find that the chick will jump, or rather fly, nearly, if not quite, over the wall, flapping his wings lustily and holding on to the edge with his neck while he clammers over. Chicks one day old will, in about 57 per cent. of the cases, balance themselves for five or six seconds when placed on a stiff perch. If eight or nine days old, they will, though never before on any perch or anything like one, balance perfectly for a minute or more. The muscular coördination required is invoked immediately when the chick feels the situation "feet on a perch." The *strength* is lacking in the first few days. From the fifth or sixth day on chicks are also able (their ability increases with age) to balance themselves on a slowly swinging perch.

Another complex coördination is seen in the somewhat remarkable instinct of swimming. Chicks only a day or two old will, if tossed into a pond, head straight for the shore and swim rapidly to it. It is impossible to compare their movements in so doing with those of ducklings, for the chick is agitated, paddles his feet very fast and swims to get out, not for swimming's sake. Dr. Bashford Dean, of Columbia University, has suggested to me that the movements may not be those of swimming, but only of running. At all events, they are utterly different from those of an adult fowl. In the case of the adult there is no vigorous instinct to strike out toward the shore. The hen may try to fly back into the boat if it is dropped overboard, and whether dropped in or slung in from the shore will float about aimlessly for a while and only very slowly reach the shore. The movements the chick makes do look to be such as trying to run in water might lead to, but it is hard to see why a hen shouldn't run to get out of cold water as well as a chick. If, on the other hand, the actions of the chick are due to a real swimming instinct, it is easy to see that, being unused, the instinct might wane as the animal grew up.

Such instinctive coördinations as these, together with the walking, running, preening of feathers, stretching out of leg

backwards, scratching the head, etc., noted by other observers, make the infant chick a very interesting contrast to the infant man. That the helplessness of the child is a sacrifice to plasticity, instability and consequent power to develop we all know; but one begins to realize how much of a sacrifice when one sees what twenty-one days of embryonic life do for the chick brain. And one cannot help wondering whether some of the space-perception we trace to experience, some of the coördinations which we attribute to a gradual development from random, accidentally caused movements may not be more or less definitely provided for by the child's inherited brain structure. Walking has been found to be instinctive; why not other things?

#### INSTINCTIVE EMOTIONAL REACTIONS.

The only experiments to which I wish to refer at length under this heading are some concerning the chick's instinctive fears. Before describing them it may be well to mention their general bearing on the results obtained by Spalding and Morgan. They corroborate Morgan's decision that no well-defined specific fears are present; that the fears of young chicks are of strange moving objects in general, shock in general, strange sounds in general. On the other hand, no such general disturbances of the chick's environment led to such well-marked reactions as Spalding described. And so when Morgan thinks that such behavior as Spalding witnessed on the part of the chick that heard the hawk's cry demands for its explanation nothing more than a general fear of strange sounds, my experiments do not allow me to agree with him. If Spalding really saw the conduct which he says the chick exhibited on the third day of its life in the presence of man, and later at the stimulus of the sight or sound of the hawk, there are specific reactions, for the running, crouching, silence, quivering, etc., that one gets by yelling, banging doors, tormenting a violin, throwing hats, bottles, or brushes at the chick is never anything like so pronounced and never lasts one-tenth as long as it did with Spalding's chicks. But as to the fear of man, Spalding must have been deluded. In the second, third and fourth days there is no such reaction to the sight of man as he thought he saw.

Miss Hattie E. Hunt, in the *American Journal of Psychology*, Vol. IX., No. 1, asserts that there is no instinctive fear of a cat. Morgan did not find such. I myself put chicks of 2, 5, 9 and 17 days (different individuals each time, 11 in all) in the presence of a cat. They showed no fear; went on eating as if nothing about. The cat was still, or only slowly moving. I further put a young kitten (eight inches long) in the pen with chicks. He felt of them with his paw, and walked around among them for five or ten minutes, yet they showed no fear (nor did he instinctively attack them). If, however, you let a cat jump at chicks in real earnest, they will not stay to be eaten, but will manifest fear—at least chicks three to four weeks old will. I did not try this experiment with a lot of chicks at different ages, because it seemed rather cruel and degrading to the experimenter. When in the case of the older chicks nature happened to make the experiment, it was hard to decide whether there was more violent fear of the jumping cat than there was when one threw a basket or foot-ball into the pen. There was not very much more.

We may now proceed to a brief recital of the facts shown by the experiments in so far as they are novel. It should be remembered throughout that in every case chicks of different ages were tested so as to demonstrate transitory instincts if such existed—*e. g.*, the presence of a fear of flame was tested with chicks 59 and 60, one day old, 30 and 32, two days old, 21 and 22, three days old, 23 and 24, seven days old, 27 and 29, nine days old, 16 and 19, eleven days old, and so on up to twenty days old chicks. By thus using different subjects at each trial one, of course, eliminates any influence of experience.

The first notable fact is that there develops in the first month a general fear of novel objects in motion. For four or five days there seems to be no such. You may throw a hat or slipper or shaving-mug at a chick of that age, and he will do no more than get out of the way of it. But a twenty-five days old chick will generally chirr, run and crouch for five or ten seconds. My records show this sort of thing beginning about the tenth day, but it is about ten days more before it is very marked. In general, also, the reaction is more pronounced if a lot of chicks are



together, and is then displayed earlier (only two at a time were taken in the experiments the results of which have just been quoted). Thus the reaction is to some degree a social performance, the presence of other chicks combining with the strange object to increase the vigor of the reaction. Chicks ordinarily scatter apart when they thus run from an object.

One witnesses a similar gradual growth of the fear of man (not as such probably, but merely as a large moving object). For four or five days you can jump at the chick, grab at it with your hands, etc., without disturbing it in the least. A chick twenty days old, however, although he has never been touched or approached by a man, and in some cases never seen one except as the daily bringer of food, and has never been in any way injured by any large moving object of any sort, will run from you if you try to catch him or even get very near him. There is, however, even then, nothing like the utter fear described by Spalding.

Up to thirty days there was no fear of a mocking bird into whose cage the chicks were put, no fear of a stuffed hawk or a stuffed owl (kept stationary). Chicks try to escape from water (even though warmed to the temperature of their bodies) from the very first. Up to forty days there appears no marked waning of the instinct. They did not show any emotional reaction to the flame produced by six candles stuck closely together. From the start they react instinctively to confinement, to loneliness, to bodily restraint, but their feeling in these cases would better be called discomfort than fear. From the 10th or 12th to the 20th day, and probably later and very possibly earlier, one notices in chicks a general avoidance of open places. Turn them out in your study and they will not go out into the middle of the room, but will cling to the edges, go under chairs, around table-legs and along the walls. One sees nothing of the sort up through the fourth day. Some experiments with feeding hive bees to the chicks are interesting in connection with the following statement by Lloyd Morgan: "One of my chicks, three or four days old, snapped up a hive bee and ran off with it. Then he dropped it, shook his head much and often, and wiped his bill repeatedly. I do not think he had been stung; *probably*

*he tasted the poison.*" (*Int. to Comp. Psy.*, p. 86.) I fed seven bees apiece to three chicks from ten to twenty days old. *They ate them all greedily*, first mashing them down on the ground violently in a rather dextrous manner. Apparently this method of treatment is peculiar to the object. Chicks *three* days old did not eat the bees. Some pecked at them but none would snap them up, and when the bee approached they sometimes sounded the danger-note. Finally an account may be given of the reaction of chicks at different ages, up to twenty-six days, to loud sounds. These were the sounds made by clapping the hands, slamming a door, whistling sharply, banging a tin pan on the floor, mewing like a cat, playing a violin, thumping a coal-scuttle with a shovel, etc., etc. Two chicks were together in each experiment. Three-fourths of the times no effect was produced. On the other occasions there was some running or crouching or, at least, starting to run or crouch; but, as was said, nothing like what Spalding reports as the reaction to the 'cheep' of the hawk. It is interesting to notice that the two most emphatic reactions were to the imitation mew. One time a chick ran wildly, chirring, and then crouched and stayed still until I had counted 105. The other time a chick crouched and stayed still until I counted 40. But the other chick with them did not and in a dozen other cases the 'meaw' had no effect.

I think that the main interest of most of these experiments is the proof they afford that instinctive reactions are not necessarily definite, perfectly appropriate and unvarying responses to accurately sensed and, so to speak, estimated stimuli. The old notion that instinct was a God-given substitute for reason left us an unhappy legacy in the shape of the tendency to think of all inherited powers of reaction as definite particular acts invariably done in the presence of certain equally definite situations. Such an act as the spider's web-spinning might be a stock example. Of course, there are many such instinctive reactions in which a well-defined act follows a well-defined stimulus with the regularity and precision with which the needle approaches the magnet. But our experiments show that there are acts just as truly instinctive, depending in just the same way on inherited brain-structure, but characterized by being vague, irregular, and, to some extent, dissimilar reactions to vague, complex situations.

The same stimulus doesn't always produce just the same effect, doesn't produce precisely the same effect in all individuals. The chick's brain is evidently prepared in a general way to react more or less appropriately to certain stimuli, and these reactions are among the most important of its instincts or inherited functions. But yet one cannot take these and find them always and everywhere. This helps us further to realize the danger of supposing that in observation of animals you can depend on a rigid uniformity. One would never suppose because one boy twirled his thumb when asked a question that all boys of that age did. But naturalists have been ready to believe that because one young animal made a certain response to a certain stimulus, the thing was an instinct common to all in precisely that same form. But a loud sound may make one chick run, another crouch, another give the danger call, and another do nothing whatever.

In closing this article I may speak of one instinct which shows itself clearly from at least as early as the sixth day, which is preparatory to the duties of adult life and of no other use whatsoever. It is interesting in connection with the general matter of animal play. The phenomenon is as follows: The chicks are feeding quietly when suddenly two chicks rush at each other, face each other a moment and then go about their business. This thing keeps up and grows into the ordinary combat of roosters. It is rather a puzzle on any theory that an instinct needed so late should begin to develop so early.



## DISCUSSIONS.

### PROFESSOR MÜNSTERBERG ON MYSTICISM.

The criticism of 'The New Psychology,' it seems, has a sequel. We have an equal and presumably impartial attack upon Mysticism, of which one form is psychical research. In taking up the cudgels, however, I am not going to defend this curious department of inquiry. Even among those who are interested in it there is room enough for scepticism of the most scrutinizing sort. I accord any man whatever opinion he pleases to have about it. But I should ask that the scientific method that Professor Münsterberg demands in this and all psychological work be represented in his criticism, or a frank admission made that dogmatism is the fundamental instrument of knowledge. To me his recent article in the *Atlantic Monthly* is one of the most amusing documents that I have ever had the pleasure of reading. I am not going to attack the discussion as a whole, but only to deal with that part of it which criticises psychical research. Let us see how much science there is in his method.

Professor Münsterberg in one passage confesses that until the last summer vacation he felt rather guilty for forming and stating opinions on this subject before reading its literature. He then proceeds to enjoy his vacation 'in working through more than a hundred volumes of the so-called evidence.'!!! Just think of that! A scientist spending the summer rest of a few weeks reading more than one hundred volumes of matters involving a question of evidence, and actually forming what he thinks a scientific conclusion on them!! I do not believe there are twenty-five volumes in existence on this subject that any sane man ought to read at all, let alone doing it at such a time. I have watched this subject for ten years, and have in all that time read no more than ten volumes, some of them exceedingly carefully, and I did not dream of forming an opinion or irreversable conclusion upon them. On this subject of psychical research, unless you have made sufficiently decisive experiments personally (not merely curing one hallucination by suggestion), it may take a hundred years to arrive at any scientific conclusion at all. But would Professor Münsterberg advise his students to study psychology generally at the rate of 'more

than a hundred volumes' a vacation when the temperature is between eighty and a hundred? Moreover, what right has a professed scientist to depend upon books, no matter how many of them, for a conclusion that involves matters of very delicate experiment, and not analytical and introspective methods? Professor Münsterberg says that he is not a detective. He should then not pronounce upon problems that require that sort of ability. Here is a place for a confession of ignorance and to eschew the pretensions of knowledge.

Apropos of this last remark it is well to recall another singular confession of our author. His reason for not making a personal investigation into this question is that it is not 'dignified to visit such performances' as séances!! If physiology and biology had acted on this maxim we should have known very little about life on the one hand, and about brain processes on the other, on which Professor Münsterberg relies so much for his assurance against mysticism. Dignity is not anything that should stand in the way of experiment or exact method. I confess I admire Darwin for playing a bassoon to his garden plants to test some supposition, though his neighbors, had they seen him at it, would have thought him suitable for a lunatic asylum. Science at one time was too dignified to examine the stories about falling meteors, but it came to terms at last. It did the same with hypnotism. It first packed a jury to condemn it, and thought it had laid the monster, but after forty years contempt decided to embrace it as a fact nevertheless!! Its dignity would not save its scepticism.

It seems, again, that Professor Münsterberg cannot protect himself against fraud. He thinks the scientist is trained to 'an instinctive confidence in his coöperators.' Granted. All scientific truth involving the coöperation of others, then, must be taken on authority. Everything depends upon the assurance of men that there is no fraud who have either not looked for it or are not able to detect it!! When science comes to that pass I shall have done with it. A man who cannot protect himself against fraud must not expect his opinion to be worth very much. He may read 'more than a hundred volumes' in his vacation and form theories in that way, but he must not expect us to take his experimental work seriously.

Let us have some science. "If I talk with others whom I wish to convince there is no physical process in question, mind reaches mind, thought reaches thought, but in this aspect thoughts are not psychophysical phenomena in space and time, but attitudes and propositions in the sphere of the will." Well, this is either telepathy with a vengeance or it is blank nonsense. Just think of the statement that

there is no physical process in the communication of thoughts!! Where is the evidence for all this? Can science escape the demand for fact to prove an assertion? What facts has Professor Münsterberg to show that this view is either true or intelligible? Then, what does he mean by a 'proposition in the sphere of the will'? While we are playing 'ducks and drakes' with the language of science, why not go further and say that fear is a feeling in the sphere of logic? As to what Professor Münsterberg may intend by this description of the communication of ideas, I can well imagine. But I can do it only by having some knowledge of the process myself, and not from any statement that he makes. When I wish to transmit my thought to others by talking I make a disturbance in the air, and the receiver interprets the sound. Now, if 'communication' be convertible with 'interpretation' we may agree that there 'is no physical process in question,' but in all intelligible parlance, outside the suppositions of telepathy, 'communication' means that the physical process is a part of the totality. Otherwise there is no interpretation even, and the only resource for common thoughts would be universal telepathy, which Professor Münsterberg will not admit as possible. And neither for nor against one or the other of the claims does he produce any facts!! It is simply bald blank assertion, and this is supposed to be science after laughing at the dogmatism of the Middle Ages!

We have another illustration of the same sort of thing. "The ethical belief in immortality means that as subjects of will we are immortal; that is, that we are not reached by death. For the philosophical mind, which sees the difference between reality and psychological transformation, immortality is certain; for him the denial of immortality would be even quite meaningless. Death is a biological phenomenon in the world of objects in time; how, then, can death reach a reality which is not an object, but an attitude, and, therefore, neither in time nor space? Our real inner subjective life has its felt validity, not in time, but beyond time: it is eternal." This is science, I suppose!! Not a fact to prove it. It is said that 'philosophy' shows this. Whose philosophy? On what facts is it founded? Then, again, *what* is immortal? We are not told what it is. From a previous reference to the 'ethical belief' Professor Münsterberg says that it ends in mysticism, and I imagine that what he says of it here is intended to be condemned as compared with the philosophic verdict. If so it cannot be the subject of will, and if it is not this we have immortality affirmed without telling us what is immortal. But assuming it is 'we as subjects of will' that are immortal, what is this 'we,'



especially when an earlier passage asserts that the 'inner reality,' which is here said to be eternal, 'never consists of psychological phenomena.' But this sort of criticism aside as savoring of quibbles, I must press the scientific demand for fact to show that the tremendous assertion here made has another basis than the mere speculative opinion of the author. As for myself I must contend that there is not one iota of rational evidence for immortality, of any intelligible or desirable kind, outside the sphere and method of psychical research. I do not maintain that even this is rational, but it is all that can lay the slightest claim to being rational from the standpoint of science, and the philosophic standpoint I absolutely reject as merely a process of looking into one's navel to solve the riddle of the Sphinx. Heaven knows that the spiritualist's 'scientific' evidence for his belief has been meager and poor enough, but the philosopher's has been worse. It has rested mainly on 'dignity' and 'dignified' methods, tempered with equivocation and hypocrisy to escape persecution. I follow the method and accept the verdict of science on this matter. If it gives me trustworthy facts making immortality a rational belief, I can affirm it; if it cannot produce these facts I either suspend judgment or accept the probability, from the connection of consciousness with a perishable organism, that this function dissolves with it. And when I speak of immortality I mean personal survival; that is, the continuance of consciousness beyond the life of the body. Any other immortality I do not care a picayune for, and would not be caught juggling with any affirmative proposition containing the term. What amazes me is that any man making the slightest pretense to scientific method would, after the terrible lesson of scholasticism, attempt for a moment to make such a tremendous assertion as that of immortality without at least a small array of empirical facts to support it. There is another very singular passage. After telling us in fine language that science must not prejudge a question, must not 'reject a fact because it does not fit into the scientific system of to-day,' etc., Professor Münsterberg goes on to say: "This is the old text," etc., "Yet it is wrong and dangerous from beginning to end, and has endlessly more harm in it than a superficial view reveals, as it is in last consequences not only the death of real science, but worse, the death of real idealism." Well, we have to choose between psychical research and idealism. But what is idealism? Is that so clear in these times that men have no freedom to question it? When I read a book or essay on idealism I am reminded of the sermon which the old woman could not understand, but which, nevertheless, edified and consoled her by

the presence in it of 'the blessed word Mesopotamia.' Idealism is unintelligible, but then it is the basis of ethics and art! I say frankly that if I had to choose between psychical research and idealism I should unhesitatingly take the side of psychical research for clearness and knowing 'where you are at.' For I do not know any field of thought which is more full of intellectual hobgoblins than that of Kanto-Hegelian idealism. I am not opposing idealism, because if I am allowed to define it for myself I should say that it is a mere truism. It is to me like the proposition that water is wet or blue is a color. But I do not expect to solve any problems with it. Least of all, do I consider it a sanctuary in which I am not allowed to say anything about either materialism or spiritualism. The only way that idealism can get into antagonism with any theory is to limit itself to solipsism. In any other form it is only a field for that kind of intellectual gymnastics which, as Kant remarks, characterizes the heroes of Valhalla. They are forever hewing down shadows which only spring up again to renew their ceaseless and bloodless conflict.

But the most remarkable thing about this passage is its distinct renunciation of scientific method for a dogmatism that knows all about the universe without any further inquiry. I do not see why a man talks any more about 'science' and 'scientific' method as an enemy of superstition when he shows that he has no other conception of it than that which denies the right to revise existing opinion. Evidently, science and dogmatism are the same here, while idealism is 'that blessed word' which is to exorcise all spirits except its own, and they are as shadowy as the ghosts that inhabit Homer's Cimmerian shades.

Taking the article as a whole, I do not see why Professor Münsterberg did not distinguish between the relevancy of the various alleged phenomena that he was criticising. Table turning, telepathy, clairvoyance, hypnotism and what not were lumped together with no more conception of their differences than is usually displayed by the spiritualist himself. The fact is that not one of them, unless we except telepathy, even if they were genuine, has any bearing on the question of spiritualism, and telepathy, if true, might be used as a very effective bar to spiritualism. But as in the phenomena of insanity and hallucinations, which, by the way, Professor Münsterberg is not too dignified to study, we can classify alleged facts and discuss their relevancy to the hypotheses which they are said to support. Professor Münsterberg should have read that hundred volumes with sufficient care to discover the distinction that a scientist ought to master at first. There is no use to assume that the spiritualist has the right conception



either of his problems or of his facts. I consider that he has neither, as a rule, and it would save some reputation if these alleged phenomena could be treated as patiently as are those of insanity. I am here defending only the method of psychical research. I do not care what becomes of its facts or alleged phenomena. I merely ask that its critics deal with it from the inside, and not in a confessedly *a priori* manner. My attention to it for ten years has convinced me that there is enough in the subject to engage serious consideration, no matter what the conclusions may be. In fact, the plausibility of some tremendous claims is so great, and so thoroughly in accord with what the common mind in this sceptical age would like to see established, that it will require all the severity and sceptical scrutiny of scientific method at our command to get any proper attention to normal psychology. I happen to know some genuine supernormal phenomena, not explainable by either fraud, illusion, or suggestion, and whose significance, or at least plausible significance, will have to be reckoned with by men who, like the mediæval theologians refusing to look through Galileo's telescope, cannot sacrifice their dignity for the sake of controlling a movement instead of following in its wake. Some of those who are making haste to laugh at it without studying it and its alleged facts at first hand will find themselves where they will have either to lose their influence for all psychology or, in order to save it, will have to 'eat crow,' and 'white crow' at that. It is not the remarkable nature of the alleged phenomena of psychical research that gives them so much interest and influence; for the scientific scepticism of the last century has very well fortified the average intelligence against some of the vagaries of spiritualism. But it is the wonderful triumphs of invention and discovery in the fields of both science and art that have destroyed the ordinary criteria of the limits of human knowledge and capacity, so that the average mind is rapidly coming to expect that almost anything is possible. Electricity, the telephone, Roentgen rays, the phonograph, surgery, hypnotism, etc., have opened up such a fairy land of wonders and possibilities to the common mind that it is not surprising to see many otherwise balanced intellects yielding to the claims of spiritualism. Science must reckon with this condition of mind and, instead of employing dogmatism against it, treat its alleged phenomena in the same serious and sympathetic manner that insanity receives. Science has taught us not to burn witches, as they did once, but to put them in asylums. Perhaps the same generous treatment of psychical research may still further extend the operations of humanity. To do this also it will not re-



quire us to spend our summer vacations in reading any very large amount of occult lore.

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### MR. MARSHALL AND THE THEORY OF RELIGION.

Perhaps the most interesting problem with which evolutionary science has to deal is as to the social function of religion. Religion as a very general and large phenomenon could have survived and grown only as a useful element in the struggle of existence of the individual and his society. This evolutionary assumption that what is, subsists and increases only by virtue of function; that natural evolution is an evolution of utilities, and that useless factors are always speedily eliminated in the struggle of existence, is really a close approach to the old doctrine of evidences by which the theologian makes the warrant of religion to be the function which it plays in man's life. For instance, the apologist for prayer has always assured us that such a practice could not have arisen and developed except that it met a real need of human life and was in some way truly answered, and the evolutionist as biologist and sociologist likewise finds that prayer by its very existence shows its validation as an important factor in human life, if not in the way the religionist assumes, at least in some way. It is an interesting fact that, though science, by widening the domain of naturalism indefinitely, has shown that the religions are ineffectual in their methods, yet science, by its own assumption, sees in religion a function which has arisen in the struggle of existence.

Of recent interpretations of religion from the point of view of evolutionary science Mr. H. R. Marshall's 'Instinct and Reason' is the most notable and thoroughgoing. Mr. Marshall finds that evolution is toward organism, which is action of the part for the whole, and instinct is the psychic side of this organic tendency, while reason is the correspondent of individualistic action. The main stress of evolution is to subordinate the individual organ to the organic whole, the eye to minister to the body rather than to itself, the individual body to minister to the perpetuation of the species and of social wholes. But individualism is also a primitive and strong interfering tendency, and so, to give weight to the organic, Nature gives birth to religion as an instinct restraining us from undue individualism. Thus, when inclined to selfish actions religion appears as restraint, and so impulsive to social activity. The earliest sociality has to do with the perpetuation of the

species and the family relations; hence the earliest form of religion is phallicism. Religion is in this and all its later forms fundamentally a 'governing instinct' as suppressing individualism and helping to sociality. This is its utility amidst all its seemingly strange and perverse forms, an eminent utility which is subserved by all expressions of the religious instinct, fasting, prayer, sacrifice, etc.

That social utility is the function whereby religion has persisted and increased in human affairs is a familiar thought, but the merit of Mr. Marshall is that he has given this a large biological setting, and has brought it into line with the evolutionism of to-day. In his theory religion loses its absolutism as worship of the Deity. Religion is "the restraint of individualistic impulses to racial ones," implying that the belief in the Deity as usually found being from the psychological point of view an attachment to, rather than of the essence of, the religious feeling. The conservatism of religion means merely that it is instinct, which is by its nature conservative, and thus religion opposes individualism as an action variant from the general racial forms. Religion appears as restraining influence, an instinctive '*do not*,' even in its most egotistic forms, as it marks a dependence and a certain outward reference of conduct. Thus religion has its value, not in its supposed intrinsic advantage of obtaining good things from a deity, which is mostly fiction and illusion, but as repressive to the lower selfish instincts which tend to make man an independent unsocial being.

Now, in touching upon this theory, we must first remark that it reposes in a large part upon a one-sided view of organism. Mere natural organism is in its origin and early stage not an altruism of part to whole; it is in the struggle of existence a method of advantage by a reciprocity of individuals; a mode of exchange of values whereby the exchanger always seeks to give the least for the most; to get off with as small a *quid pro quo* as possible, or none at all, if the individual is strong enough. The struggle for advantage in organic reciprocity is common to the origin and early progress of organism and of altruism as mere justice. In the crude struggle of existence organic social forms arise and develop to a certain degree of community of interest and reciprocity, but with an intrinsic struggle within the organism itself for dominance by each organ. Societies are very largely of this type even in civilized life, as in the keen rivalry of industrialism and commercialism, which takes every advantage for a bargain. And in every organism under purely natural conditions there is internal disharmony and rivalry as keen in its way and as selfish as the struggle of the individual society with other societies. Hence religion in the sense of



restraint does not appear in pure naturalism, and is not essential to organic activity. The only restraint in elementary societies is weakness; the individual does not take more because he dares not. The social organism as range of reciprocity is at first wholly governed in the measure of reciprocity by force and cunning, and thus religion as restraint cannot be accounted coextensive with organism.

Now, in the primitive social *status*, where a mere competitive reciprocity is the mainspring, religion does in a very real sense exist as direct function as contrasted with the indirect function of restraint. Religion is primarily a method of reciprocity with superiors, a method involved in the struggle of existence, a method of worship, homage, devotion, etc., to find favor and obtain advantage with superiors, human and extra-human. Religion is a mode of socialization, a tie which binds child to parent, wife to husband, vassal to lord, as well as a tie to other superiors (supposedly existing in our view) as ancestors and nature deities. In militarism the direct function of religion is very great, and most very successful military leaders have been largely successful by being able to make themselves worshipped and adored by their men, and so securing perfect obedience, and unity, and dependence. And the leader surrounds himself with godhood by his special relationship to the ancestral and nature deities. It is in this way that what appear to us fictitious deities exercise a real and valuable utility in socialization; that is, by association with the living leader and chief. At the same time it must be recognized that mere religiousness as dependence upon the extra-human superior has been disadvantageous as destroying intelligent self-reliance. The superior and intense religiousness of the Hebrews never made them a great conquering nation. In the military competitive struggle of tribes for existence in West Africa, Miss Kingsley notes that the Fans, a comparatively unreligious tribe, are in the ascendant. In modern times the most successful militarism is not the most religious, but the most scientifically self-reliant. And the tendency is to rely less and less on religious observance, as noting of omens, invoking deities, etc., and to give the time to drill and tactics, and to make the soldier self-reliant in every emergency.

But undoubtedly religion as direct function plays yet a most important function in militarism and in political and social aggregation. A worship and homage bind the masses to the Czar Alexander and to Queen Victoria. Human beings transcendently exalted by their power, intellect, wealth, still enlist a vast amount of religious feeling and activity toward themselves, which unifies society under their absolute leadership.



But the tendency of modern socialization is not a unification of inferiors to supreme superiors, but of equals to equals in democracy. Hence, as the supremely superior is lost, religion as direct social function is lost also. But this form of religion has a survival form in the phrases of courtesy, such as, "I pray you and beg you to accept," by which by politeness we put ourselves as suppliants, but this is a mere ghost of the historic reality. Democracy emphasizes *vox populi* as *vox dei*; hence a positivist religion, religion as worship of humanity, is its natural outcome and its natural binding tie. And this religion directly emphasizes the true organic dependence of the individual and the supremacy of the race as such. But this evolution has scarcely begun, and it throws no light on the historic function of religion, and it can hardly be claimed that the direct function of religion sufficiently accounts for the large and important place of religion in the history of society, but it is still a valuable clue, and one much neglected by Mr. Marshall. Wherever immense superiority has appeared man has sought to ingratiate himself by acts of worship and homage, and this has been, and still is, a successful method and a social tie within the range of living human superiors; but it has been utterly insufficient when applied to what science deems fictitious superiors in environment, as ghosts and nature deities; nor can the religion of these have its full function as merely a background and basis for the living human superior fully to exalt himself and secure worship. Superior though nature be, we now know that the only real adaptation to it is not by the personal method of religion, but by intelligent self-reliant method of applied science. A vast deal of historic religion has thus failed of direct utility, and we can only suppose that the justification of its existence lies in some indirect function.

But, if as mere sanction and basis of authority of living human superiority this religion is scarcely sufficient function, we may add other indirect functions; for instance, dependence for example and also as restraint.

First, then, it may be said that religion as dependence and obedience thereby emphasizes and encourages a habit which is most necessary to socialization. We must consider this function of religion as of value in the history of society, and yet we must regard it as of no high significance. And we must note that religion begins rather as interdependence and reciprocity, the god being as dependent on the worshipper as the worshipper on him. In this matter early religion but reflects early society. But religion gradually loses reciprocity and becomes absolute dependence of man on deity, and at the same time

society assumes forms of absolute dependence on supreme power of monarch and despot and hierarch. It must, then, be considered likely not merely that religion sets a model for social relations, but *vice versa* also. Indeed, so far as religion concerns itself with the extra-human it is probably derived in its forms and spirit from religion as direct function in sociality. And, at any rate, religion as setting an example of absolute dependence is harmful to high socialization. Religion as fostering mendicancy, poverty, and all forms of unreciprocal dependence has been a distinct drag on social progress, which demands a high interdependence. The dependent classes are the problem of modern society. A vigorous independence and individuality is most valuable in societies where freedom reigns, where initiative has the freest scope, and progress is least hindered by conservative religion. The evolution of society is from a bare competitive reciprocity up through absolutism to the higher reciprocity of rational free individualism. Modern society is dominated by the scientific spirit, which demands that man work out his own salvation by practical appreciation of scientific knowledge and method. Science encourages an intelligent dependence on the specialist, but recognizes infallibility nowhere, and it must regard religion as anti-social so far as it emphasizes dependence on extra-human beings, and thus defeats real social coöperation to secure the end. Thus the highest sociality is a very complex interdependent, self-reliant specialism, which seeks to control nature through knowledge by natural means. Thus the British government repressing the plague in India is a higher, more successful type of society than the Hindu attitude and method toward the plague. And so everywhere religious socialization tends to be supplanted by scientific, and so far as religion hinders, by example or precept, it must be accounted as loss, as pathological rather than really functional.

We have mentioned the direct function of religion and one indirect function, namely as setting example and giving emphasis to dependence, and thus helping the bond between inferior and superior in society. These forms of function have little if any notice from Mr. Marshall, but the sociologist must, I think, regard them as very important, although hardly solving the whole problem of the function of religion. However, the negative side of the indirect function we have mentioned is restraint. That is, it is plain that religion, by encouraging dependence, thereby restrains and inhibits independence. In the moment of individualistic action we receive an instinctive check and an impulse towards social and racial activity. Religion thus holds us to the narrow path of typical racial action.



In regarding religion as merely negative and indirect function in repressing the lower, inorganic, individualistic instincts, Mr. Marshall must highly offend religionists in general, who make religion the absolute and supreme end of life, and not a social means. Religion has always had to do battle with the State, which has ever sought to enslave her and make her a tool, and the restraint function theory lies along the same line. But we note that so soon as the feeling permeates religion itself that God and the God-consciousness are mere social instruments religion is decadent, becoming formal and losing real vitality. That is, when indirect function becomes direct, and religion is observed for its social values, it loses its real power. It is certain that religion becoming conscious that it is not attaining its supposed and natural end, but seeking to continue itself as a mere social function, soon loses this value. However, in my paper on the psychology of religion (*PSYCHOLOGICAL REVIEW*, May, 1898) I have discussed this point of the socialistic theory, and it is only necessary here to remark the paradox that religion must be kept in ignorance of itself if it is to be itself and exercise its due function.

That restraint, the negative indirect function, as impulse which keeps us from offending the social order, counts largely in explaining the persistence of religion will at once be granted; but it cannot be regarded as the sole social utility of religion, as we have just sought to show. But early religion is mainly positive in function, and it seems highly improbable that it originated as negative inhibitive instinct. Religion primarily is a direct mode of obtaining advantage from *high superiors*, and has thus been carried on by successive generations until it has become instinctive, as, for instance, in prayer. It is not unlikely that prayer originated in the field of battle, when a fallen wounded foe prayed for his life to his conqueror. Religion arises, like all other utilities, as activity hit upon in a critical moment, in this case by some inferior in relation to superior, and then continued and improved and ultimately embodied in the race as instinct. This is the assumption which brings religion into the line of evolution. A Gold Coast negro prays, "God give me to-day rice and yams, gold and agries; give me slaves, riches and health, and that I may be brisk and swift!" (Taylor, *Primitive Culture*, Vol. II., p. 367.) Where is restraint implied in such religion? And yet it is an extremely common form in all degrees of culture, especially the lower. We cannot see trace of restraint-function in a vast mass of religion which must be accounted for. It is mainly as conducing toward a human intermediary as religious specialist, the sorcerer, and so towards a religious organization of society,



that such prayer and such religion in general have social bearing and utility, and partly as fostering the habit of dependence; but the restraint function cannot be said to appear at this stage. The simple, self-seeking one seeks without restraint a gratification by religious means. In a vast deal of similar religion there is evidently no conflict of racial and individual, and hence no higher governing instinct, as religion, to enter in and restrain the powerful instinctive, selfish activity. When religion becomes not merely personal means, but personal indulgence, religion becomes unrestraint, as we see, for instance, in many phenomena of revivalism. (See, for instance, Sidis, *Psychology of Suggestion, passim.*) Religion has done much to retard society by its formal conservatism, and to break up society in the heat of powerful emotions; and all this must be taken into account in a full view of the subject. The reformer and radical believe they have the God-given message and methods, and thus society has often been disturbed and sometimes even the family set at nought, as in hermitage and celibacy. In Christianity itself the family is secondary, and the disciple must be ready to hate even father and mother. Religion of the highest type, as interfering with the natural evolution of worldly success and advantage by individuals and societies, and setting up an unworldly, mystic, spiritual kingdom, destroys natural evolution, and hence naturalistic science must consider it pathological or seek some indirect function, as does Mr. Marshall.

Mr. Marshall's definition of religion as an instinctive check to individualistic action, a 'stop,' 'do not,' coming as from a high divine source, is far too narrow for either psychology or sociology, which must study religion as a general relation of inferior to superior in manifold forms and functions, as direct, and sanction, as example, as restraint. Every hypothesis, such as Marshall's, however skillfully deduced from biological assumptions, must be tested without bias by definite and extended study of historic facts, an immense and very complex field. And as Mr. Marshall fails to do this the scientific mind is disappointed. If even the religious experience of some single individual were thoroughly analyzed, something would have been gained for scientific exactness, but, as it is, his work remains as at best an ingenious suggestive speculation.

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## PSYCHOLOGICAL LITERATURE.

*The Psychology of Peoples.* GUSTAVE LE BON. New York, The Macmillan Company. 1898. Pp. xx + 236.

As the title implies, the author believes that it is the psychology, chiefly the character, of peoples which determine their arts, institutions and history. The problems involved have been treated more fully in works on the civilizations of the East, and "this short volume may be regarded as a brief synthesis." "Each of the chapters composing it should be regarded as the conclusion arrived at by anterior investigations." The work is divided into five books dealing with the psychological characteristics of races, how these are displayed in the various elements of the culture of races, race-history determined by race-character, how psychological characteristics are modified, and the dissociation of race-character and decadence.

The central idea of the work is that races possess souls the acquisition of which marks the apogee of their greatness and the loss of which marks their decay. In this soul, sentiment, beliefs and interests are the moving and directing principles, and these constitute the basis of what the author calls character. Very meagre importance is assigned to the rôle of intelligence in the civilization of peoples, even beliefs being determined by suggestion and imitation so far as they affect the masses. Culture is merely a matter of memory: it can be acquired by inferior races, but does not affect character. Inferior races are distinguished from superior ones by differences of character solely; superior races are distinguished from each other by variations of both character and intelligence. Wide variations between individuals are the mark of developed races, but they do not count in determining the rank of a race. In estimating character, the masses alone are to be taken into account.

Ideas "do not exert an influence until, after a very slow evolution, they have been transformed into sentiments and have come, in consequence, to form part of character. They are then unaffected by argument and take a long time to disappear." "Religious ideas are among the most important of the guiding ideas of a civilization. The majority of historical events have been due indirectly to the variation of

religious beliefs. The history of humanity has always run parallel to that of its gods" (p. 235). "In religion, as in politics, success always goes to those who believe, never to those who are sceptical, and if at the present day it would seem as if the future belonged to the socialists, in spite of the dangerous absurdity of their dogmas, the reason is that they are now the only party possessing real convictions" (p. 178). "Faith is the only serious enemy which faith has to fear." "A people is only led by those who embody its dreams." The author gives an interesting account of the genesis of such faiths, a process in which reason plays but an insignificant, and suggestion an all-important, rôle. Propagation of faith is never by argument, and always by assertion, affirmation, impression. In time "the mere effect of imitation, acting as a contagion, a faculty with which men are generally endowed in as high a degree as are the big anthropoid apes," insures the spread of the idea; and then it is that it becomes a matter of sentiment and an element in character in the race. Then it is irresistible to argument. It is such factors of the common life of individuals which make the race 'a permanent being that is independent of time.'

The author holds that religious faith is the all-important moment in the life of humanity, but he considers the objects of religious faith to be 'illusions,' 'chimeras,' 'hallucinations' and 'the children of our dreams,' leaving the reader to infer that humanity is self-deluded. And this inference accords in general with the almost cynical and pessimistic tone which the author's thought at times assumes. He predicts that Europe will be swallowed up of socialism, and that America will be torn to pieces by an inter-race war between the incompatible elements that constitute her population. As to human nature in general, "of all the factors in the development of civilizations, illusions are, perhaps, the most powerful" (p. 207). The triumph of ideas "is insured when they are defended by the hallucinated and by enthusiasts. It is of slight importance whether they be true or not" (p. 206). "Doubtless it is man who created the gods, but after having created them he promptly became their slave" (p. 192). Is it cynicism, or is it simply a love for antithesis and epigram?

The work gives an interesting account of many of the facts of the race-consciousness and laws of its modifications. The style is interesting and strong. Many valuable suggestions are contained in the work. The author's personal philosophy of religion is, however, irrelevant to the theme, and rather weakens than strengthens the closing chapters of the work.



*Les origines de la psychologie contemporaine.* D. MERCIER.

Louvain, Institut Supérieure de Philosophie, 1897. Pp. xii + 486.

Fr. 5.

This book is mainly a critical review of modern idealism from the standpoint of the neo-scholastic. To him the development and outcome of post-Cartesian idealism amounts to a *reductio ad absurdum* of its fundamental assumption. This assumption is none other than the sharp distinction introduced by Descartes between the two substances soul and body, soul essentially thought, body essentially extension, the two being utterly irreducible and incomparable, and their union a sphinx's riddle. Occasionalism, ontologism, and parallelism were but so many artificial efforts to guess the riddle. More consequent is the negative attitude of idealism, beginning with Locke's denial of *clear* ideas of the substances matter and mind, and passing through Berkeley's utter rejection of matter, and Hume's utter rejection of mind, to Kant's demonstration that knowledge of substance was not only unattained, but from the nature of thought unattainable. Kant still believed in empirical certainty, but here the author finds him inconsequent. If the objects of thought are determined by thought, no certainty is possible. The system of Hegel did not escape the subjectivistic difficulty, and was, indeed, a construction of the imagination rather than of reason (239). The truly logical consequences of the Kantian principles are seen in the doctrines of some recent French writers—such as Remacle, who contends that agnostic idealism must be extended to cover ideas themselves; that is, that even inner experiences, as known, are not the experiences as they are in themselves—or such as Louis Weber, who concludes that the only truth is the truth of a judgment, that outside of the judgment there is no existence.

This position of utter negation, though the only logical outcome of idealism, is self-destructive (340). For a judgment is meaningless unless it refers to some existence independent of the judgment. And unless an object is known as it is in itself, it evidently cannot be known to be different from our idea of it. The Kantian difficulty is the Cartesian folly, that of first conceiving a mind apart from its objects ('pure' reason), and objects apart from any mind (things 'in themselves'), and then wondering how the two are to be brought together (344).

As idealism proceeds from Descartes' definition of the soul, so from his definition of the physical has grown the conception of a universal reign of mechanical law. If all other bodies are machines, why not the human body; and if the single human body, why not those larger

organisms, the species in its development (Darwin), and human society in its history (Comte)? The system of Comte is thus on its positive side a carrying-out of the Cartesian doctrine of body, while on its negative, anti-metaphysical side it is an attempt to escape the more brutal materialistic consequences of the same doctrine (77).

Thus our author traces back to Descartes the two negative characteristics of contemporary psychology; its idealistic dependence, in all matters of theory, on the data of consciousness alone, and its positivistic neglect of ontology and rational psychology. Its third great characteristic, derived from natural science, is its increasing use of experiment. The author brings together evidence of the neglect of metaphysics in the universities and of the vigor of experimental study.

Among contemporary psychologists, Mercier picks out three as attempting either to harmonize or to supplement the current Cartesian tendencies. *Spencer* attempts a synthesis of the various conflicting elements. But he supplies no true organic unity. Nor does his 'transfigured realism' amount to more than a hazy belief. As for his doctrine of universal evolution, it is a mere expansion by analogy of a biological hypothesis, and owes its prestige less to agreement with facts than to its hold on the imagination (144, 145). *Fouillée*, though idealist and positivist, tries to avoid some of the negative consequences of these doctrines by introducing the conception of 'idées-forces,' *i. e.*, of the idea as dynamic, and of conscious or sub-conscious life as the dynamic principle of all physical existence. But when he would furnish a ground for knowledge of substantial reality, he can do no better than to allow the dynamic idea to create or postulate the reality it wants ('fiat Deus'). *Wundt* would enrich idealism by substituting the conception of 'actuality' for the conception of substance, and voluntarism for intellectualism. The latter attempt he carries too far. His genesis of ideas from the action of 'pure will' is as much a creation 'ex nihilo' as the intellectualist's derivation of the will from mere ideas (214). Yet Wundt is not far from the kingdom. If he "could disencumber himself of his idealistic and positivistic prejudices, and of the false notion of substance that he borrowed from Kant, and follow freely the direction which his own researches force upon him, he would logically be led to accept the fundamental theories of Aristotle's psychology. He would no longer consider the characteristic mark of the psychical as residing in consciousness. He would accept \* \* \* the conception which regards the soul as 'the first entelechy of the living body.' And the soul, so considered, would appear in all truth as 'that empirical concept of which

everyone makes use who really and successfully cultivates empirical psychology and not barren speculation'” (216).

This is the doctrine of the soul which the neo-scholastic propounds in place of the Cartesian separation between body and soul. Man as we know him is not a pure consciousness, but a compound of mind and body. Results deduced from the conception of consciousness as isolated from the body are not applicable to the actual man. Psychology, so understood, is an arbitrary abstraction. What is wanted is an anthropology, based upon the fundamental thesis of the *substantial unity of man* (295). The soul of man is the soul of the entire man, and is not to be found wholly in consciousness. Its primordial function is not thought or feeling, but the 'informing and animating of the matter of the body.'

Between the soul and its acts we must, on metaphysical grounds, assume the existence of *faculties*, substantially distinct from the soul (304). Since a faculty is simply a means of arriving at an act, there are as many separate faculties as there are types of the soul's action. There are five groups of faculties, those of organic life, those of sensory knowledge, those of intellectual knowledge, those of will, and those of locomotion. Feeling and will, on the other hand, are not separate acts, but parts of the same act. Feeling is but the passive side which appears in every state of consciousness, but which has been overlooked because the mind, in its Cartesian isolation, was, almost of necessity, conceived as the source of its own ideas and as altogether active.

In conclusion, the author sketches the history of the neo-Thomistic movement, recognizes that the dogmatic method of the old scholastic philosophers must now-a-days be replaced by a critical method, and urges his colleagues to foster experimental psychology, for which their own fundamental conceptions provide the only logical basis.

The above summary of the author's principal line of argument passes by several interesting chapters, such as those which defend the conceptions of supra-sensible knowledge and of finality immanent in nature. To criticise the author's views would lead us too far afield. His style is clear and attractive. His argument is skillfully conducted, and is well worth some attention from those who have been brought up on an idealistic diet. The main defects of the book lie in the direction of inadequacy. His statements of opposing views are admirably fair and objective, so far as they go. But he does not do justice to the positive side of the work of Kant and his successors. Still less is his summary statement of certain scholastic doctrines adequate to carry



conviction. For a fuller statement of them the reader is presumably referred to other books by the same author.

ROBERT S. WOODWORTH.

COLUMBIA UNIVERSITY.

*La Personne Humaine.* L'ABBÉ C. PIAT. Paris, Alcan. 1897.  
Pp. 401.

The theme of this work is suggested by the contradiction between the representations of human personality formulated by the traditional spiritualistic philosophy and those current in modern scientific psychology. According to the former the human person is a unique, indivisible, self-identical and permanent entity, actively producing and supporting its states and possessed of intrinsic capacities of reflection, in virtue of which it is rational, responsible and free. According to the latter the conscious self is a resultant of the play of a manifold of elements, the coördination of which constitutes its unity, and this coördination is never absolutely complete, but is capable, under pathological conditions, of such profound disturbances that two or even more personal consciousnesses may successively or simultaneously arise in connection with the same bodily organism; the organism itself, and not an independent conscious entity, is then commonly regarded as the substantial bearer of the mental life, which latter is represented as everywhere conforming to general principles of evolution and subject to the inexorable necessity of nature. We have here a clear, forcible and eloquent apology for the spiritualistic tradition face to face with the newly-discovered or newly-emphasized facts of science and in conflict with dominant scientific hypotheses.

The argument falls into three main divisions, entitled respectively Perception, Reflection and Responsibility. The first maintains by the usual appeal to the unity of consciousness, recognitive memory, etc., the original unity and persistent identity of the self, and criticises the evidence to the contrary in the facts of double consciousness. The second maintains the unique originality of the power of the human mind to think of and through universals, and criticises the evidence for the derivation of this faculty on evolutionary principles from an organic process, from instinct or from the language of the lower animals. The third maintains the reality of freedom relatively to the moral ideal as a living and concrete perception, and, explaining the consciousness of responsibility as dependent on a number of elements independently variable, sets forth the causes and consequences of its enfeeblement with impressive reference to certain features in the dark obverse of modern civilization.

From a scientific point of view the most valuable parts of the book are probably the critical. The criticism of the phenomena of double consciousness follows the lines made familiar to us by Professor Ladd in demanding more accurate observation and description of the facts and in explaining the accredited phenomena partly as pure automatisms and partly as changes due to distraction of attention in the field, rather than in the subject of consciousness. The criticism of the evolution theory in the second part is also unquestionably acute, though in insisting so strongly on the 'fait décisif' it seems to overlook the vast heuristic importance of a conception which may fall far short of the verification desired. Still, as over against a certain tendency to elevate a scientific theory into a scientific dogma, it is not bad to be reminded once in a while, even at the risk of some exaggeration, of its actual shortcomings.

As to the positive constructions of the book, the questions involved are so many and so complex that it is difficult to say anything without saying much, and any extended discussion would be here out of place. A few words on one point only must suffice. The contradiction which the author notices at the outset is certainly one which occasions no little perplexity to the student and the clearing up of which is a task worthy of a philosopher. But when, fresh from the reading of Ribot and Binet or, say, from the penetrating chapters of Mr. Bradley, one goes for more light on the unity and identity of the human person to the pages of this book, one can hardly help feeling, with all admiration for the lucidity of the style and the intellectual and moral tone of the discussion, that the real difficulties have not been met after all. One admits, of course, the unity of the consciousness, in each particular act of attention, what James calls 'the unity of the passing state,' whether the state be one of perception, of recognitive memory or what not; but to find in this the evidence of the unity and persistent identity of the concrete self, seems trivial. For this unity of consciousness, even if we include in it the invisible unity of the subject 'I,' surely is not the self, the person, of which we and the writers whom M. Piat opposes are thinking. And when, taking up the concrete self in all the complexity of its changing content and the variety of its aspects, we ask after its nature and significance, then it is that our question becomes burdened with all the weight of the problems of biological and psychological science and of social and metaphysical interpretation. It is noteworthy that, while claiming for the ego an existence as a permanent unity, on the direct testimony of consciousness, M. Piat refers the question of its substantiality to theology and faith. So far as this



points to a higher criterion of judgment than that furnished by empirical psychology, it embodies a true instinct. For the ultimate meaning of personality is found, not in the facts of consciousness, but in ideals of the will. As Hegel said, the great thing is not to be a person, but to become one. But, if this is so, then the first thing to do is to discriminate as far as may be with the utmost clearness the different questions at issue and the different points of view from which they may be legitimately regarded. It is more than likely that when the presuppositions and relativity of the different standpoints are fully understood, the disputes between spiritualists and phenomenologists, metaphysicians and scientific psychologists, will largely disappear. Philosophical dogmatism now as of old renders discussion interminable.

H. N. GARDINER.

SMITH COLLEGE.

*Citizenship and Salvation, or Greek and Jew. A Study in the Philosophy of History.* ALFRED H. LLOYD. Boston, Little, Brown & Co. 1897. Pp. 142.

Stevenson, in one of his essays, remarks that the purpose of a preface is to give the author of a book the opportunity, after his labors are over, to appear before the public with his plans, and proudly proclaim the nature of his achievement. The reviewer of *Citizenship and Salvation* would be much surer of his ground if Dr. Lloyd had availed himself of this privilege of the author and not sent this interesting, but very obscure, little book out into the world without a prefatory word. The work is divided into three parts, entitled, respectively, 'The Death of Socrates,' 'The Death of Christ,' and 'Resurrection.' It is, as its secondary title indicates, a 'Study in the Philosophy of History,' and it is conceived in a thoroughly Hegelian spirit, although entirely independent and original in plan and execution. It is also called by the author a 'biological study of self-denial,' and might equally well be styled a metaphysical study of self-realization. It is not a 'supernatural' or 'unnatural self-hood,' however, that is realized in self-denial, not 'a self that originally was not.' Self-denial is 'the way to the expression of an already active life, of an already living ideal.' In other words, the self that gets 'fulfilled' in the historical process through self-denial was from the first 'secure' and 'active.' History, progress, means the record of successive self-denials, rendered necessary by the incompleteness of ideals and the consequent clashing of opposing tendencies, which ever result in self-fulfilment. Socrates, for example, the real Socrates, was 'vitaly present in the life of Greece from the



very beginning;’ he was ‘the inner motive of Greece that had in spite of all determined her destiny from the very beginning.’ He was alive in Greece long before Phænarete gave him birth on the slopes of Lycabettus, and he continued to live after he had drunk the poison, getting fulfilled even in his own death and in the death of his race in the triumph of Rome. The contradiction which led to the tragedy of Socrates’ death was the contradiction between the worldly life, the ‘miserly’ life, which takes means for end, and ‘the life apart from the world, which assumes that the end will realize itself.’ Either attitude alone would check fulfilment. Divorce of means and end meant their reunion in an historical movement, which Philip of Macedon and Alexander in fact inaugurated, and which Rome completed. Of this movement the philosopher was Aristotle, who taught that the soul is not an end by itself, but the end or purpose of the body, and that, similarly, the world is to be regarded as the incarnation of reason. When reason is regarded, however, as the world’s, it comes to be regarded as no longer man’s, and thus forgetfulness becomes ‘the successor of reason in man,’ and man himself is then considered as ‘but a means to the world’s end.’ The universal empire of Rome, with its militarism and mechanism, is the inevitable outcome. Thus Rome completes the work of Alexander and Aristotle.

The way is now prepared for Christ, who as the ‘World-Reason’ (the ‘Word Incarnate,’ the ‘revealed ideal’) is ‘the liberator of the world.’ This Christ, however, is not merely the historical Jesus, but, again, as in the case of Socrates, a motive always present, although in him become at last, in a special sense, active. This idea of a World-Reason revealed unto men, is the self of Christ that triumphs in Roman Christianity. When we turn to Judea, moreover, we observe a conflict similar to that which existed at Athens; there we find the same ‘co-operation of symmetrical opposites.’ Jewish idolatry of the past had come to be pure formalism, lacking all vital content; and Christ, as the Messiah, expressed in life ‘an as yet unrealized ideal.’ The respective attitudes of Christ and his people were, however, but contradictory and one-sided aspects of the single activity which constituted the national motive of the Jewish people. Hence the collision was inevitable, and in that collision—the crucifixion—the Jewish ideal is set free. And here we come upon a very ingenious theory of our author’s. The motive of the Jewish people found also its expression in money lending. Money, as a commodity, is the treasured past which the Jews idolized; lending, on the other hand, affirms the future as motive. ‘In money-lending the confusion of future with

past found expression, and a national life, so long isolated, so long deprived of participation in distinctly worldly affairs, was set free, the people turning their necessity into opportunity' (p. 81). And so the Jews became Christians in their own—'a very worldly way.' They with their talent for money-lending, and the Christians, with their spiritual other world, severally conquer Rome.

Let us apply these ideas. Rome had become, before the appearance of Christianity, a military government, a mechanism. Each part, each citizen, of such a government says in effect: 'I am not, because we are all one and equal.' Yet each feels, when the mechanism moves, as the soldier feels after the battle: 'I did it' (p. 90, 91). 'A sentient mechanism is a whole which upon action breaks into a group of microcosmic reproductions of itself,' that is, it becomes an organism (p. 91). Christianity, therefore, was able to interpret Rome unto herself, for Christianity is summed up in the word organism, which Dr. Lloyd 'likes to call the Christ-motive.' Now there are two 'chief incidents of all activity,' sanction and interpretation. (This is introduced with an 'of course.') Socrates 'sanctioned' the Roman empire, Christ 'interpreted' it. But the interpretation is, as always, fatal. Organism and mechanism cannot co-exist. The remaining history of Rome is simply the record of a process, in which the leaven of the idea of organism is spreading more and more. Philosophy, with her dispute over universals, over substances and monads and *a priori* forms, tells the same story. And Kant is the last great Roman philosopher.

In the third part of the book, conclusions are drawn—not without an apology. Democracy is seen to be the goal, and at the same time the fulfilment of monarchy. It is also the consummation of the 'Christ-motive' organism. In a democracy, each citizen is, in fact, a *parvus in suo genere rex*, each has 'imperial rights over his own complete self-expression.' At the same time, each becomes a mediator for, represents, all the rest, in his own individual way. In order that he may properly do this, however, he should have 'credit' precisely in proportion to his power to apply the world's forces, and the bank should be merely an institution for gauging this credit by accurately measuring each man's individual 'capacity for action.' When banking reaches this perfect stage, every capitalist will be a laborer, and every laborer a capitalist. The church will undergo a similar resurrection. It will cease hoarding the future, as the bank will cease hoarding the past, and turn its attention to the 'more vital expression' of the soul, here and now. Prayer is then simply 'the earnest, hon-



est, trusting definition of the sphere of one's activity; it is 'science becoming motive, or mind liberating soul.' And, on the other hand, 'ritual' is simply 'body expressing soul,' the action which prayer sets free.

To criticise Dr. Lloyd's work in any complete way would require a book larger than the volume before us. I shall confine myself therefore to a few of the more obvious reflections that suggest themselves. It is probably safe to surmise that no one will be convinced by the book. It is far too brief to prove its position, and far too long for a mere statement of it. There are numerous repetitions—a defect which the author himself recognizes. Now, very frequently these repetitions concern just those matters about which we should like to have more light, but the repetition does not give the added light. Startling assertions are frequently made as matters of course, a fact which intimates that Dr. Lloyd has as yet let the world only into a little corner of his thinking-shop. One cannot but feel that there is much that is fanciful in our author's reasoning, as, for instance, in his discussion of money-lending as 'left-handed' Christianity, and in the whole treatment of banking. So in the characterization of historical events and personages, one is sure that the facts are distorted, or, at least, but very partially presented, in order to fit the formula. The description of Socrates, for example, as a 'spendthrift,' taking end for means, and standing for 'abstract spirituality,' is Socrates twisted so as to form the proper antithesis to his contemporaries, regarded, also by a *tour de force*, as 'misers taking means for end.' Again, when we are told: 'in the nature of organisms, as he who runs may read, are the primal teachings of Christ' (p. 92), we are hardly satisfied to forego the evidence. To try to show that history had to be as it has been, is a dangerous and difficult, if not impossible, undertaking. Dr. Lloyd would seem not to have a proper appreciation of the magnitude of his task. He slips over and around obstacles smoothly enough, but for this very reason leaves the impression that much of his writing is mere word-play. It is often hard to see what he is thinking behind his phrases. Words are not used with that consistency which logical procedure demands. We find ourselves reading of Christ, the motive of his people, and anon, without warning, we are dealing with the historic Jesus of Nazareth. There is a similar shuffling in the case of Socrates. The term organism is used with great vagueness. The general objection to this whole way of thinking the universe under the form of an organism—the objection, namely, that the individual is lost in the process, that his significance, his freedom, is destroyed—is



lightly passed over with the remark, repeated several times, with slight variations, that when we say that history 'required' the appearance of a given man, at a given time, we mean also that his own true selfhood required the same thing: (p. 61) that 'internal sanction,' corresponding to 'external stimulus,' frees us from determinism. But this 'true selfhood' appears, after all, to be a sort of spirit of humanity behind the scenes, the 'inner motive' of the life of the people, and, thus considered, the doctrine becomes 'as vague as all unsweet.' One finds, however, many passages in the book which dimly suggest that Dr. Lloyd has a message of which he has not yet succeeded in delivering himself. *Citizenship and Salvation* is a program, and it is to be hoped that Dr. Lloyd will live to carry it out. Only we cannot refrain from adding the further wish that in the meantime he read more French and less German, so that the message may be more intelligible when it comes. What, for instance, could be more hopelessly obscure than pages 72, 73?

CHARLES M. BAKEWELL.

BRYN MAWR.

*A Study of a Child.* LOUISE E. HOGAN. New York, Harper & Bros. 1898. Pp. x + 220.

This is so distinctively a popular book that one hesitates to offer a review of it for publication in a psychological journal. But a justification for so doing is to be found in Chapter I., wherein the author tells us that "the few facts that were noted (during the first year) may be of greater interest possibly to psychologists than to the general reader." So she presents her observations to psychology. To quote from these records will serve to illustrate the nature of them, and the psychologist will perceive that he is not to expect to find trustworthy observations, critically considered. "When the child received his first bath he lifted his head unaided from the lap in which he was lying, thus showing to the popular mind an early inclination to know what was going on about him \* \* \* and to the psychologist great promise of brain powers" (p. 15). On page 16 is noted his objection to a Raff concerto for the violin and piano, and his toleration upon the same occasion of Handel's Largo. This observation, at least in its present form, is not available to psychology except as having the value of an impression; for it lacks the verification which it should have received from subsequent observations, or from an alternation of the concerto and the largo in order to determine whether the child's feelings changed with the change in the music. The main feature of the book is a series of

500 drawings by the child, executed by him during a period of some six years. These show an advance in the acquirement of manual dexterity, and an increasing appreciation of detail in the objects represented. The subjects chosen by the child for the exercise of his skill are also noteworthy, as indicating the direction of his interests. But we are not told (except in the case of the locomotive) whether drawings were often made for him, so that he followed or was helped by a copy; whether he drew from an object, from the memory of an object, or by all of these methods. Many of the drawings are obviously imaginative. These should have constituted a separate series. The want of system in the arrangement of the cuttings and drawings is to be regretted, and is a hindrance to their usefulness.

The language record also is fairly full; yet here only a time record is given, from which one learns that the child was able to say certain things by a certain date. It is a pity not to have formulated the records for correlation with other observations on child language.

The book is full of suggestions as to methods of inculcating desirable habits and various virtues, which will attract both kindergartners and parents, and it leaves on one's mind the pleasant impression of a happy, lovable child.

KATHLEEN CARTER MOORE.

PHILADELPHIA.

*Die praktische Anwendung der Sprachphysiologie beim ersten Leseunterricht.* H. GUTZMANN. Berlin, Reuther u. Reichard. 1898.

Every medical man is more or less interested in the physiology of speech in proportion as he is called upon to study the many defects which are met with among school-children, and the conviction is forced upon him that by good or bad methods of instruction latent tendencies to such defects may be either developed or eradicated. From such a practical acquaintance with the results of imperfect training arose Dr. Gutzmann's interest in the theoretical problems discussed in this work.

The monograph is divided into four parts: first, an historical review of the opinions of educational writers concerning the place of the physiology of speech in school instruction; second, the psychological justification for the study of speech physiology, and its practicability as a school method; third, the hygienic value of a physiological method in teaching to read; and fourth, the practical application of physiological principles in school instruction.

The earlier treatment of the problem was based on fanciful analogies between the form of the printed letter and the adjustment of the organs necessary in pronouncing it, as when Bonet the Spaniard remarks that the shape of the letter B, with its two semicircles meeting the perpendicular line, signifies the closing of the lips involved in uttering it, and that the letter A is formed like a trumpet  $\Delta$  to indicate that the letter must be pronounced with open mouth and constricted throat, which latter, however, as the cross-bar indicates, must not be wholly closed. Passing by these, we find that a long series of those who grasped the real significance of the speech-learning process have urged, or adopted, the physiological method of instruction. Graser, Fechner, Böhme, Krug, Grassmann—all these made earnest efforts to apply the principles of speech physiology in their instruction. Krug, the most explicit and insistent, demands that every child shall be made to construct each vocal element with a clear consciousness of the various adjustments of the organs involved; this exercise shall precede the actual instruction in reading. Krug's principles are intricate, his process, at least at first sight, artificial, and his method tiresome and wasteful. It is necessary to seek more simplified and practicable ways of applying these principles in school instruction.

The psychological justification of this method lies in the nature of the processes involved in learning to speak or to read. The combined process involves the activity of five brain-centers with their respective tracts: (1) The perceptive centre and auditory nerve-tract; (2) the motor centre and nervous tract connecting it with the mechanism of speech; (3) the visual perception-centre of the movements of speech and writing; (4) the kinæsthetic perception-centre which makes aware of the adjustments of the organs involved in speaking and writing, and (5) the motor centre by which the movements of the hand in writing are produced and directed.

The child learns by imitation; in speech this is chiefly through the ear, but not solely; the eye also participates. The child imitates movements of the lips when soundless; blind children come to speech later than the normal; of those who have lost their hearing some rapidly recover power to communicate by reading the lips. In German deaf-mute schools the sole method of teaching vocal language has been by the use of kinæsthetic and visual sensations in acquiring direct conscious control of the mechanism of speech. In blind deaf-mutes the sense of touch replaces vision in connection with kinæsthesia.

The objection may be urged that such a method is not practicable;



it is too confusing and burdensome, and can be applied only individually, not to classes. In reply, the success of deaf-mute instruction, and the relatively rapid progress of the pupils, demonstrate its possibility. By this method idiots have been taught to speak when all other means had failed. (v. Piper, Gutzmann, etc.) In normal fully-endowed children the progress should be correspondingly more rapid. As a matter of fact, it has been successfully used with young children and with stutterers to the number of nearly 400 by the elder Gutzmann. The objection is valid only against particular forms of the method which have been employed in the past, a disability which a perfectly possible simplification will remove.

The hygienic value of the physiological method of instruction in reading will be found in the correction of a long list of defects in speech and the prevention of a still greater number. There are 80,000 stammering school children in Germany. The proportion increases greatly with the age of the pupils, the number in the highest grades in some schools reaching three times that in the lowest. The statistics from half a dozen cities show that the most rapid increase takes place between the ages of seven and eight years, that is, immediately after the first instruction in reading. Of these a large part on their entrance upon school life were not developed stutterers, but showed only a tendency to such defect. It lies in the power of the teacher to correct this predisposition by training; else the habit, which is highly infectious, will be fixed through imitation. The still imperfect control of speech when the child begins school life, combined with frequent tendency to stuttering and lack of self-confidence, affords at once the condition for the establishment of all sorts of defects. The root of all such troubles lies in the imperfect control of the mechanism of speech, which has all along been practically met by directing attention to the processes involved and endeavoring consciously to perform the correct movements. Diesterweg and Gutzmann especially have urged the use of this means as a corrective, advising systematic practice in breathing, vocalization and articulation.

The practical application of these principles should not precede the teaching of reading, but should accompany and illustrate it from the first moment. The question of method presents three problems: (1) the means which the psychology of speech reveals for awakening the right physical images; (2) the way in which the individual organs can best be exercised; (3) the arrangement of the course of instruction in response to this demand.

The means are hearing, feeling, seeing. Clear apprehension of the

sound to be produced is necessary, since by it the correctness or incorrectness of the adjustment is chiefly to be judged. The child should be trained to observe, by direct feeling, how the mouth and throat are adjusted in speaking, for by means of these kinæsthetic images the movement is afterward produced. He should also know the form taken by the vocal organs in pronouncing the elements of speech, for by this means he is directly assisted in the production of the specific sounds desired.

The author does not propose the substitution of a radically new method in teaching, but only the introduction of a rational system of training in vocalization and articulation, in connection with the use of illustrated primers and photographs of the positions of the vocal organs in articulation. A plate of twelve such pictures accompanies the monograph.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

*Die Ideenassoziation des Kindes.* PROFESSOR ZIEHEN. Berlin, Reuther u. Reichard. 1898.

In his introduction Professor Ziehen reviews the experimental work done concerning the association of ideas in children, and gives full bibliographical references to researches upon the nature of association in general. The work reported on was confined to children from eight to fourteen years of age, and considered four things: the determination of the child's store of ideas; of the nature of associations resulting from a given initial idea; of the rapidity of the association process, and of the influence of special conditions, such as fatigue, upon the rapidity of association. The first inquiry was a preliminary one. Of the three principal questions of the investigation only that concerning the association process is taken up in the present monograph. The initial idea was suggested by an object seen, heard, or felt, or by a word. The words were arbitrarily chosen monosyllables, usually concrete ideas, with occasional terms of relation (*e. g.* 'similarity') of processes ('storm') and proper names.

Concerning the form of association the question of chief interest is whether the process of association in the child shows a greater or less tendency than in the adult to special fixed association groups.

Our earliest associations are, without exception, spatially and temporally determined individual ideas. From these are derived spatially and temporally indeterminate object ideas. In adults the word (except in the case of proper names) awakens throughout universal

ideas; to give individual worth it is necessary to add a definite article or pronoun. The author's distinction here is a logical one, but it is doubtful if such a discrimination between adult and child can be drawn psychologically. The image, or psychological representative, must be without exception a concrete image, definite or indefinite, derived from individual past experiences. Accompanying this psychological element, however, is the awareness that the image *means* a type and not an individual. But such knowledge of his meaning the child of eight already possesses. The truer distinction—and perhaps that intended—is that in the adult the concrete image has less localization; it hangs before the mind as an isolated thing, but not, therefore, as a universal.

Verbal associations were rare; only in one case were they frequent, where they formed twenty-four per cent. of the total. Among adults such forms are much richer and more frequent. Of verbal associations the most usual form was word-completing, *e. g.*, *Bett-federn*; *Post-Karte*, etc. Rhyme associations were rare, *Schlange*; *Zange*; *Fisch*; *Tisch*, etc., but individual cases were found with almost every child. One noticeable type smacks strongly of the school and its training, *e. g.*, *Bett* is written with 'tt'; *Macht* is written with a capital.

In the case of adults not only is the representative image of a concrete term a general one, but almost always the image which it calls up is likewise general. It is astonishingly different with the child. The author says: "I was prepared for a relative predominance of the individual association form. Of the degree of this prevalence I had not the slightest suspicion \* \* \* \* Most of the children connected with almost every stimulus word an individual idea, and with this again an individual idea, and in many cases both were spatially determined." The percentage of individual associations decreases with the age of the child; in the third class it is seventy-two per cent.; in the first, sixty-two per cent.; among adults the author found it to be on the average ten per cent., in regard to analytic and synthetic associations the elementary idea never aroused another elementary hetero-sensorial idea (*e. g.*, *sweet white*). This is natural; it awakens always the totalized object association (*e. g.*, *sweet-sugar*); this point suggests such phenomena as colored hearing, of which the author makes no mention, and the question whether they are of later development and not present in the imagery of children of eight to fourteen years of age. The elementary idea arouses a composite image four times as often as a simple; the type is *red-blood*, not *red-green*. This also is natural; the child



finds red combined in the concrete with elements of the other senses constantly, but seldom with green. The composite idea arouses most frequently (59 % of all cases) as its associate another composite which bears no relation of partiality to the first (*e. g.*, *window-door*); next to this, but far behind, comes its association with a greater composite of which it is a part (*e. g.*, *window-wall*); very seldom is this latter relation reversed (*e. g.*, *window-window-sash*).

The visual type predominates much more than among adults. Affective partitive ideas are very rare (*e. g.*, *gut—nicht gut; thut Weh*; etc.). In spite of the variety of content the form of association is always *contiguity* in the wider sense. No case of pure (indisputable) association through resemblance was observed. Associates far fetched in space and time are found much oftener than with adults. With adults familiarity is predominant; with the child congruity plays a much greater rôle; on the other hand, distinctness and constellation bear a much less important part. In closing the author recalls again the difficulty in tracing all the linkages of association upon which the very form depends, since the child himself forgets, and the consequent need for extended and patient investigation for the determination of these problems.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

*Psychologie collective et Psychologie individuelle.* RENÉ WORMS.

Lecture delivered before the Paris Academy of Moral and Political Sciences, November 12 and 19, 1898. Pp. 35.

These are by no means the first helpful words which psychologists have received from this eminent French sociologist, but they contain the most pointed bit of advice which he has addressed to them directly.

Starting with a reference to the rapid growth of sociology in recent years, he passes with a word the evident dependence of sociology upon psychology, and proposes to trace the influence of sociology upon psychology, in the formation of a collective psychology and in modifying the psychology of the individual, and then to sketch the outline of a new psychology which would recognize these changes. He is not here concerned with the metaphysical reality of the collective mind, but with the scientific inquiry into the causes of such common phenomena as the mind of a nation, a family or a crowd. He finds two sets of influences always present, a common environment and the reciprocal influence of the members of a group upon one another. The soil, climate, and productions of a country, for instance, arouse numer-

ous common sentiments in the minds of a people, while the social relations of family, friendship, religion, politics and education have an equally large share in the formation of the national mind. In the family the same influences are at work, together with a new element still stronger, that of heredity. Men engaged in the same industry come to have mental characteristics which are distinguished from those of the national type in the same twofold way, by the more limited and therefore more intense effect of the common environment—city, village, shop and fellow workmen, and by the particular stamp of that industry. The various groups thus mentally differentiated within the nation may be divided into four classes: those founded on blood-relationship—families and races; second, those of locality—cities, villages and provinces; third, those of industry—farmers, mechanics, merchants, etc.; fourth, those based on intellectual interests—all sorts of political, religious, literary and social organizations. A reference to various French writers on the state, family, city, provincial life and the life of workingmen shows that a collective psychology is already an established fact. Few of these writers are professional psychologists, and much remains to be done, but the way has been mapped out. Development will follow naturally.

In showing how sociology has influenced the study of individual psychology attention is confined to the higher mental faculties of reason and free will. Reason, as the faculty of general ideas, is of practical value in the social life, since every individual is making constant appeal to laws and general ideas, but it is also social in its origin. Man gets his first general ideas from the constant recurrence of phenomena which pass before his eyes, but he is much more influenced by the character and action of the men around him than by the cosmical or biological elements of his environment. Therefore, the first general ideas are social. The preservation and development of these ideas is in turn possible only through the medium of language, another social function. In short, human reason is penetrated with social elements.

Free will is impossible apart from reason. Therefore, free will, in its nature, shares the social elements of reason, while the field of its activity is the social world. Its highest aim is the moral elevation of humanity; its standards of right action are social standards; its rules of life those which can be adopted by all right wills. Thus the factors in the creation and development of human personality are almost wholly social. Heredity may furnish the first elements of our being, but education is equally important in the formation of our character



and minds, and in adult life it is in social relations with our fellows that our personality is developed—by imitation, opposition and adaptation. The larger part of individual mentality is a product of our collective existence.

But although the social mind has become an object of scientific study, and the individual mind is shown to be largely a product of the social life, there are objections to setting up a collective psychology as opposed to individual psychology. Collective psychology is, in the last analysis, psychology of individuals, while the mental life of any individual can be understood only in the light of the social life. All psychology is at the same time collective and individual. Secondly, our ordinary psychology, which is generally called individual, is anything but individual. It deals with general principles which are true for all men, and is even more comprehensive than collective psychology.

A better division of psychology would study separately the three sets of elements which we have found entering into each personality: those common to the whole race; those common to the group of which he is a member, and those which make up his individual personality. First, we would have a general psychology of the mental faculties common to all human beings; second, a special psychology, or what has been called collective psychology. The word 'special' brings out more clearly the essential nature of these researches, that of distinguishing between different groups of men which from the social point of view constitute different species; third, an individual psychology which would study the particular mental life of concrete individuals, the normal development and crises of intellect and heart, their natural tendencies, their influence on associates, and the net result of their mental existence.

Should this division be adopted, the younger science of sociology would render a distinct service to psychology, but at the same time would be doing itself a good turn. The results of psychology thus specialized would be far more valuable than the universal and abstract principles of the present psychology. The second of the proposed groups would be of especial value to the sociologist. In the mind of the nation he would find the general causes of its economical, moral, and political organization. In the mental characteristics of its families, cities, industries, and social groups he would often find the explanation of the details of this organization, and also of the strifes and internal difficulties which the national life has to surmount. At the same time the first group would give him the general mental characteristics of mankind, and thus help explain the striking similarities in the de-



velopment of different nations, while the third group would throw light on the question how the intellect or will of a single man sometimes transforms an industry, a whole region, or perhaps a nation.

Our first impression is that, so far as the making of books is concerned, the second of these fields belongs to the sociologist, while the third is the peculiar province of the man of letters. Our second thought is that the radical reforms suggested in psychology have already taken place. Present psychology is not confined to abstract or general principles. It has had no difficulty in absorbing sociological doctrines, if it has not succeeded in absorbing the sociologists themselves, and it has also picked up a few facts from physiology, biology, anthropology, history, and other sciences. Nine-tenths of all the large and increasing literature of psychology belongs to the second or third of these groups—practically all of the so-called new psychology with its experimental work, child study, educational investigations, animal psychology, and abnormal psychology, with its suggestive researches in hypnotism, insanity, and the subconscious realm. A good beginning has also been made, especially by French psychologists, in the study of individuals; for instance, noted writers and men of skill. In fact, it is impossible to carry on psychological investigation and add to the sum of the truths contained in the first group except through the study of the concrete individual. In this particular psychology is not different from geology, physics, chemistry, or any of the other sciences.

But it is feared that there are many teachers of psychology in America, as well as in France, who imagine that when it comes to the classroom psychology is radically different from the other sciences, and that here general truths may be made interesting and profitable quite apart from the concrete facts. If a fuller recognition of this threefold division shall avail to inspire teachers with the newer spirit of their science, and bring the student into closer contact with concrete mental facts, making them all sociologists and giving them all the literary insight into human nature, and awakening them to the practical possibilities of psychology in the professions and in the daily life of every individual, psychologists will, indeed, be grateful to the sociologist.

But it is by no means clear that anything would be gained by attempting to introduce this threefold division into the text-books used in the ordinary courses of psychology. Where only one course is given, at any rate, it is better to do justice to the three elements simultaneously. There is more crying need for improvement in methods of teaching than for wholesale changes in text-books.

CHARLES B. BLISS.

*O wahaniach w natezeniu minimalnych optycznych i akustycznych wrazen (zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke).* W. HEINRICH. Reprint from the Bulletin of the Academy of Sciences in Krakau. November, 1898. Pp. 18.

This is an abstract of a paper which reviewed the whole discussion of the fluctuation of minimal visual and auditory sensations and presented the results of the author's investigations, some of which at least were previously described in the *Zeitschrift für Psych. und Phys. der Sinnesorgane*, Vol. IX. and XI. According to Heinrich the fluctuations with visual stimuli are definitely proved to be due to the constant fluctuations taking place in the curvature of the lens, while there is every reason to believe that the fluctuations of auditory sensations are caused by the effect of breathing upon the tension of the ear drum. Experiments with a carefully trained observer who had lost both ear drums failed to reveal any fluctuation. Microscopical examination showed that the ear drum does move outward with every inspiration and inward with every expiration. A graphic record of the breathing and the auditory fluctuations showed that with deep breathing the number of fluctuations corresponds with the rate of breathing. With normal breathing, out of fifteen respiration periods ten corresponded to fluctuations in sensation. The author thinks that a more exact knowledge of the movements of the ear drum will furnish a complete explanation of the phenomenon.

One of the most striking announcements is that no fluctuation could be detected when the faint sound was a steady tone instead of a watch tick. These experiments were made upon only one person, and it is hard to believe that this will be found true for all persons under all conditions.

CHARLES B. BLISS.

LEONARD'S BRIDGE, CONN.

*Society's Need of Effective Ethical Instruction in Church and School, and the Suggestion of an Available Method.* E. M. FAIRCHILD. *The American Journal of Sociology*, January, 1899. Pp. 433-447.

The writer describes his method for the visual instruction of ethics in the public schools. By means of the camera and lantern slides, scenes illustrating the various practical ethical problems of child life, quarrels and fights, work and play, the sneak, the thief, the bully, the cry-baby, the general good-for-nothing, are shown to the children,

while the teacher, in a carefully prepared lecture, describes the proper adult feeling called forth by the successive pictures.

CHARLES B. BLISS.

*The Dawn of Reason or Mental Traits in the Lower Animals.*

JAMES WEIR, JR. New York, The Macmillan Company. Pp. 234.

The author begins with the following definition, "Mind is a resultant of nerve, in the beginning of life, neuro-plasmic action, through which and by which animal life in all its phases is consciously and unconsciously, directly and indirectly, maintained, sustained, governed and directed." He holds that conscious mind originated in sensual perception thousands of years after unconscious mind. The book treats of the following topics: Sense in the Lower Animals, Conscious Determination, Memory, Emotions, Æstheticism, Parental Affection, Reason, Auxiliary Senses, and Letisimulation, and the whole is followed by general conclusions, a bibliography and an index. No writings recognizing the Weismann theories seem to have been consulted, for few, if any, appear in his bibliography, and he writes as if the inheritance of acquired characteristics had never been questioned.

The author has spent many years in observing and experimenting with animals, and reports some very interesting and valuable facts, but his long association with animals and habits of reading his own ideas and feelings into their actions make him partisan and uncritical. He has discovered that when dogs appear to be baying the moon they are listening to the echo of their own barking, and says that the 'dog's voice is exceedingly pleasing to himself,' and that this indicates a 'high degree of æsthetic feeling in the dog,' when the more natural explanation would be that he supposes that he is answering another dog. He thinks that animals can count, and even holds that a blind dog who recognized the loss when one of her six puppies was taken away soon after birth must have had an abstract idea of the number six. The most surprising example of uncritical judgment, especially for a medical man, occurs when he says of the mason wasps that "they possess a mental faculty which far transcends any like act of human intelligence; they are able to tell which of the eggs will produce males and which females. Not only are they able to do this; but seemingly fully aware of the fact that it takes a longer time for the female larvæ to perfect than it does the male larvæ, they provide for this emergency by depositing in the cells containing female eggs a



larger amount of food." It seems never to have occurred to him that nutriment may determine sex. He cites the recognition of one ant by others of its nest as an example of *memory*, although Lubbock has proved that this takes place when the ant has not been hatched in the same nest, but has been hatched elsewhere. In general, he thinks of but one explanation of a fact, or else accepts the one most favorable to the intelligence of his animal friends; hence his *conclusions* in regard to the higher mental activities of animals will have little weight in the present critical study of animal intelligence, though some of his *facts* are valuable.

His most important contribution would seem to be the work he has done in studying the sense organs and discriminative power of the lower animals. If he can prove to the satisfaction of other psychologists what he claims to have demonstrated he will be entitled to a high place as an investigator in this field. In most cases he gives few details in regard to his experiments, so that it is impossible to judge as to the care with which they were performed.

He holds that all animals can tell the difference between light and dark, even without an eye or optic nerve, as is shown by such instances as the blind fish from Mammoth Cave always seeking the darkest place in the aquaria. He holds that such low animals as jellyfish will follow a light, and that their so-called 'marginal bodies' are eyes, instead of ears, as others have claimed. He claims to have discovered rudimentary eyes in the star fish, oysters and worms, and holds that a snail has a cornea, a lens and retina, and can detect a white moving object like a ball of cotton, with which he experimented, at a distance of two feet and a black one at from twelve to fourteen inches, and that a crayfish can descry a man at the distance of twenty or twenty-five feet.

As to hearing he says: "It is highly probable that the majority of lower animals, especially those which are sound producers, can hear just as we hear," and that others can hear "by *feeling* the sound waves." He claims to have demonstrated the organs of hearing in a number of insects, and that only in the Lepidoptera and certain Hemiptera are they in the antennæ, as has been claimed by many entomologists.

He claims that animals have at least two auxiliary senses, "tinctumutation, the color-changing sense, and the sense of direction, or, as it is erroneously termed, the 'homing instinct.' Neither of these faculties is instinctive, but they are, on the contrary, true senses, just as hearing or taste or smell," and he thinks he has demonstrated the gan-

gion centers concerned in these senses. If Dr. Weir desires his claims in regard to the senses to be accepted by scientists he should publish further details, for naturally no careful scientist will accept such important conclusions till details have been given and the results verified by the experiments and observations of others. The present book is popular rather than scientific, as, indeed, the author intended it should be.

E. A. KIRKPATRICK.

FITCHBURG, MASS.

*Vergleichende Untersuchungen der Sehschärfe des hell- und des dunkeladaptirten Augis.* S. BLOOM, und S. GARTEN. Pflüger's Archiv, LXXII., 372-408. 1898.

This paper contains errors of carelessness in the parts that are open to the eye of the reader, and hence it fails to inspire confidence that the thousand and one little details that require constant attention in the carrying out of any investigation have not been neglected. The reviewer does not, of course, usually take the time to look for such errors, but in this case he is much struck to find, from the diagram on p. 404, that the visual acuity of an observer is, under certain circumstances,  $\frac{4}{100}$  at a distance of  $9^\circ$  from the fovea, and has risen to  $\frac{8}{100}$  at a distance of  $8^\circ$  from the fovea, and also that upon another occasion, the change from  $\frac{4}{100}$  to  $\frac{8}{100}$  of visual acuity takes place between the distances  $12^\circ$  and  $10^\circ$  from the fovea. Upon referring to the table which the diagram illustrates, it appears that this is purely an error in the drawing, evidently caused by substituting at one point millimeters instead of the degrees into which they are being converted. Any one is liable to make a momentary mistake now and then, but it is difficult to understand how so palpable an absurdity in a drawing can have withstood the inspection of the two authors of the paper (and also, no doubt, of the head of the Physiological Institute of the University of Leipzig). One is not surprised after this to find that there are errors in the making up of simple averages. And in glancing at the other pages of diagrams, one notices that, on p. 389, when the visual acuity should be twice as good for the bright adapted eye as for the other, it is represented as being three times as good; this causes such a discrepancy in the course of the two curves as occurs nowhere else, and hence it is here also very singular that the authors did not look back at their tables to see if it was justified. On p. 398 we are asked to believe that an observer is able to distinguish two dots brought gradually in from the periphery at exactly the same distance whether the dots are five or

eight millimeters apart—that is, that his visual acuity at  $21^{\circ} 20'$  from the fovea is, in a certain measure, *both* 100 and 63, and that nevertheless a superiority of a degree or two in the distance at which definition takes place on the part of the darkness-adapted eye (that is, transferring to the above measure a superiority of 12) is sufficient to found theories upon! Again, we find from the table on page 388 that at  $6^{\circ}$  from the fovea and again at  $13^{\circ} 30'$  the visual acuity of the darkness-eye remains exactly the same up to the one-thousandth of the unit—here the ordinary unit—while the objective illumination is increased to eleven times, to fifty-seven times and to seventy-nine times that which was employed at first. (At  $3^{\circ}$  and at  $12^{\circ}$ , on the other hand, there is shown a gradual improvement.) This, indeed, would be an addition to our knowledge of very remarkable interest if it were a result to be depended upon.

A paper so riddled with evidences of utter ignorance on the part of its authors of the precautions to be taken in observations on the powers of the human eye in the periphery (and of the control to be exercised by common sense) has probably never before found its way into print. But in spite of these grounds for suspicion one finds oneself capable of a fresh feeling of surprise on seeing that the summing up of the results of the investigation is quite in disaccord with the body of the paper. We are told in the summary that “as appears from all our experiments on central and peripheral visual acuity, \* \* \* in spite of the objective illumination being extremely faint, and alike for both eyes (the bright and the dark-adapted), the dark eye, though it sees things much brighter, sees them much less sharply.” But as regards *central* vision, this was not the case at all for a very faint illumination, as the tables show, and as the authors themselves plainly state a page farther on. Thus it appears from the table on p. 388 that it was only when the lowest illumination tried had been increased 1,170 times that the bright-adapted eye saw better than the dark-adapted eye in the center; and the authors say on another page of this same summing up: “only a much more considerable increase of the illumination brought about for the center of the retina as well a superiority in the capacity of the bright-adapted eye.”

If we overlook these numerous marks of inadvertence on the part of the authors and treat their results, for the moment, as deserving of acceptance, they would appear to have made out that, starting with an illumination just invisible to the dark-adapted eye at the center: (1) the dark-adapted eye remains the superior up to eighty times that illumination as far as three degrees from the fovea, but either farther



out, or for brighter lights, the bright-adapted eye renders the better service; (2) this superiority of the bright-adapted eye becomes so small at a distance of  $40^\circ$  as to fall within the probable error of the observations, that is to say, to disappear entirely (a fact which is not drawn attention to by the authors); (3) nothing is gained for the dark-adapted eye by reducing the illumination for it until objects look no brighter than for the other eye. (It does not appear why a long investigation of this point was thought necessary, in view of the fact that it had already been shown that a diminution of intensity had no favorable effect upon vision for an eye in this state. A condition X having been shown to be superior to a condition Y, it would not seem, as a matter of logic, to be necessary to go through an investigation to show that it is also better than  $Y_1$ , when it is known that  $Y_1$  is never better and is nearly always worse than Y.)

The authors give no discussion of the theoretical bearing of their results, except to point out that, since there is no illumination at which the dark-adapted eye sees as well as the bright-adapted eye sees at its *optimum* illumination, the state of dark adaptation cannot be simply a state of non-fatigue. If our knowledge of the retina were still in the condition which it was in before we had gained any information about the growth of the visual purple or the descent of the pigment granules, this would be an interesting contribution. As it is, the result is simply what we had every reason to expect. The withdrawal of the pigment granules has for its evident effect the reinforcement of the faint light which enters the retina by reflection and refraction from one visual element to another; it would be very strange if the space-giving elements of the retina, whichever they may be, should not perform much better service when they are isolated in their beds of black than when they are subject to an influx of light on every side. Many of the recent writers on these subjects speak as if the night-adaptation of the retina were an affair of the visual purple only, and apparently forget the important change which takes place (and which cannot be without effect) in the position of the black pigment of the epithelium, a change which is entirely adapted to explaining the diminished visual acuity for a given subjective brightness of the night-adapted eye. This phenomenon does not apparently throw any light on the burning question whether the rods are or are not chiefly instrumental in the renewed vision that comes to us by night. It is only when the cones are known to be *hors de combat* by means of the night-blindness of the fovea, that we can be sure that we are dealing with rods only; the experiment made at this illumination goes

to show that what the rods gain in sense of brightness by the increase of the visual purple they more than lose in definition (*i. e.*, in space sense) by the loss of their separating pigment granules, but that (even when the increased sense for brightness is wholly counteracted by causing the night-adapted eye to look through gray glass) this superiority practically ceases at a distance of 40° from the fovea. It would be interesting to know if there is here also a diminution in the extent of retreat of the pigment granules.

There is every reason to expect that the cones as well as the rods should show subjectively some effect of night adaptation, for their change of size is a very marked phenomenon. This may easily account for the fact that there is some slight adaptation, if not at the center, still within the rodless region. We know now that the feeling of pressure is dependent upon a deformation of the skin and probably a change of concentration of fluids in which nerve-ends are immersed. Such a change of conditions might also easily follow upon the shrinking of the visual elements of the retina.

C. LADD FRANKLIN.

BALTIMORE.

*A Study of the Sense Epithets of Shelley and Keats.* MARY GRACE CALDWELL. Wellesley College Psychological Studies. Poet-Lore, Vol. X., No. 4, 1898. Pp. 573-579.

This study gives a careful tabulation of all the sense epithets used by the two writers. The first table compares the frequency of adjectives of the different senses—sight, sound, touch, taste and smell. The second shows the number of adjectives used figuratively compared with the number used literally. The third gives the number of adjectives of color, lustre and form, while a fourth compares the frequency of the nine colors most used. Sight stands first in frequency, sound second, while touch, taste and smell are less adapted to poetic use. Shelley writes less of the external and uses fewer sense epithets than Keats. A larger proportion of those that he does use are figurative.

CHARLES B. BLISS.

*Dendro-Psychoses.* J. O. QUANTZ. American Journal of Psychology, Vol. IX., No. 4, pp. 443-306.

Even a psychologist has to think, for a moment, what Tree-States-of-Mind may be. Dr. Quantz has in this article given in a valuable collation of facts an interesting view of the vegetable kingdom as it has affected the body and mind of man in all their manifestations—

emotions, customs, religion, medicine and poetry. In the first section he sums up the biological and anatomical evidence for the descent of man from some race of tree-climbers, and in the next—'Psychic Reverberations'—he outlines certain psychoses which, existing to-day, can be, he thinks, accounted for only by the supposition that we spent our lives in trees in some previous pre-simian existence. Such states of mind are, among others, fears of serpents, winds, thunder-storms, and the fear of falling. 'Tree Worship,' 'The Life Tree,' 'The World Tree,' 'The Paradise Tree,' are followed by the tree 'in Medicine,' 'in Child Life' and 'in Poetry,' the last being the least successful, as for adequate treatment it would require a volume by itself. The prominence of the tree in all these relations seems, however, hardly to be proved, even by the very wide range of folk-lore covered by the author; and the article at times falls very near being a mere catalogue of the uses of the word 'tree' and its synonyms, wherever they occur. It seems credible that vegetable life, being next in importance to animal life, should receive a secondary amount of human attention; but it seems, likewise, somewhat in the air to use these facts as an argument for the spiritual descent of the human soul from the sensations and reactions of tree-climbers. 'T were to consider too curiously to consider so. It is hard to tell upon what subject such an article might *not* be written, where analogy runs rampant and the result is an intoxicating series of similitudes, which, if regarded with any degree of credulousness, dazzle one with their bizarre aspect. From the fact that when two branches of a tree grow together again, or the twig of a bramble enters the ground again making a hole, they have a remedial power, why should we not better infer a belief in holes or circles than in wood? These examples seem just as likely to be instances of the importance of the circle in magic or of suggestion as a therapeutic agent. As raw material for poetry Dr. Quantz's article is most interesting.

WILFRID LAY.

COLUMBIA UNIVERSITY.

*Hydro-Psychoses.* FREDERICK E. BOLTON. Am. Jour. Psy., January, 1899. Vol. X., No. 2, pp. 169-227.

*Minor Studies from the Psychological Laboratory of Clark University,* XII., XIII. and XIV. Ibid., pp. 280-295.

In the first-named paper the author investigates the influence that water has exerted in shaping and moulding man's psychic organism. Evidences of man's pelagic ancestry are found in the fact that his embryo goes through all the stages of evolution. Thus he is, at one



time before his birth, practically indistinguishable from a fish. The brain and nervous system, the organs of circulation and respiration show structural rudimentary organs; and vestigial structures in man are cited to show the subaqueous existence of our infinitely distant ancestors. Of course, a great argument is the fact that there *are* amphibious animals, and that, when young, they are all aquatic; and another is in the 'animal retrogressions to aquatic life,' seen in the whale, seal, beaver, walrus and sea lion. 'Psychic reverberations' are felt by us even to-day in the hypnagogic phenomena of swimming, floating and jumping, and in the preference for suicide by drowning. In 'the primitive conceptions of life' water is seen to be important, and the theories of the Ionic philosophers are dragged in, in the section 'Water in Philosophical Speculation,' 'Sacred Waters,' with their oracular powers and superstitions; and 'Water Deities' are cited in great numbers, as are 'Rivers of Death' and 'Paradise' as a land beyond the sea. Water itself is animate in the superstitions of childhood and primitive culture. 'Lustrations and Ceremonial Purifications' by water, including 'Infant Baptism,' show the natural reverence one has for one's forebears. Even 'Water in Literature,' poetical and religious, is touched upon and found to be a great source of all kinds of emotion. 'The Feelings of People at Present toward Water' have been investigated by Mr. Bolton with a Clark University questionnaire which contained rubrics on 'Running Water,' 'Large Expanses,' 'Waves, Billows, etc.,' 'Children's Animistic Conceptions of Water' and the 'Earliest Feelings toward Water;' and the answers to these numerous questions are given to the extent of almost nine pages of fine print. The 'Pedagogic Significance' of all this is that children like to play in the water, and they ought to be allowed to do it; and that the human soul is benefited by communion with water. Here is the final sentence (p. 227): "The childhood of the race was spent in delightful contact with nature; the child, ontogenetically recapitulating the phylogenetic development of the race, craves instinctively for communion with nature."

The serious objection to papers of this kind is that the writer has not given proof of the applicability of his facts to his theory and to his theory alone. This would have been most desirable in an article consisting largely, if not solely, of a conglomeration of facts and legends, a mixture of science and folk-lore.

This remarkable paper and its fellow *Dendro-Psychoses* noticed immediately above, are beautiful examples of the way to make a syllabus seem interesting; but the possibilities of the continuance, *ad*

*infin.*, of such lines of thought should be appalling to the mathematically scientific mind. For why should we not have articles on Aer-Psychoses, Geo-Psychoses and Omichlo-Psychoses, as air, earth and fog (particularly the last) must have exercised a great, in proportion as it is distant in time of evolution, and, therefore, fundamental, influence upon our thought. Supporting the thesis in Geo-Psychoses which we expect to see emanating from Clark (I give a few hints for the forthcoming article) would be 'Dust thou art,' etc., and the fact that so many people prefer to be buried in the earth rather than burned up; and the fact that a great many persons will have themselves cremated and turned into their constituent gases is a good point for the article Aer-Psychoses. The present writer confesses to an extraordinary fondness for similitudes and analogies, and that he much enjoyed reading Mr. Bolton's article, for it awakened in him many desires to drop books and seek some well-known swimming hole; but it is a pertinent fact that, while reading *Hydro-Psychoses*, something (was it his subconscious self?) kept humming in his ears the tune out of the hymn-book: 'Pull for the Shore.'

Numbers XII., XIII. and XIV. of the Minor Studies are (a) 'On Nearly Simultaneous Clicks and Flashes,' (b) 'The Time Required for Recognition' and (c) 'Notes on Mental Standards of Length.' (a) The first research, conducted by Mr. G. M. Whipple, seems to show that the flashes, either by reason of their faintness and the focussing of the attention necessary to take them in, or for some other reason, tend to be perceived before the clicks. Might not, however, the variability of the results of Whipple and those of Exner, Gonnesiat and others be due to the visual or auditory type of the subject? Summed up, the results of these experiments (6 subjects) are to show 'a greater attention-claiming quality' of the flash, which makes the interval for recognition shorter for the flash-click than for the click-flash order, and that this holds true for series of pairs. (b) In the second research, by F. W. Colegrove, illustrations from magazines were shown to the subject, some of which he had not seen before, and he reacted, indicating whether he had or had not. The results (from 5 subjects) seem to show that the judgments vary in quickness with the expectation of known (quicker) or unknown (slower) pictures. (c) In the third, by Mr. Colegrove, the mental standards of length were studied by giving 10 subjects a series of fifty circles graduated from  $1\frac{1}{2}$  to  $4\frac{9}{16}$  inches in diameter and a series of lines the same lengths, and asking them to say how long they were. Three inches was the favorite estimation.

*The Dynamogenic Factors in Pacemaking and Competition.* NORMAN TRIPLETT. Am. Jour. Psych., Vol. IX., 1898, pp. 507.

In bicycle races the value of a pace may be from twenty to thirty seconds per mile. Mr. Triplett states the theories that have been proposed to account for this wholly or in part. The nature of each of these theories is indicated by their respective titles, namely, suction, shelter, encouragement, brain worry, hypnotic suggestion and automatism. He then advances another theory, stated as follows: "Bodily presence of another rider is a stimulus to the racer in arousing the competitive instinct; another can thus be the means of releasing or freeing nervous energy that he cannot himself release; and, further, the sight of movement, by suggesting a higher rate of speed, is also an inspiration to greater effort." This theory does not exclude the above-mentioned factors in bicycle pacing, but it is supported by laboratory experiments in which most of them were eliminated. The experiments consisted in a flag race. The flags were attached to cord belts that were run by turning a crank like that of an ordinary fishing reel. The races were made alternately with and without a pacemaker or rival, *i. e.*, alternately against time and time plus a rival. Of forty children experimented upon, twenty were stimulated positively; they made greater speed in the presence of the pacemaker. Ten were overstimulated; they lost by the presence of the pacemaker. Ten were stimulated but little. As with wheelmen, the value of a pace was different for different children, but somewhat constant for the same individual in successive trials. Variations for age and sex were small and fluctuating.

In support of the second clause of the theory, he cites an experiment from Féré, illustrating this author's theory that the energy of a movement is proportional to the idea of that movement. The third clause of the theory is based upon an experiment in which the speed of counting from one to twenty was increased by 'pacing.'

C. E. SEASHORE.

UNIVERSITY OF IOWA.

*L'Invention.* Par FR. PAULHAN. Revue Philosophique. March 1898.

This contribution to the psychology of invention is characterized by a detailed description of the phenomenology of inventive processes, on the basis of intellectual and emotional experiences of the inventors themselves. The two most obvious generalizations drawn from this wealth of material, including inventions in art, science and technique, are,



first, that all inventive processes are essentially alike in presenting certain well-defined phases, and secondly that these processes are essentially volitional in their type; the main problem which emerges is the question of the relation of the chance associations or suggestions to the dominating idea of the invention.

In all invention there is first of all a tendency of desire, unsatisfied, which imposes upon the mind certain more or less fixed ideas upon which it counts for satisfaction. This is followed by a crisis, akin to volition, in which the dominating idea remains confused and unlogical often until the last moment, and may follow as well as precede the details.

The conditions of this semi-volitional phenomenon are to be found naturally in the sensational and affective sides of consciousness; the former being either general nervous stimulation, as when thought is stimulated by music or walking, or secondly by stimulation through some special artistic sense, either by the same sense in which the invention is conceived or by a law of transposition of the senses, as Paulhan calls it; an invention in one artistic sense may be stimulated by the experiences of another, as when Massenet is stimulated to the composition of oriental airs by the sight of a turban or by the taste of Greek wine, or when Flaubert desires to write a story in *purple*! Color schemes have suggested music, and *vice versa*.

The lack of logical connection between the conditions and results of invention indicates that the connections between the ideas are often largely emotional; and to this color is lent by the foregoing facts, which lead the writer to compare a specialized artistic faculty, which may be thus variously stimulated to the eye nerves whose functioning may be brought about by other than the normal stimulus. As further proof of the volitional nature of invention, it is shown that unsatisfied passions and instincts are often effective causes, Chateaubriand and Rousseau furnishing neat examples.

This tendency to affiliate invention with the volitional rather than the logical side of consciousness—going so far indeed as to consider the difference only one of content—leads naturally in the direction of reducing invention to a continuation of instinctive life, and to the false view which Ribot holds out, that it is capable of a purely nervous explanation. But the author saves himself from a too mechanical point of view—which when pressed must resolve itself into James' chance tipping of the nerve cells—by refusing to call in the element of chance, and by substituting for the fascinating definition of M. Paul Sauriaü, that the element of 'hazard' in invention is 'the conflict of

external casualty with internal finality,' a more comprehensive notion of invention as the resultant of a conflict of different systems of internal finality. This conception M. Paulhan has, unfortunately, not developed further, and the reader will miss likewise a detailed treatment of the social *criteria* of the reception of an invention. A study of these *criteria* from M. Paulhan's volitional standpoint, such as Baldwin has developed from a different point of view, is necessary to the completion of his study. In conclusion we can only call attention to the interesting discussions of the relation of invention to imitation, and to the sources of the subjective sufficiency of an invention.

WILBUR MARSHALL URBAN.

*On the Alleged Sensory Functions of the Motor Cortex Cerebri.*

E. A. SCHAEFER. Journ. of Physiol., Vol. XXIII., No. 4, Nov., 1898. Pp. 310-314.

This important though brief article or report was read before the Congress of Physiologists at Cambridge, England, in August, 1898. It has especial interest at this time because of the prominence which discussions of the will as bodily action hold in current psychological discussions.

H. Munk had made the assertion, followed by many, that "after total extirpation of the arm- and leg-area [of the cortex of monkeys] tactile sensibility of the opposite extremities is permanently lost; a touch or light pressure is without any effect; neither tactile reflexes nor eye- nor head-movements are produced. The same is the case if the whole arm- or leg-region is removed in monkeys; tactile sensibility is permanently lost in the opposite arm or leg." It was to test the truth of this assertion that this set of thirty experiments was performed on the cortex of monkeys. They were made (1) upon the area connected with the movements of the face (already published); (2) upon the area connected with the movements of the leg; (3) upon the combined area connected with the movements of both arm and leg; and (4) upon the gyrus fornicatus.

These experiments lead Dr. Schaefer to assert boldly that the above cited statement of Munk is "entirely erroneous; that, in fact, complete voluntary motor paralysis of a part may be produced by a cortical lesion without perceptible loss of tactile sensibility" (a fact certainly in accord with frequent clinical experience). "It cannot, therefore, be the case," says the writer, "that the motor paralysis which is produced by a lesion of the Rolandic area is due to a sensory disturbance. And it also follows that tactile sensibility is not localized

in the same part of the cortex from which voluntary motor impulses directly emanate." This does not preclude the possibility that branchlets of the sensory sort of nerves enter the motor areas of the brain; indeed, the tingling often felt upon stimulation of the region suggests that they do so. It seems to the experimenter conclusive that repeatedly excision of the motor area produced no anæsthesia in the part which was thereby paralyzed. We shall await with much interest the outcome of this important and seemingly difficult discussion.

GEORGE V. N. DEARBORN.

COLUMBIA UNIVERSITY.

*Recent Views as to the Topical Basis of Mental Disorders.* DR. KIRCHHOFF. Trans. by A. W. McCORN. Am. Jour. Insanity, Vol. LV., No. 3, January, 1899. Pp. 481-495.

This is a brief review of the present status of the localization of cerebral functions, with especial psychiatric reference.

The region about the fissure of Rolando, formerly called motor, is now shown to be half-composed of sensory fibres from all parts of the body; hence it is called by Flechsig the somæsthetic area. Quite analogous to this is the visual center ('those cortical regions into which the fibres of the occipito-thalamic radiations of Gratiolet extend'—parts of the cortex adjacent to the calcarine fissure), while it further appears that the retina is directly represented, homologously, in the cortex, as to a less perfect extent is the neural portion of the ear. The auditory area is in the posterior part of the superior temporal gyri and in deeper transverse convolutions. The olfactory area is probably in the gyrus hippocampi, and taste is most likely represented in the uncus near the nucleus amygdalæ. Pain may very probably be represented in the external limb of the lenticular nucleus (the putamen) and in the nucleus caudatus. These centers seem to have trophic functions also, and this relation empirical lessening of pain by improved nutrition corroborates. The unity of the whole nervous system is to be recognized continually. Flechsig's notion of thought-centers in the form of association-centers scattered through the cortex between the sensory regions, but mainly three in number, of which two, the middle and posterior, are probably united into one, the seat of the intellect, Dr. Kirchhoff considers tentatively admissible.

GEORGE V. N. DEARBORN.



*Neural Dynamics.* W. J. HERDMAN. Journ. of the Am. Med. Assn., Vol. XXXI., No. 21, December 19, 1898. Pp. 1211-1214.

This is another theory of neural dynamics and quite in line with the most natural suppositions concerning this doubtful matter; the article was read at the meeting of the American Medical Association held in Denver in June, 1898.

Dr. Herdman likens neurons to charged electrical condensers. The nutritive processes and states of a neuron are indices of its readiness for action, it having a surface-tension and a corresponding electrical potential. The end-organs of sensory nerves serve as avenues of ingress for forms of motion, which latter causes rearrangement of the cell's molecules and a change in the static electrical condition. On the one hand, every change in a neuron acts as a stimulus on every neighboring neuron, but at the same time, by the principle on which electro-magnetic induction acts, each neuron restrains the action of the others, the balance thus being easily disturbable. "Thus conduction and transference of nerve force are, according to this theory, to be regarded as of the nature of electrostatic phenomena."

GEORGE V. N. DEARBORN.

*Ueber die Primitivfibrillen in den Ganglienzellen vom Menschen und anderen Wirbelthieren.* ALBRECHT BETHE. Morphologische Arbeiten, VIII. Band, 1 Heft, 1898. Pp. 95-115.

This article by Dr. Bethe, of the University of Strasburg, is one more of the reports of very important histological research into the nature of the ultimate neural unit which are making this basal discussion so animated and seemingly contradictory. He is one with Remak, Max Schultze, Nissl, and Apáthy, besides very many others, in considering the fibrilla, and by no means the cells or the neuron, as the anatomical and physiological unit of the nervous system. By an improvement on Apáthy's method he has been able to demonstrate the fibrillæ in the *vertebrates*, namely, in the frog, dog, rabbit, and in man.

After a brief historical review of the work in this direction to date, and considerations in regard to methods employed, he describes the ultimate fibrillæ in, first, the axis-cylinder, and, second, as it appears in the ganglion-cell. Two plates of drawings, thirteen in number, finely represent what he has seen even, without the elaborate and detailed description of the text. He seems to have examined with

his new methods nearly every sort of nerve-cell found in the species studied, as well as the various sorts of conductive structures.

Dr. Bethe considers it probable that the fibrillæ may conduct impressions both toward and away from the cell; that the protoplasmic cell-projections are not neural, but nutritive; that the connection between the cell, especially its nucleus, and the fibrillæ is very 'loose.'

"The result of this research," says its conductor, "I may give in the very words of Max Schultze, expressed more than twenty-six years ago, but not recognized until to-day: 'Hence such a ganglion-cell, out of which a centrifugal nerve-fiber arises, has meaning as the originating organ of this fiber only in the sense that the fibrillæ out of which the axis-cylinder is composed lead to it by way of the lateral branches of the ganglion-cell, while the fibrillæ, which may be seen extending through the substance of the ganglion-cell, do not arise from the cell, but in themselves only surround it after the manner of the branches of the axis-cylinder and continue on into other lateral branches.'"

GEORGE V. N. DEARBORN.

*Neuron Energy and its Psychomotor Manifestations.* IRA VAN GIBSON and BORIS SIDIS. Archives of Neurology and Psychopathology. Vol. I., No. 1, 1898. Pp. 5-24.

This is the first article in the *State Hospitals Bulletin* under its new title and in its greatly improved dress. The object of the article, announced as only preliminary, is thus stated by its authors: "We intend here to set forth, in a concrete, diagrammatic form a theory that attempts to correlate the various general manifestations of psychomotor life with more or less definite physiological processes depending on the expenditure or restitution of nerve energy."

Many useful and precise definitions occur, and the article is especially rich (about three pages) in seemingly elaborate algebraic formulæ expressing the various sorts of metabolism of the neuron-groups. Psychopathies indicates psychic disaggregation correlative to dissociation within constellations of neurons, the neuron itself remaining uninjured; while Neuropathies is defined as "a group of psychophysical manifestations running parallel to fluctuations of static energy and accompanied by organic changes in the neuron." In mental disease psychopathies may become neuropathies, and the latter may go on to cytoclasis through processes of catalysis and cytolysis.

"The cycles in dynamic energy correspond to the physiological manifestations of the nervous system in the activity and rest of the

individual in normal daily life. Concomitant with the expenditure of dynamic energy of the neurons, the individual passes through the active normal waking state, and hand in hand with the restitution of this expended dynamic energy, he passes through the sleeping state of normal daily life. When, however, in the expenditure of energy, the border line is crossed, dynamic energy is used up and static energy is drawn upon. The border line that separates the normal physiological from the abnormal or pathological psychomotor manifestations is stepped over. \* \* \* \* *Catalysis* corresponds to liberation of the upper levels of static energy, and is accompanied by *retraction of aggregates of neurons*, bringing about the phenomena of psychophysiological dissociation. Restitution of the energy expended in the catalytic process is accompanied by *expansion* or *synthesis* of the neurons, which are again able to transmit or receive impulses in the particular aggregate to which they belong. An arrest or halt after the expenditure of energy in these upper static levels, corresponds again to a state of retraction of the neuron or catalysis. \* \* \* \* Broadly speaking, psychopathies run parallel to the phenomena of retraction and expansion of aggregates of neurons, while neuropathies are concomitant with actual degeneration of the neuron, especially of its cytolymph. \* \* \* \* This one continuous process of liberation of neuron energy may cover the life of a single individual or may extend over the life-history of many generations.

“The continuous descending pathological process may spread out in time and space, may extend over a long duration of time and embrace a great number of individuals. The tide of neuron energy may ebb away gradually, leaving each succeeding generation on a lower stage and deeper level in the continuous process of neuron disaggregation and degeneration, thus giving rise to the different stages and manifestations of *congenital degeneracy*. Many of the so-called degeneracies and the congenital diseases of the nervous system arise, we believe, in this way.”

“We may conclude this brief preliminary communication,” say these two scientists of the Pathological Institute, “with a few laws relating to the metabolic processes of neuron activity: I. Catalysis stands in direct and synthesis in inverse ratio to the number of disaggregated neuron associations. II. All other conditions remaining the same, the instability of a cell aggregate is proportionate to the number and complexity of its associative functioning groups. III. The stability of a neuron aggregate is proportionate to the frequency and duration of its associative activity. IV. The instability of a neuron



aggregate is proportionate to the frequency and duration of the interruptions in its functioning activity. V. The mass of formed metaplastm granules stands in direct ratio to the intensity of cytolysis and in inverse ratio to the progress of cytothesis."

GEORGE V. DEARBORN.

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## NEW BOOKS.

*De la Méthode dans la Psychologie des Sentiments.* F. RAUH. Paris, Alcan. 1899. Pp. 305. Fr. 5.

*La Nouvelle Monadologie.* CH. RENOUIER and L. PRAT. Paris, Colin et C<sup>ie</sup>. 1899. Pp. 546. 12 fr.

*Wörterbuch der philosophischen Begriffe und Ausdrücke.* R. EISLER. In 8 parts. Parts I., II. Berlin, E. S. Mittler u. Sohn. 1899. Pp vi + 1-96 and 97-192. M. 2 each part.

*The Foundations of Zoölogy.* W. K. BROOKS. New York and London, Macmillan. 1899. Pp. viii + 339. \$2.50.

*The Development of English Thought.* SIMON N. PATTEN. New York and London, Macmillan. 1899. Pp. xxvii + 409. \$3.00.

*Philosophy of Theism.* A. C. FRASER. 2d ed. amended. Edinburgh and London. 1899. Pp. xviii + 338.

*Manual of Psychology.* G. F. STOUT. Vol. I. London, W. B. Clive. 1898. Pp. xii + 240.

*Essay on the Bases of the Mystic Knowledge.* E. RÉCÉJAC. Trans. by SARA C. UPTON. New York, Scribners. 1899. Pp. xi + 287. \$2.50.

*The Public School Mental Arithmetic.* J. A. McLENNAN. New York, Macmillan. 1899. Pp. x + 138. 25 cents.

*Anthropological Investigations on one Thousand White and Colored Children of Both Sexes.* ALES HRDLICKA. Illustrated. New York and Albany, Crawford Co. 1899 (?). Pp. 86.

*Spinoza, his Life and Philosophy.* F. POLLOCK. 2d Ed. London, Duckworth; New York, Macmillans. 1899. Pp. xxiv + 427. \$3.

All students of philosophy will welcome the new edition of Sir Frederick Pollock's standard work—so long out of print. This edition is somewhat reduced in size from the omission of the 'critical and bibliographical matter collected' in the former edition. The appendices to the earlier edition are also omitted 'except the English ver-

sion of Colerus.' Certain later bibliographical indications, on the other hand, are now included. J. M. B.

*Lectures on the Republic of Plato.* R. L. NETTLESHIP. Ed. by G. R. BENSON. London and New York, Macmillans. 1898. Pp. vi + 364. \$2.75.

## NOTES.

THE 'Teacher's Professional Library' is the title of a series of books announced by The Macmillan Company under the general editorship of Professor Nicholas Murray Butler, of Columbia University. The contributors to this series will be leading teachers and students of education in Europe as well as in the United States. A number of volumes have already been arranged for.

PROFESSOR EDWARD BRADFORD TITCHENER is preparing for publication early in the fall 'A Laboratory Manual of Experimental Psychology,' which will be published by The Macmillan Company. The work will be in two volumes and will detail an elementary course of laboratory work. The first volume will deal with qualitative analysis, the second with the exact measurement of mental processes. Each volume will be published in a student's and a teacher's edition, the former giving instructions as regards the conduct of experiments, control of introspection, etc., and the latter furnishing references, cognate questions and exercises, and standard results. (Publisher's note.)

WE notice in the *Journal of Mental Science*, which is much improved in form and appearance, the beginning of an 'Index Medico-Psychologicus,' prepared by Dr. J. Turner. The first part goes from A to I, for the year 1893-4. The arrangement is alphabetical simply.

DR. DODGE has been advanced to an associate professorship of philosophy at Wesleyan University.

MESSRS. MAYER AND MÜLLER, of Berlin, are publishing in three volumes the mathematical correspondence of Gottfried Wilhelm Leibnitz, under the editorship of C. J. Gerhardt.

DR. BENJAMIN RAND, of the department of philosophy, will publish in April a work entitled 'The Life, Letters and Philosophical Regimen of the Third Earl of Shaftesbury.'

EDWARD THORNDIKE, Ph.D. (Columbia), instructor in education in Western Reserve University, has been appointed instructor in genetic psychology in Teachers College, Columbia University.

# THE PSYCHOLOGICAL REVIEW.

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## STUDIES ON THE TELEGRAPHIC LANGUAGE. THE ACQUISITION OF A HIERARCHY OF HABITS.

BY PROFESSOR WILLIAM LOWE BRYAN,

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AND SUPERINTENDENT NOBLE HARTER,

*Warsaw, Indiana.*

### I.

#### THE PSYCHOLOGY OF AN OCCUPATION.

A field for research is offered in the psychology of occupations. The chief engagement of every one is the acquisition or exercise of one or another association of habits, such as constitutes skill in a game, trade, profession, language, science or the like. With a little license one may call all of these occupations. In mastering an occupation, doubtless the whole man is involved, body and mind, sensation and movement, thought, interest, imagination, will,—innumerable known and unknown aspects of our psycho-physical life.

It might be argued that such an affair is too complex for scientific treatment until we have done with more elementary things, the fusion of ideas, the psycho-physic law, the chemistry of the cell, or whatever may be still more elementary. In reply, it may be said that the history of science justifies the study of concrete facts, however simple or complex, whether or not the results can at once be correlated with other facts and theories. One studies microscopically, another macroscopically. One studies the chemistry of the cell, another tone sensations,



another comparative religion. A fact fixed at any point stands in its own right, throws light at once upon the less and upon the more complicated aspects of reality, and so does its share toward a future correlation of the sciences into science. The fashion of a time may run now to narrower, now to broader studies; but time justifies all work which meets its test, verifiability *ad libitum*.

Most psychological studies, doubtless with good reason, have dealt with abstractions. This is obviously true of the studies, earlier and later, on will, association, attention, etc.; for these 'faculties' are plainly not concrete phenomena of conscious life, but artificially isolated aspects of conscious life. It is no less true that in the later laboratory studies on the fatigue of a muscle, the reaction time in a silence cabinet, or the like, we are dealing with abstractions. The reacting man, muscle, or ganglion is, indeed, concrete; but when a given process in one of these is studied experimentally, the first and hardest task is just the isolation of that process from 'disturbing conditions'—that is, from the complex stream of life in which alone it normally occurs.

The best of these analytic studies, earlier and later, are invaluable to science and, in due course, to the conduct of affairs. Invaluable, but still far from sufficient, by themselves, either for science or for practical guidance. The scholar singles out of the complex processes before him, some general aspect (law) or some group of facts. He exploits one or the other precisely and systematically. Excellent! But too often the price of this precision and system is an absorption which makes him blinder than his neighbors to facts or laws that are in the processes concerned, but outside the range of his methods, and to the actual course of events in which all the facts and laws known and unknown are interfused.

This blindness to things before his nose, but out of the focus of his attention, is the disease-of-the-scholar. He assumes that the particular principle or fact which he has defined substantially determines the whole stream of life in which it belongs. He writes an essay on will, or studies the latent period of an excised muscle, and thereupon issues commands to the

public schools. Science is his debtor if he has developed any truth. Science has time to wait for the rest. But if he tries to put his learning to work, the realities which he has ignored will have their revenge.

However, it is easier to see the need of trustworthy concrete psychology than to supply the need. The actual concrete processes of life are, indeed, all about and within us, but in a bewildering tangle. Out of this tangle we are all forced to get some 'knowledge of human nature' so that we may live together. To our own insights in this direction we may add those of others, those of artists and other sagacious men, those sanctioned by the folk. In this way we build up a concrete psychology, each for himself, and by this we guide ourselves in dealing with one another. It is the dream of the scholar to supplant this lore of the folk by an array of knowledge equally concrete and practical, but immeasurably wider, more accurate, more systematic, and freer from personal bias. The dream is long in fulfilling. There are quick ways, but they lead to pseudo-science. Witness phrenology, physiognomy, graphology and the more precocious chapters in criminology. Such outcomes warn us that there is no profit in fleeing from studies which pay for their precision by being abstract, to studies which pay for their concreteness by being untrustworthy. Better any fragment of cerebral physiology which is true, though by itself unable to tell any one what to do, than a Science of Human Character which tells every one what to do, but is not true. It must be recognized that macroscopic studies are subject to the same tests as the microscopic. The essential test in both cases is verifiability *ad libitum*.

The best examples of psychological studies at once concrete and reliable are to be found in the literatures of comparative psychology, psychiatry, criminal and individual psychology. Here in the best cases we have pictures of the typical conduct of animals, children, melancholiacs, paranoiacs et cetera, which instruct us better than unscientific popular psychology can, what to expect and what to do in dealing with individuals of these sorts. To this group of studies the psychology of an occupation would belong.

It would be well worth while if we could discern in any one man the chief subjective effects of mastering an occupation. Learning the business has been his chief concern, his most thoroughly evolutionizing experience. It has been an affair not of weeks or months of forced laboratory practice, but of years, wherein the natural interests of life have constantly driven him toward levels of skill only to be reached under such stimulation. In the measure that he has mastered the occupation, it has mastered him. Body and soul, from head to foot, he has—or one may say he *is*—the array of habits which constitutes proficiency in that sort.

Can such a case be studied with profit to science? The probability that it can be is increased by the fact that an occupation leads many men toward the acquisition of the same set of habits. These men are scattered all along the way from apprenticeship to mastery. Many of them begin and quit after touching lightly and being lightly touched by the business. These dabblers and failures are highly instructive objects of study. Many others press on into some usable degree of proficiency. These men are colleagues not in name only, but psychologically and physiologically. They have similar knacks, or similar traditions of the trade, or similar habitudes of some kind necessary in their business. They know, as well as they know anything about themselves, what the main habitudes developed by their occupation are; and if the psychologist can find his way to the right questions, they can give a valuable introspective account of those habitudes. It may be possible in the case of some occupations to supplement such testimony by objective experimental tests. A few in each occupation become experts, and of these an occasional one becomes able to do easily and quickly what his lesser colleagues can scarcely believe possible. Such cases are, of course, hardest to understand, and may escape all definition. But it would surely be worth while to begin the study of the genius by following him along that part of his path which he shared with many others. We might in this way, at least, find the point where he disappeared. That would be something.

In a word, society has already made for us in each occupa-



tion a vast experiment in the development of habits. If we can make use of some of these ready-made experiments, if we can delineate the path or paths by which one travels toward mastery of an occupation, if we can discover and describe the characteristic stages of the progress, if we can do these things so that every detail of our work can be objectively verified by any competent scientist, and so that the outcome will be accepted as true by those who have mastered the occupation, this should prove not unprofitable work. It should supplement what analytic psychology can do for pedagogy and psychiatry; for it would portray the actual typical procedures of men in learning or in failing to learn. And it should supplement what analytic psychology can do toward developing the science of mind; for, it would exhibit not theoretical syntheses of alleged psychic elements, but the actual syntheses which the science of mind must accept and explain.

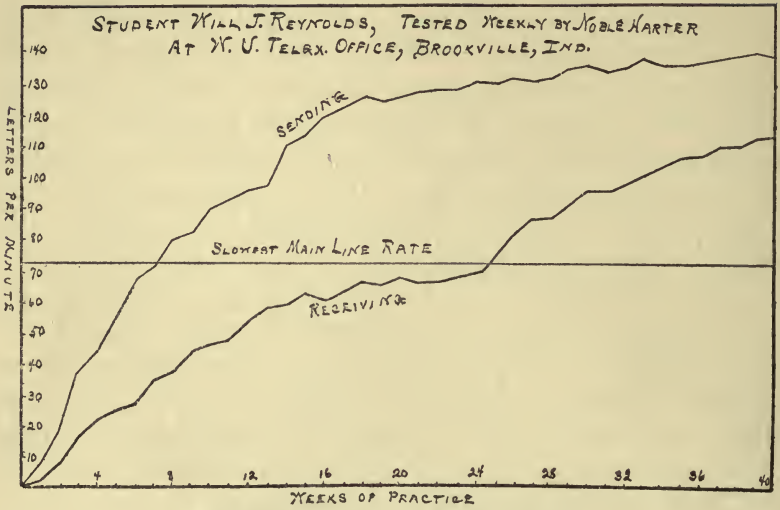
During the past five years the authors have made studies in the psychology of one occupation—telegraphy, utilizing throughout the work the experience of telegraphers as well as the methods of psychological research. The foregoing pages are not intended to overemphasize the importance of the results obtained, but to express a conviction which the study has developed, that in this direction lies a programme worthy the labor of many good men.

## II.

### DATA OLD AND NEW.

In a former series of studies on the physiology and psychology of the telegraphic language [PSYCH. REV., IV., p. 27] the authors gave the curves of improvement in sending and receiving. These curves were determined by the records of individuals tested each week, from the beginning of practice until fair proficiency was reached, and were confirmed by a consensus of opinion from about two hundred operators. As the conclusions of this paper are based in part upon those curves, one of the figures (X.) from the former paper is reproduced for convenience of reference.

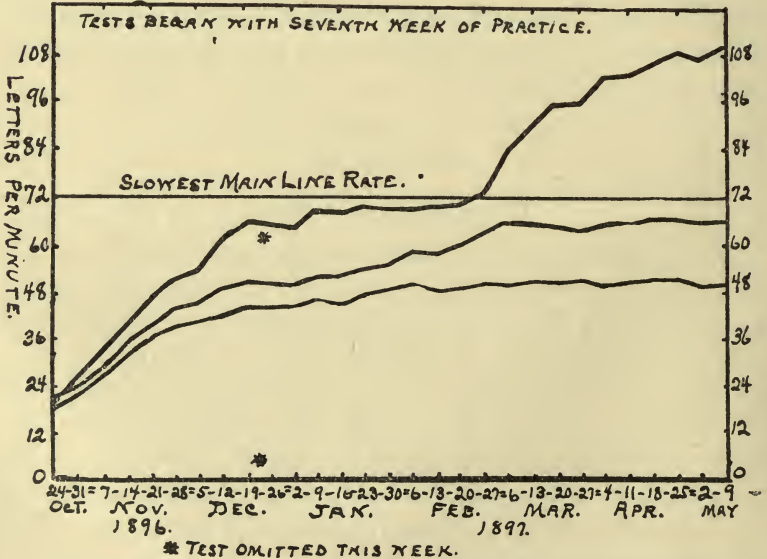
Fig. X



Reproduced from PSYCH. REV., IV., 44.

Fig. XI.

RECEIVING RATES OF STUDENT JOHN SMITH, BROOKVILLE, IND.



Connected discourse curve at the top; word curve in the middle; letter curve at the bottom.

The salient feature of the pictures shown in Figures II. to X., is the difference between the two curves. The sending curve has a form made familiar by many published practice curves. The receiving curve has for several months a similar form, but suddenly rises into what looks like a second practice curve. Moreover the history of expert telegraphers shows that after some years the receiving curve may ascend rapidly a third time.

Interest in the novel form of this curve deepens as evidence appears to show that it represents, in general, the course of improvement in various other acquisitions, *e. g.*, the learning of a foreign language, of chemistry, of English composition, etc. Interest is further challenged by the difficulty of explaining the form of the curve. In the former paper the authors proposed no explanation. None of our reviewers, nor of the psychologists with whom we have conversed, has given us a hint as to its meaning.

To investigate the problem further the following experiment was devised. A student should be tested each week on

- (*a*) rate of receiving letters not making words,
- (*b*) rate of receiving letters making words, the words not making sentences,
- (*c*) rate of receiving letters making words, the words making sentences.

These tests were made in the winter of 1896-1897. The subject was John Shaw, of Brookville, Indiana, who had begun the study of telegraphy about six weeks before the making of first test, Oct. 24, 1896. The method of making the test is described in *PSYCHOLOGICAL REVIEW*, IV., p. 48. The test was made each week until May 9. One test day, Dec. 26, was missed. The results are given in Figure XI.

Before discussing these results we subjoin evidence relating thereto derived from the introspections and observations of telegraphers. As hitherto noted (*loc. cit.*, p. 27), one of the authors (H.) was for years a telegrapher. To supplement his experience we have held long and satisfactory conversations with operators<sup>1</sup> of every grade up to the most expert men in the

<sup>1</sup> We cannot express too warmly our thanks to the members of the telegraphic profession for their cordial assistance without which the present study



country. We have asked telegraphers three principal questions:

*A.* To what is attention mainly directed at different stages of progress?

The answers agreed entirely, and were as follows: (*a*) At the outset one 'hustles for the letters.' (*b*) Later one is 'after words.' (*c*) The fair operator is not held so closely to words. He can take in several words at a mouthful, a phrase or even a short sentence. (*d*) The real expert has all the details of the language with such automatic perfection that he gives them practically no attention at all. He can give his attention freely to the sense of the message, or, if the message is sent accurately and distinctly, he can transcribe it upon the typewriter while his mind is running upon things wholly apart.

The feat of the expert receiver—for example of the receiver of press despatches—is more remarkable than is generally supposed. The receiver has two advantages over the sender. He can receive mentally far faster than any one can send; and with the typewriter he can transcribe much faster than any one can send. To bring the sender's rate up to that of the receiver abbreviated codes have been prepared. The receiver must translate the code into English words, and transcribe these correctly capitalized and punctuated, upon the typewriter. He takes, in this way, eighty or eighty-five words a minute. If mistakes are made by the sender, the receiver is expected to correct them as they come, and send a clean copy to press. The work continues for hours without leisure for re-reading, the pages being taken away to press as fast as they are finished. Yet, even during the performance of this astonishing feat, the operator is able at will to think about the significance of the despatches or to think of anything else he chooses. An Associated Press man, who has worked for years in one of our large cities, said to us: "I am in danger of allowing errors

could not have been successfully carried on. Especial thanks are due to Messrs. H. E. Jones, Assoc. Press, Cincinnati; Lot Lee, Assoc. Press, Indianapolis; Supt. Miller, Western Union, Cincinnati; E. B. Cassel, Chief Despatcher, Monon R. R., Bloomington, Indiana; and J. E. Sullivan, Chief Despatcher, Wabash Railroad, Peru, Indiana.

made by the sender to get into my copy, if I let my mind wander; but the truth is that in the last weeks, while taking press, my mind has been most of the time at home with a sick child."

*B.* How far can one 'copy behind' in different stages of his progress?

It should be explained that receiving is practically always 'copying behind.' That is, one does not, or should not, anticipate from part of a group of clicks what the rest will be; for if one guesses wrong, confusion of mind and error are likely to follow. Beginners are prone to guess ahead, and must acquire the habit of not doing so. Experts learn to wait. One expert said, "It is more natural to read back." He was asked if 'reading back' was like counting the strokes of a clock just after it is done striking. He replied, 'precisely.'<sup>1</sup>

The answers to the second question were also concurrent. (*a*) The beginner must take each letter as it comes, *i. e.*, he can copy behind one letter. (*b*) Later he can wait for words. (*c*) A fair operator can copy behind several words in connected discourse. (*d*) The expert prefers to keep six to ten or twelve words behind the instrument.

A count of the number of clicks (dots and dashes) in ten groups of ten words each, taken from a press despatch, gave the following result: 220, 275, 172, 214, 189, 267, 303, 260, 196, 281; average, 237.7. The achievement of the telegrapher in keeping correct hold of so long a series of sounds, and in doing this with a constantly changing series is, without doubt, one of the most remarkable feats of its kind. This is an example of a skill not to be reached by forced laboratory practice, but only by years of intense work.

*C.* What happens when you have to receive the disconnected words of a strange code or list of figures, such as bank clearings or the like?

The universal experience of operators upon this point was expressed by one expert thus: "When I get a word indicating

<sup>1</sup>If, however, the first words of a very familiar phrase occur, they may betray even the expert into anticipating the rest of the phrase. This fact is a significant illustration of the subjective solidarity of phrases. See below, p. 364.

that a list of figures is to follow, I sweat blood until I can catch up." He said he could wait for six figures if they were in groups of three separated by a comma, but if the figures were isolated, he would want to be not more than three or four behind. In a word, he could hold in mind forty to sixty or more of the elementary groups of the Morse code, if these 'made sense,' but only three or four, if wholly disconnected.

*Note on the Reading of the Blind.* To get cross light upon some of the foregoing points, information was sought concerning the reading of the blind, from Miss Nellie Love, an expert teacher in the Indiana Institute for the Blind at Indianapolis. She reports as follows:

"(1) Upon what is the attention of the pupil fixed as he reads?

Upon first reading a new selection:

(a) In a First Reader class of twelve every one kept his finger on the letters, spelling each word either out loud or to himself.

(b) In a Second Reader class of eighteen the attention of all but three was upon the words. These three read to see what the story was about.

(c) In the Fourth Reader class of seventeen the larger number gave attention to the words; the others to the thought.

(d) In the next grade, a class of about the same size, more regarded the thought, only three or four the words.

(e) In the highest grades the attention was upon the thought, except when the words were unfamiliar.

"(2) How far does the pupil read with his finger ahead of his voice?

(a) In First and Second Reader classes, not at all.

(b) In Third and Fourth Reader classes, most pupils keep finger and voice together. Two report the fingers one word ahead.

(c) In the highest reading classes the majority keep finger and voice together. Several read three or four words ahead. One pupil, a very bright boy, keeps a line ahead, eight or ten words. He reads the end of one line with the finger of his right hand and at the same time reads the beginning of the next line with his left hand.



(d) In the advanced classes, where reading is not a special subject, the best pupils keep finger and voice together. In each class that studies reading as one subject, pupils who study each day, read and study the lesson, and then are able to read smoothly, rapidly, and several words ahead of the voice."

In all grades, sentences are read faster than disconnected words, and disconnected words faster than disconnected letters. The rates are not reported. All these results are closely analogous to those found among the telegraphers. Of course there are no blind children who have attained a proficiency corresponding to that of the expert telegrapher.

### III.

#### CONCLUSIONS.

The immediate conclusions from the foregoing data will be given first; later (under IV.), an interpretation and discussion of these conclusions in connection with related literature.

##### 1. *A Hierarchy of Habits.*

One might perhaps suppose that receiving telegraphic messages is simply transliteration or, at most, transverbalization from the code into the mother tongue. The operators reject this view. The evidence before us proves that they are right in doing so. Neither the letter curve nor the word curve nor both together, account for the receiving curve<sup>1</sup> except for a short period (see Figure XI.). Most plainly, the letter and word curves fail to account for the receiving curve where it rises rapidly from the plateau, while they continue their slight ascent. From an early stage some curve or curves associated with the combination of words in connected discourse must coalesce with the letter and word curves to give as a resultant the receiving curve. At the period when the resultant curve is rising rapidly, while the letter and word curves are rising slowly, the higher constituent curve (or curves) must be rising rapidly.

What does this higher constituent curve represent in the

<sup>1</sup>The connected discourse curve in Figure XI. will be spoken of as the receiving curve; its constituent curves, as letter and word curves respectively.

learner? Certainly not merely nor mainly increased familiarity with the meaning, structure or logical connection of sentences in the mother tongue. When, for example, the learner has rapidly shot up from a rate of eighteen to a rate of twenty-five words per minute, no one can believe that he has made this gain because of a sudden and enormous gain in knowledge of the language he has used all his life. All the facts point to the conclusion that the telegrapher must acquire, besides letter, syllable, and word habits, an array of higher language habits, associated with the combination of words in connected discourse. Mastery of the telegraphic language involves mastery of the habits of all orders. In a word, *learning to receive the telegraphic language consists in acquiring a hierarchy of psycho-physical habits*. For a discussion of this conclusion in connection with related literature see below, under IV., p. 360.

## 2. *The Order of Learning the Habits of the Telegraphic Language.*

The synchronous curves of Table XI. and the experience of operators agree in showing that from an early period letter, word and higher habits make gains (a) *simultaneously*, but (b) *not equally*.

(a) The simultaneity in these gains is shown in Fig. XI. by the fact that from the point where the curves diverge, each continues to rise. This is perhaps to be explained by the fact that from an early stage the learner practises with sentences, taking them as slowly as necessary. In this way there is incidental practice of every language unit and of every language unit in its proper setting.

(b) The curves of Figure XI. show also, however, that for many months the chief gain is in the letter and word habits, that the rate of receiving sentences is, in this period, mainly determined by the rate of receiving letters and words, and that rapid gain in the higher language habits does not begin until letter and word habits are well fixed. This objective result is supported by the introspective evidence of operators. In the first days one is forced to attend to letters. In the first months one is forced to attend to words. If the learner es-

says a freedom for which he is unfit, suddenly a letter or word which is unfamiliar explodes in his ears and leaves him wrecked. He has no useful freedom for higher language units which he has not earned by making the lower ones automatic. The rank and file of operators are slaves to the machinery of the telegraphic language. They must copy close. They cannot attend much to the sense of the message as it comes, but must get its form, and re-read for the sense. Only when all the necessary habits, high and low, have become automatic, does one rise into the freedom and speed of the expert.

### 3. *The Plateaus.*

We are now prepared to offer an explanation for the salient peculiarity of the receiving curve,—its plateaus.

*A plateau in the curve means that the lower-order habits are approaching their maximum development, but are not yet sufficiently automatic to leave the attention free to attack the higher-order habits. The length of the plateau is a measure of the difficulty of making the lower-order habits sufficiently automatic.*

(a) *The first ascent.* No plateau appears between the learning of letters and of words, because very soon these are learned simultaneously. However, as the letters are few, one is each week able to give more complete attention to the mastery of syllables and words as wholes. This perhaps accounts, in part, for the rapid progress of the first weeks.

(b) *The first plateau.* For several months the learner is compelled to attend almost exclusively to words. The number of words which he has to learn in order to receive whatever messages come, is great. The average amount of practice which each word receives is therefore small, and the increase in the average rate of receiving correspondingly slow. This very slow increase of rate we have called a plateau. It continues until the learner has the necessary vocabulary so well learned that he can have his attention free for something else.

Another retarding influence during this period is doubtless the learner's slight hold upon the higher language habits. The importance of this retarding influence in comparison with that



of an imperfect vocabulary, can not be determined without additional investigation.

(c) *The second ascent* represents the acquisition of a new set of language habits. This is *a priori* probable from the consideration that in practice curves generally rapid progress appears when the developing function is in an early stage. We are not, however, left with a probability. While the receiving curve is rising rapidly the synchronous word and letter curves are continuing their ascent slowly. We, therefore, *know* that the learner is gaining speed by taking in some way increasing advantage of word combinations. Part of the reason why he improves so fast is, doubtless, that he has already been unconsciously habituated for certain phrases and forms of word combination in the period when he was attending mainly to words. *It may be that the rapid ascent of any practice curve represents mainly a quick realization of powers potentially present by reason of preceding gradual and unconscious habituation.* With the increased ability in taking sentences there comes, without doubt, increased ability to take isolated words and letters; *but, as one improves, the three curves diverge more and more. This means that skill depends more and more upon the acquisition of higher language habits.*

(d) Only the first few months of the period during which one is a practical operator, but not an expert, have been investigated experimentally. Our knowledge of this period rests mainly upon the testimony of operators. Men of this rank, of course, vary widely in skill and in rate of improvement. There is, however, one essential point in which operators who are not experts are more or less alike. They are all, in some degree, tied to the mechanism of the language. They cannot copy far behind. The mind must not wander far from the incoming stream of words, even to dwell upon the sense of the words. Few operators ever obtain complete freedom in the telegraphic language. These few must earn their freedom by many years of hard apprenticeship. Our evidence is that it requires ten years to make a thoroughly seasoned press despatcher.<sup>1</sup>

<sup>1</sup> We have shown above that receiving is not translating either letter by letter or word by word into the mother tongue, but involves the use of a great

(e) *The final ascent.* The testimony of experts is that the ascent from drudgery into freedom is as sudden as was the ascent from the first plateau.

*Note on the Sending Curve.*

Why does the sending curve have no such succession of plateau and ascent as appears in the receiving curve ?

There is no plateau in the sending curve in the earlier part of its course, because, as in the early part of the receiving curve, the various habits involved are acquired simultaneously (compare page 357), and there is no sharp ascent later, even when one becomes an expert, because such an ascent is mechanically impossible. At all stages one has in mind plenty of words ready to be sent as fast as the motor habits will permit. At first one is learning motor letter habits. Soon, however, also motor word habits. The sending curve rises accordingly in a fashion analogous to that of the receiving curve in its early stage. By and by, however, a mechanical limit is reached. Sending is, at the best, a slow business. A letter or digit requires from one to six strokes. Spaces of various length must be allowed for. One cannot utilize both hands and several fingers, as with a typewriter. So, at less than fifty words a minute, a maximum has been reached that cannot be surpassed.

4. *Effective Speed and Accuracy.*

(a) *Effective Speed.*

It has long been known that connected words can be read faster than disconnected, and letters combined in words faster than disconnected letters.<sup>1</sup> The facts upon this point, old and new, justify, we believe, the following conclusion: *Effective* array of higher language habits—that telegraphy is psychologically a distinct language, almost or quite as elaborate as the mother tongue. This view is supported by the fact that so long a time and such intense labor are required for the mastery of telegraphy—an amount of time and labor which would, without doubt, make the same men equally expert in any foreign language.

<sup>1</sup> We dissent, however, from the view that it is only or mainly the logical connection in sentences which accounts for the rapid rate in reading them. We believe (p. 366) that there are mechanical habits corresponding to often recurring peculiarities of sentences. This is shown by the fact that a series of words making no sense, if skillfully arranged in familiar sentence forms, can be read far faster than a series of words taken at random, and almost as fast as words making sense. Almost, but not quite. A consciousness of the sense appears to be still one factor in the affair.

speed depends, in a relatively small degree, upon the rate at which the processes dominant in consciousness occur; in a relatively great degree, upon how much is included in each of those processes. For further discussion see below, under IV., 4. p. 374.

(b) *Effective Speed and Accuracy.*

The gain in speed made possible by adding mastery of the higher language habits to mastery of the lower, does not lead to less, but to greater accuracy in detail. We have found invariably that many more mistakes are made in receiving disconnected letters than in receiving, at a much more rapid rate, letters that form words; and that, in turn, many more mistakes are made in receiving disconnected words than in receiving, at a still rapider rate, connected discourse. The practical experience of the telegraph companies proves the same. Although mastery of the higher order habits thus helps the receiver to accuracy in details, it cannot supply his ignorance of details. If a word not in his vocabulary comes as part of a dispatch, he is very likely to get it wrong. If he is often found making errors of this sort, it is proof that he needs a more extensive and accurate telegraphic vocabulary. Such a man is trying to receive faster than he can. He is trying to gain speed at the expense of accuracy. This is not *effective* speed, as his superiors will quickly let him discover. For further discussion see below, IV., 4. p. 374.

#### IV.

##### DISCUSSION.

In the foregoing, we have given little more than a bare statement of results. In the discussion of these results, we desire, first of all, to give the plain meaning of the facts known to us. We shall, however, use entire freedom in suggesting a wider circle of interpretations for which the evidence is not made out. We have, however, no interest in any theory suggested, except to see it tried by facts and assigned its proper measure of probability.

##### 1. *A Hierarchy of Habits.*

A man is organized in spots—or rather in some spots far more than in others. This is true structurally and functionally.



It is strikingly true of the various sense organs and their functions. No less of the various parts of the central nervous system and their functions. A man has some habits which are sporadic and isolated, some which are bunched together in loose groups (such as the outlay of skills which make one a carpenter), and then, some habits which are knit together into a hierarchy.

A hierarchy of habits may be described in this way: (1) There are a certain number of habits which are elementary constituents of all the other habits within the hierarchy. (2) There are habits of a higher order which, embracing the lower as elements, are themselves in turn elements of higher habits, and so on. (3) A habit of any order, when thoroughly acquired, has physiological and, if conscious, psychological unity. The habits of lower order which are its elements tend to lose themselves in it, and it tends to lose itself in habits of higher order when it appears as an element therein.

There is reason to believe that proficiency in chess, geometry, chemistry and the like, involves in each case the mastery of habits which are associated in some such hierarchical fashion. Leaving these slightly investigated fields, however, we turn to that of language. The proposition that a language exists subjectively as a hierarchy of habits, is supported by a considerable amount of evidence scattered through recent psychological literature. This proposition is by no means identical with the obvious truth that a language is, objectively considered, a system composed of various units—letters, words, sentences, etc. The existence of the objective system is evident to all who know the language; the existence of a corresponding system of subjective habits demands proof. Is there, for example, a psycho-physically unitary habit corresponding to a familiar word, or does the recognition of a word involve the separate recognition of each letter? The latter view has been held. It requires convincing evidence from experimental psychology and psychiatry to prove that the recognition of a word is ‘eine gesonderte Funktion.’ In like manner it will require evidence not yet fully forthcoming, to show what higher language units and what characteristics of spoken and written language (*e. g.*, cadence, sentence-length, etc.) are represented subjectively by distinct habits.

(a) *Letters*.—A letter (printed or telegraphic) presents to sense a manifold. Recognition of the letter and recognition of its elements are distinct functions. One may recognize the dash and the dot of the telegraphic code after a little practice, and may *know* that J = — . — ., without being able to recognize that group of clicks when heard. To recognize the group as a whole with maximum rapidity requires weeks of practice. On the other hand, one may recognize a letter as a whole—for example, in Old English type—but be wholly unable to reproduce in memory the essential parts of which it is composed.<sup>1</sup>

(b) *Syllables*.—Höpfner, in his study ‘Ueber die geistige Ermüdung von Schulkindern,’<sup>2</sup> finding that word errors are more frequent than syllable errors, and that letter errors are more frequent than errors as to parts of letters, remarks: “Silben sind im Wort und Buchstabe-teile im Buchstaben fester gefügt als Wörter im Satz und als Buchstaben im Wort. Wörter und Buchstaben sind also ‘sebständigere’ Elemente.”

This observation is doubtless correct. Syllables are, however, sufficiently ‘independent’ to make it worth while for primary teachers to use the child’s stock of known syllables in teaching new words. Mr. Harter is of the opinion that a learner of telegraphy pays little direct attention to the syllables as such, but is really helped in the hearing of new words by the presence of familiar syllables.

(c) *Words*.—A child or one suffering partial aphasia, may recognize the letters of a word, but not the word as a whole. See, for example, the case reported by R. Sommer,<sup>3</sup> who concludes: “Die Verbindung von Lautreihen zu Wörter ist eine gesonderte Funktion. Ein ‘Wort’ ist schon deshalb nicht als ‘Lautreihe’ zu betrachten.” On the other hand, children are frequently taught to recognize words as wholes before they know the letters of the alphabet. Decisive proof that the recognition of a word does not consist in the successive recognition of its letters, is afforded by Cattell’s result<sup>4</sup> that a familiar word can be re-

<sup>1</sup> See Goldscheider and Müller, *Zur Physiologie und Pathologie des Lesens. Zeitschrift f. klin. Med.*, Bd., XXIII., s. 131-167 (1893). Reviewed by Wallaschek in *Zeitschrift f. Phys. und Psych. d. Sinnesorgane*, VII., 228.

<sup>2</sup> *Zeitschrift f. P. und P. d. Sinnesorgane*, VI., 217.

<sup>3</sup> *Zeitschrift f. P. und P. d. Sinnesorgane*, V., 318.

<sup>4</sup> *Phil. Stud.*, II., 647; III., 470.

cognized in almost the same time that it takes to recognize one of its letters. This abundantly verified result one of the writers has found true of many children who are in their second school year.

Analogous facts appear on the motor side. One may be able to produce the separate sounds of a foreign language with considerable accuracy, as Karsten points out,<sup>1</sup> and still may not be able, without additional practice, to pronounce words. On the other hand, we pronounce the words of our own language with ease, but require special practice to produce the elementary sounds composing them. Karsten puts the matter thus :

(3) Nach dem oben gesagten wird man nicht einwenden wollen, dass, wer das bewegungsgefühl für das ganze hat, auch das für die einzelnen theile besitze und umgekehrt. Durch das erinnerungsbild ist eine bewegung von anfang bis ende abgegrenzt, dauer und art der mitwirkung aller in betracht kommenden organe fest bestimmt. Zwar können wir eine bewegung absichtlich an irgend einem puncte abbrechen, aber diese abgebrochene bewegung ist dann eben nicht mehr dieselbe, sondern eine andere, welche bei genügender wiederholung ihr eigenes erinnerungsbild entwickelt. Die bewegungen des arztes beim operieren, des malers, des musikers sind mechanisch und räumlich alle enthalten in den einem jeden von uns geläufigen bewegungen; doch gehört übung, das heisst ausbildung der bewegungsgefühle dazu, um gerade eine bestimmte bewegung genau auszuführen. Auch kann man eine bewegung, die man z. b. mit fünf fingern leicht macht, nicht sofort mit einem oder zwei fingern nachahmen; das wäre zwar ein theil der früheren, aber doch auch eine bewegung für sich, für die das bewegungsgefühl erst eigens entwickelt werden muss.—Kurz das bewegungsgefühl kann etwas einheitliches sein, auch wenn die wirkliche bewegung compliciert ist, und einheitliche bewegungsgefühle für grössere lautgruppen können in der seele sich bilden getrennt von denen für die einzelnen theile, aus welchen jene gruppen bestehen.

(d) *Word groups.* As certain letters often appearing in the same order give rise to a unitary word habit, so several words often appearing in the same order give rise to a phrase habit. Such word groups sometimes come to have a unity almost equal

<sup>1</sup>Sprecheinheiten ü d. Rolle in Lautwandel ü Lautgesetz; *Proceedings Mod. Lang. Assoc.*, Vol. III., 1887, p. 3.



to that of single words. As a rule, doubtless, the fusion is not so close; that is, we pass more easily than in the case of words from the consciousness of the whole to the consciousness of the parts. Nevertheless, the tendency of the first part of a familiar phrase to suggest the rest,<sup>1</sup> and the fact that everyone has not only a characteristic vocabulary, but a characteristic outlay of word groups, show that phrases exist subjectively as unitary habits. Furthermore, it has been shown that one who reads a language with a certain skill is liable to make phrase errors as distinct from letter or word errors.<sup>2</sup>

Paul<sup>3</sup> points out that we have many word groups (*e. g.*, *auf der Hand liegen*) in which a word has ceased to be associated with its ordinary meaning, in some cases (*e. g.*, *das Bad austragen*) so completely that it requires a knowledge of the history of language to explain the connection between the meaning of the phrase and that of the individual word. In such cases, the language unit dominant in consciousness is evidently the phrase and not the word.<sup>4</sup>

(*e*) *Habits Corresponding to Characteristics of Words, Phrases, etc.* The language habits so far noted are specific *i. e.*, in each case a specific stimulus (letters, syllable, word or group of words) leads to a specific reaction. It is, however, a fact of the highest importance that one's stock of specific habits contains the material for innumerable other specific habits (and also, some hold, for 'generic' or 'plastic' habits). When one has learned *bat, cat, many, model*, one has four specific habits; but one is within two steps (which may be taken in a breath or only after deliberate pains) of a new habit corresponding to *mat*. The first step is dissociation (in the manner described by Martineau and James<sup>5</sup>) of the *at* from the first two words, and of the *m* from the second two; the second step is the fusion of these dissociated habits, when they appear in the order *m-at*, into one new specific unitary habit correspond-

<sup>1</sup> See case mentioned above, p. 353.

<sup>2</sup> Cf. Berger: Ueber den Einfluss der Uebung auf geistige Vorgänge, *Phil. Stud.*, V., 175.

<sup>3</sup> Principien der Sprachgeschichte, 2 Aufl., 83.

<sup>4</sup> Cf. Cattell, *Mind*, XI., 64.

<sup>5</sup> James, *Psychol.*, I., 484.

ing to *mat*. (There is something arbitrary in the designation of *two* steps in the making of a new habit out of old ones. To ordinary introspection the process seems to have many steps when it occurs slowly and painfully, and only one step when it occurs in a flash, as when we recognize and adopt in an instant a new slang word—mugwump, popocrat. The words dissociation and fusion only designate and emphasize two essential phases of the whole process which ends in a new habit.)

In like manner, one's acquisition of these four words is partial preparation for *met*, *bet*, *cad*, and also for *bonnet*, *calico*, and for every word containing any syllable or letter learned. Further, the trochaic rhythm of *many* and *model* may become dissociated from these words, and may reappear as an aid in learning other trochaic words.<sup>1</sup>

In the same manner, any element or characteristic of a word group habit may become serviceable in the learning of new groups. Doubtless, the primary effect of using a given word group is to establish a quite specific habit. One can re-read a sentence more quickly than one can read a new sentence containing the same words in a different order. One can even re-read a sentence more quickly if one follows the rhythm first used. The dissociation of language elements from the specific wholes in which they have occurred, and their use in the construction or understanding of new sentences, are a task—perhaps the most remarkable task of which men are capable. The stupider or lazier one is, the less one has inclination or power for this task. But even the stupidest and laziest man meets, with some measure of success, the conversational emergencies that confront him. From his small language capital, there rise substantially the right nouns, verbs, phrases, but's, if's, not's, and even the right inflections to denote the attitude and temper of his mind; and these elements fall together with amazing swiftness into sentences never before used by him. One who has genius for expression differs from the dullard in having a larger language capital, greater facility in dissociating the elements and characteristics, and greater facility in making new combinations. Until we have had a great deal more research

<sup>1</sup> Müller und Schumann, *Zeitsch. f. Psych. u. Phys. d. Sinnesorgane*, VI., 28of.

in regard to the higher language habits, conclusions in respect to them must be proposed with reserve. At present the following points seem probable :

(a) It is well known that the average *length of sentence* is characteristic for a given author. In most cases, perhaps, the author is unconscious of his sentence-length habit.

(b) A *rhythm* often used probably becomes habitual, apart from any particular words, and is then an aid in reading and a factor in making new phrases, sentences, and paragraphs, having that rhythm.

(c) A certain *order* of the parts of speech (*e. g.*, 'he walked out of the way,' or 'out of the way walked he') often recurring becomes habitual, determines the making of new sentences, gives us a sense of ease in reading straightforward prose, and a sense of shock at sentences like Browning's 'Irks care the crop-full bird? Frets doubt the maw-crammed beast?'—even when, as in this case, the words are all familiar.

(d) A *grammatical construction* often used to express a certain feeling (of plurality, futurity, doubt or the like) comes to be automatically associated with that feeling, apart from any particular sentence, so that either instantly and effortlessly suggests the other, to serve as one of many elements in the reading or making of a new sentence.<sup>1</sup>

In like manner we may suppose that every peculiarity of style up to the structure and tone of a volume, corresponds to a more or less perfectly fixed habit. An E. P. Roeish novel betrays in its author a habit on its way to becoming as specific as sneezing.

*Note on the development of new habits out of old ones.* The old theory that doing particular things gives 'general training' of body and mind is nowadays confronted with the view that there is no such thing as 'general training'. The two views are perhaps not so irreconcilable as they appear to be in current psychological and educational discussions. The chief subjective effect of an act is doubtless its tendency to establish the habit of repeating that act; and, conversely, the best way to

<sup>1</sup> For the discussion of the point whether grammatical habits are specific or plastic, see below.



acquire skill in a particular act is to practise that, and not something else. But every bodily or mental process involved in an act is practised, and through dissociation and reassociation may appear in innumerable other actions. In the case mentioned above (p. 364), the 'fringes' of emotion and intention when the four words were learned tend to reappear upon repetition of these words; but may also, because of their exercise then, come up to reinforce the set of mind in a subsequent attack upon the multiplication table or the woodpile. When a boy drives the last nail in a fence as carefully as the first he is not thereby made ready to build a house, nor to codify the law of the commonwealth, nor to do anything else in the world so well as to drive nails into that fence; but his skill in nail driving will reappear when he undertakes carpentry; and the set of mind with which he drove them will reappear when he is a lawyer. We may deny that Grant's study of algebra gave him a general training of the mind that prepared him for the Wilderness, or for anything else so well as for that algebra, and nevertheless see that the mood of his hours with the algebra came up in his 'We'll fight it out on this line if it takes all summer.'

Professor Royce suggests<sup>1</sup> that besides specific habits one acquires generic or plastic habits, which lead not to a specific reaction upon a specific stimulus, but to a certain sort of reaction upon a certain sort of stimulus. He mentions especially the habits corresponding to the rules of syntax as in this sense generic. This view is attractive, and may be true. It may be, however, that there is no such thing as a plastic or generic habit, except in the sense that a habit may enter as an element into many different processes. Whether or not there are generic habits involved in the origination of higher mental processes, we believe that all habits tend to become in the same sense specific.

## 2. *The Order of Acquiring Habits which Constitute a Hierarchy.*

Every one knows that, in general, habituation in certain actions leaves us free for others. This principle is, however empty and useless in a given field until we know what habits

<sup>1</sup> PSYCHOLOGICAL REVIEW, V., 118; *Educational Review*, VI., 212.

are to be learned there, and which of these must be learned first, which second, etc. It is highly probable that in geometry, chemistry or whist one must acquire a hierarchy of habits; that some of these habits should be learned before others; and that some of them may with advantage be acquired simultaneously. Perhaps the most expert men have already felt their way to the right methods; but psychology and pedagogy would be greatly enriched by explicit and verifiable knowledge upon these points. Such knowledge the general principle stated above is impotent to give. It can only tell the student to do first things first. To discover what things *are* first in any particular field requires painstaking investigation, or a consensus of the practical experiences and intuitions of those who work in that field, or both. Though no one can foresee the results of such investigations in any particular case, there will be idlers in the psychological market place, when the results appear, ready to say: "Nothing new. We have known all along that some things must be done before others."

In point of fact, teachers of reading are not agreed as to the best order of studying the various language units. The older custom was to learn first the letters, then many syllables, then many words, and then at last to read sentences. In details this method varied widely; but its essential principle was to master lower units first and use these in picking out the higher. The newer custom is based upon an opposite principle. In the 'word-method' the pupil is taught a word as a whole before he knows any letter. In the 'sentence-method' the pupil is confronted with a short sentence before he knows any word or letter. In the later methods the subsidiary language units are to be learned incidentally, while the main attention is given to the higher language units and to the sense.

It is proved possible to learn to read by the older or the newer methods, and, indeed, by any method which brings the pupil for a long enough time into contact with print. The mind will find a method of its own. We believe, however, (1) *that by no device is it possible to gain freedom in using the higher language units until the lower have been so mastered that the attention is not diverted by them; and* (2) *that it is, neverthe-*

*less, wise at all stages to practise with the highest language units possible, and thus learn all the units in their proper setting.*

The alphabet-spelling-book method makes sure of the first requirement, but is grossly wasteful of time in postponing reading exercises which involve simultaneous practice of all the language units in their proper setting, and which are constantly more profitable because more interesting. The new synthetic methods gain these advantages, but lose a more necessary one, unless the teacher realizes that the pupils must all the while be getting the alphabet and vocabulary and making them automatic. If this end can be achieved incidentally, well and good. If not, it must be achieved by periods of practice devoted thereto. In no case can making the language elements automatic be skipped.

Similar principles hold in arithmetic. It is a mistake to demand of children a thorough memorizing of the number series and of the fundamental tables before giving them any exercise with concrete numbers and problems. It is a greater mistake to spend the years when the plastic memory is at its best in number exercises which are interesting, but which leave the children with the alphabets of arithmetic imperfectly mastered. The high-school boy who must halt in his mathematical work to remember the multiplication table, is enjoying the fruits of a pseudo-freedom in the grades. *There is no freedom except through automatism.* It is possible to avoid both the extremes mentioned. The work should be filled with concrete interest in ways fully displayed in our modern elementary text-books on arithmetic. But at all times the teacher should see to it that there is thorough incidental practice of those number-relations which should become automatic, and at some times there should be direct hard work at memorizing those relations.

In addition to the evidence already presented in favor of the foregoing view, two general considerations are submitted.

(1) It is quite useless to raise the question whether or not children should acquire specific automatic habits. There is no escape from such habits except by death. The Indian does not escape. The wolf does not escape. Neither Shakespeare nor Caliban escape. There is no question of escaping automatic habits. The only real question is: Which ones shall we acquire?



The school and civilization answer: While it is possible, acquire those habits which are the alphabets of learning and of cultivated life. This is the first necessary step toward the freedom, adaptability, ingenuity, and efficiency which give superiority to man.

(2) A school method must be judged by the moods and tempers which it cultivates, not simply by what is learned, still less by the momentary interest it arouses. If one forces mastery of the multiplication table by methods which keep one-half the school cowed and the other half rebellious, one has obtained a useful result at disastrous cost. Better not know the multiplication table than be thus morally maimed.

If, on the other hand, one anxiously converts all school work into a round of entertainments, if one shields the pupils from having at any time a sense of resolute effort with hard tasks, if one keeps the pupils vibrating between excitement and ennui as at a circus or picnic, what of the moods and tempers thus cultivated? To what set of character do they lead? For what occupation do they prepare? Every one knows. These are the moods and tempers of the loafer, the tramp, the sport—the idlers, rich and poor, who afflict society with their inefficiency and their consequent misery.

There is happily no need to choose between the galleys and the circus as models for the school and home. There are many schools and homes where hard tasks are performed in a good temper; where thorough drill does not arrest, but prepares the way for higher development; where children begin to do what they must later do to succeed in any business—pass cheerfully from interest in desired ends to a resolute drudgery necessary for the attainment of those ends.

If this view of education is correct, the course of study has no more important function than to make clear the essential habits involved in the mastery of each school subject, and the order in which these are to be acquired; and the teacher has no more important duty than to arouse in children such an interest in some higher aspect of the subject, that they will willingly lend themselves to mastery of its details.

3. *Plateaus.*

Wide variation and sudden changes in rate of progress are not peculiar to the learning of telegraphy. In general, it is indeed *a priori* highly improbable that the rate of change in any process will be constant. For such constancy requires an extremely improbable constancy in the many factors which unite in determining the rate. As these factors increase in number and complexity, the less likely they are to effect a constant rate. Modern evolutionary science has emphasized the facts which indicate that changes in nature are regular and gradual. *Natura saltum non facit*. It is, however, now well-known that nature does make leaps. It may even be that saltatory change is the rule. The recapitulation theory invites us to picture the history of each individual as a series of steps corresponding to the stages in animal and racial evolution. No one has made out an accurate time table for all these steps (or even ascertained exactly what the steps are). But no one would claim that the rate of progress through them is uniform. The development of the body and the mind both show 'resting periods' alternating with periods of rapid change. We 'perch and fly.' We live for months or years upon a certain level of interests, efforts and achievements, and then suddenly undergo a more or less radical conversion. All things are become new. The old life sinks into the vast subsoil upon whose surface, for a season, bloom new forms of the life of attention.

The well-known examples of rapid change are, of course, not cited as specifically analogous to the plateaus and ascents of the telegraphic curve, but only to show that such alternations of camping out and moving ahead are not exceptional or abnormal. For specific analogies we must look to the history of analogous acquisitions. In this promising field for research nearly everything remains to be done. Preliminary inquiry has developed the following provisional results.

(a) *Languages.* As hitherto noted,<sup>1</sup> in learning to read (first year primary), and in learning a foreign language, one's progress is analogous to that of the student of telegraphy. In

<sup>1</sup> Loc. cit., 52.

the latter case, especially, there is the same rapid improvement at first, the same dispiriting level just below the ability to understand ordinary conversation, the same rapid ascent into usable knowledge of the language, and the same year long struggle, seldom completed, before one has freedom in the language.

(b) *English Composition.* In the Indiana University, we have each year several hundred students in conditioned English Composition. All entering students are tested as to their ability to write printable English. Those who cannot do so, are required to take the conditioned English until they can meet the test. A student may pass out of this work at any time. The heaviness of the work, the discredit of having to take it, and the special fee required, make the motives for getting through very strong. The instructors in this work tell us that the progress of most students is pictured in a general way by the receiving curve. A few students pass out of the work very soon. This generally indicates that they failed to do themselves justice in the first test. In most cases, there is rapid progress nearly up to the passing level, and then a long plateau above which the student seems incapable of rising. In some cases, where students were expected by the instructor to pass in a few weeks, they have kept drudging away for the rest of the year with slight improvement. Doubtless, in these cases, the interference of established language habits is an important factor in retarding progress.

(c) *Chemistry.* Several teachers of chemistry have reported that the progress of students during the first year's work in that subject is similar to that of the telegraphic student. There is the same period of rapid improvement in the first months, followed by a long period of slow progress. In the Indiana University chemical laboratory the latter period has long been recognized and named 'the period of depression.' At one time it was supposed by the instructors that this period of depression might be due to an inferiority in the latter part of the laboratory manual, but further experience has shown that this is not the case. An explanation of the chemist's plateau analogous to that given for the telegrapher's plateau would be: that on the plateau the learner is constantly hampered because



he cannot, on demand, remember any one of a large number of elementary facts which he has once learned; that the large number of elementary facts which he needs to know, makes his progress toward sufficient mastery of them very slow; that a rapid progress comes at last when he can turn his attention from mastering the elements to a freer use of these facts in attacking more complex chemical problems. The chemists whom we have consulted incline to regard this explanation as correct.

(d) *Miscellaneous.* A large number of individuals have reported analogous experiences in learning mathematics, music, whist, chess, checkers, et cetera. In all these fields we find one or more long discouraging levels, where practice seems to bring no improvement, ending, at last, in the case of those who persevere, in a sudden ascent. It is probable that in each case one must acquire habits of lower and higher order, and that the explanation for the telegraphic plateaus is the explanation for the plateaus in these fields. Of course, the curves in these widely differing fields must have different specific characters. Each must be investigated for itself. In a time when some fear a dearth of significant problems for psychological research the prospect of such a field is inspiring.

In general, we have here a point of view from which we may discern a difference between the master and the man of 'all-round' development, who is master of nothing. Both have, from the informal experiences of life, some knowledges and skills which fit them to undertake the mastery of a given field. Both have developed these potential instruments of mastery, have 'gone over' the principal items of knowledge and 'gone through' with the principal forms of skill required. The master has not stopped here. He has initiated himself body and soul in the elements, so that after a time such things are to him like letters and words to an educated man. They shoot together easily into new combinations. They are units of meditation, of invention. Meanwhile, to the man who has only 'a good general knowledge of the field,' the feats of the master are impossible and almost incredible. The master's units of thought are each to him a problem. He must give time and pains to each one separately. He cannot think with them. He

is necessarily a follower, or, if he essays the freedom without the power of the master, he is worse than a follower—a crank.

#### 4. *Effective Speed and Accuracy.*

There is scarcely any difference between one man and another of greater practical importance than that of effective speed. In war, business, scientific work, manual labor and what not, we have at the one extreme the man who defeats all ordinary calculations by the vast quantity of work he gets done, and at the other extreme the man who no less defeats ordinary calculation by the little all his business achieves. The former is always arriving with an unexpected victory; the latter, with an unanswerable excuse for failure.

It has seemed to many psychologists strongly probable that the swift man should be distinguishable from the slow by reaction time tests. For (*a*), granting that the performances demanded in practical affairs are far more complicated than those required in the laboratory tests, it seems likely that one who is tuned for a rapid rate in the latter will be tuned for a rapid rate in the former, when he has mastered them. Moreover (*b*), a rapid rate in elementary processes is favorable to their fusion into higher unitary processes, each including several of the lower. Finally (*c*), a rapid rate in elementary processes is favorable to prompt voluntary combinations in presence of new emergencies.

In face of these *a priori* probabilities, eleven years' experience in this laboratory (the first three being spent mainly on reaction times) has brought the conviction that no reaction time test will surely show whether a given individual has or has not effective speed in his work. Very slow rates, especially in complicated reactions, are strongly indicative of a mind slow and ineffective at all things. But experience proves that rapid rates by no means show that the subject has effective speed in the ordinary, let alone extraordinary, tasks of life. How is this to be explained?

The following answer is proposed: The rate at which one makes practical headway depends partly upon the rate of the mental and nervous processes involved; but far more upon how

much is included in each process. If A, B and C add the same columns of figures, one using readily the method of the lightning adder, another the ordinary addition table, while the third makes each addition by counting on his fingers, the three are presently out of sight of one another, whatever the rates at which the processes involved are performed. The lightning adder may proceed more leisurely than either of the others. He steps a league while they are bustling over furlongs or inches.

Now, the ability to take league steps in receiving telegraphic messages, in reading, in addition, in mathematical reasoning and in many other fields, plainly depends upon the acquisition of league-stepping habits. No possible proficiency and rapidity in elementary processes will serve. The learner must come to do with one stroke of attention what now requires half a dozen, and presently, in one still more inclusive stroke, what now requires thirty-six. He must systematize the work to be done and must acquire a system of automatic habits corresponding to the system of tasks. When he has done this he is master of the situation in his field. He can, if he chooses, deal accurately with minute details. He can swiftly overlook great areas with an accurate sense of what the details involved amount to—indeed, with far greater justice to details than is possible for one who knows nothing else. Finally, his whole array of habits is swiftly obedient to serve in the solution of new problems. Automatism is not genius, but it is the hands and feet of genius.



COMMUNICATIONS FROM THE PSYCHOLOGICAL  
LABORATORY OF HARVARD UNIVERSITY.

AUTOMATIC REACTIONS.

BY DR. LEON M. SOLOMONS,  
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The experiments upon the time of automatic reactions, of which I wish to give a brief account here, are an outgrowth in part of the work on Motor Automatism published by Miss Stein and myself in the REVIEW for September, 1896. I had three main objects—to see whether the various stages of automatism which we there distinguished had characteristic reaction times; to get evidence, if possible, for the theory advanced in that article, that the feeling of personal agency accompanying a movement is due primarily to the motor neurons of the cortex—that is, that it is the absence of their activity which gives a movement its feeling of impersonality; and third, to attack the problem of the relation of attention to the different types of reaction by studying reactions in which attention was totally absent.

The experiments are not complete, and their evidence is not as clear and convincing as it might, I believe, be made. But since it is doubtful whether I shall be able to continue them in the near future, and especially since some of the indications may prove valuable suggestions to other workers in the field, I think it advisable to give at least a preliminary account now.

GENERAL METHOD. The mode of distraction adopted was the same as in the experiments on motor automatism—the reading of light, entertaining literature. The stimulus was the sound of an electric hammer. During part of the experiments the Scripture reaction key was used. During the last part this was changed, since some of the subjects found difficulty in

maintaining the contact between reactions without interfering with the complete automatism of the movement. I accordingly changed to an Ewald key, but used a contact through mercury instead of the simple metallic contact. With this key a considerable unconscious pressure might be exerted by the subject upon the key without breaking the connection, and yet the reaction require no special effort. The mercury contact had only a very slight immersion—never more than  $\frac{1}{32}$  of an inch—and did not, I believe, appreciably affect the reaction time, while it was of considerable assistance in maintaining connections during the intervals between the reactions.

The chronoscope—placed in a separate room to prevent the subject knowing when an observation was to be made—was connected in the usual way, the stimulus closing the circuit, and the breaking of the contact by the reaction opening it. Finding it difficult to maintain an adjustment of the fall hammer constant over long periods of time, recourse was had to a pendulum control. This had the disadvantage that the time of the control was greater than that of the reactions studied. But as relative values only were desired, this was no real difficulty, while the greater certainty of constancy of conditions from month to month was a distinct gain.

The subject was instructed to keep his attention as closely as possible upon what he was reading, and not to think of the experiment. He was asked to introspect as carefully as circumstances permitted, but not so as to interfere with the automatism. The subjects differed considerably in the ease with which they acquired the ability to react automatically, but the stages seemed to be the same in all.

At first the attention is all on the experiment, the subject reading without understanding. Gradually the incidence of attention shifts, and he is able to keep his mind on his reading between reactions, but has to stop reading to react. The interference produced by this reaction becomes less and less, until the various stages of automatism are reached and passed through. Some subjects become automatic after very little practice; others require a good deal, and their results are more valuable for the light they throw on the passage from voluntary

to automatic reactions, than for the passage from simple automatic to subconsciousness.

At first the reaction times were studied by the usual method of taking the average, corrected if necessary by throwing out those with very large residuals. But during this process it was observed that the small residuals were not, as they should be, in the majority; but that often, on the contrary, there were a large number of large residuals of about the same value, with few, if any, small ones. This showed that the average was simply a mean between two reaction times of different value, and, therefore, thoroughly misleading. Accordingly I adopted the method of plotting the reactions, as one plots an error curve. The resulting curve is, of course, of the same form as would be obtained by plotting the residuals, the position of the Y axis alone being changed.

The curves so obtained did not in general assume the form of the theoretical error curve, but showed a grouping of the reactions about several points. It had been my intention to study the effect of frequency, intensity of stimulus, etc., on the reaction times, and I had arranged my apparatus with that end. But finding the problem complicated by the reactions being of mixed types, I thought it best to confine myself to my main problem.

Owing to the uncertainty of the last figure of a reaction time obtained in thousandths of a second, I plotted the curves, during the course of the experiments, for hundredths of a second only. Becoming satisfied, however, that this method failed to bring out some important features of the reactions, I commenced a more minute study, with various methods of plotting. A comparison of these results convinced me that the best method for these results was to let the ordinate corresponding to any time represent the number of reactions having a value within  $2\sigma$  of that time. This gives a curve the main features of which may be seen at a glance, but which is, nevertheless, not misleadingly simple.

It will be seen in the following discussion that I do not place much reliance upon the lesser variations in the curves. They are probably important, but the chronoscope is too inaccurate an instrument to warrant reliance upon them.



THE TYPES OF REACTION.—My subjects, eight in number, may be divided into three groups. Group one, consisting of subjects G, B and D, required long practice before becoming thoroughly automatic. They tended toward the auditory type. That is, their thought is largely in sound terms, and their attention is readily attracted and held by sounds. The subject G sometimes distracted himself by thinking of music he had heard. Group two, consisting of subjects M, S, and De, were of the visual motor type. They could not recall sounds at all. Their imagery was all visual and motor. These subjects readily became automatic and passed through all the stages of automatism. Group three, consisting of subjects Ho and Ha, were intermediate. They were poor visualizers, but their motor and auditory memories were good. They occupied an intermediate position as regards automatism. They found it difficult to keep the attention from wandering to the experiment. Their automatism, while in general apparently very good, was easily disturbed. These two subjects experienced the most difficulty in maintaining the contact during the intervals between the reactions. Whether the correlation here appearing between the types of imagery and the tendency to automatism is accidental or significant, remains to be seen.

Fig. 1 presents a series of curves obtained from the subject G. Each curve, except the first, represents the results of reactions taken at one sitting. The abscissa gives the time of the reaction; the ordinate, the number of reactions having that time, or coming within  $2\sigma$  of it. The curves are arranged in time order, beginning at the bottom, and illustrate the progress of automatism. The subject G did not in general react automatically. He found it difficult to keep his attention away from the experiment, and when he did the reactions were often voluntary. That is, he had to turn his attention to the experiment when the stimulus came in order to react. He eventually became fairly automatic, however. His imagery is auditory and visual.

A glance at the curves shows immediately this characteristic. There are a large number of comparatively quick reactions in the earlier ones, then long reactions predominate, and then short

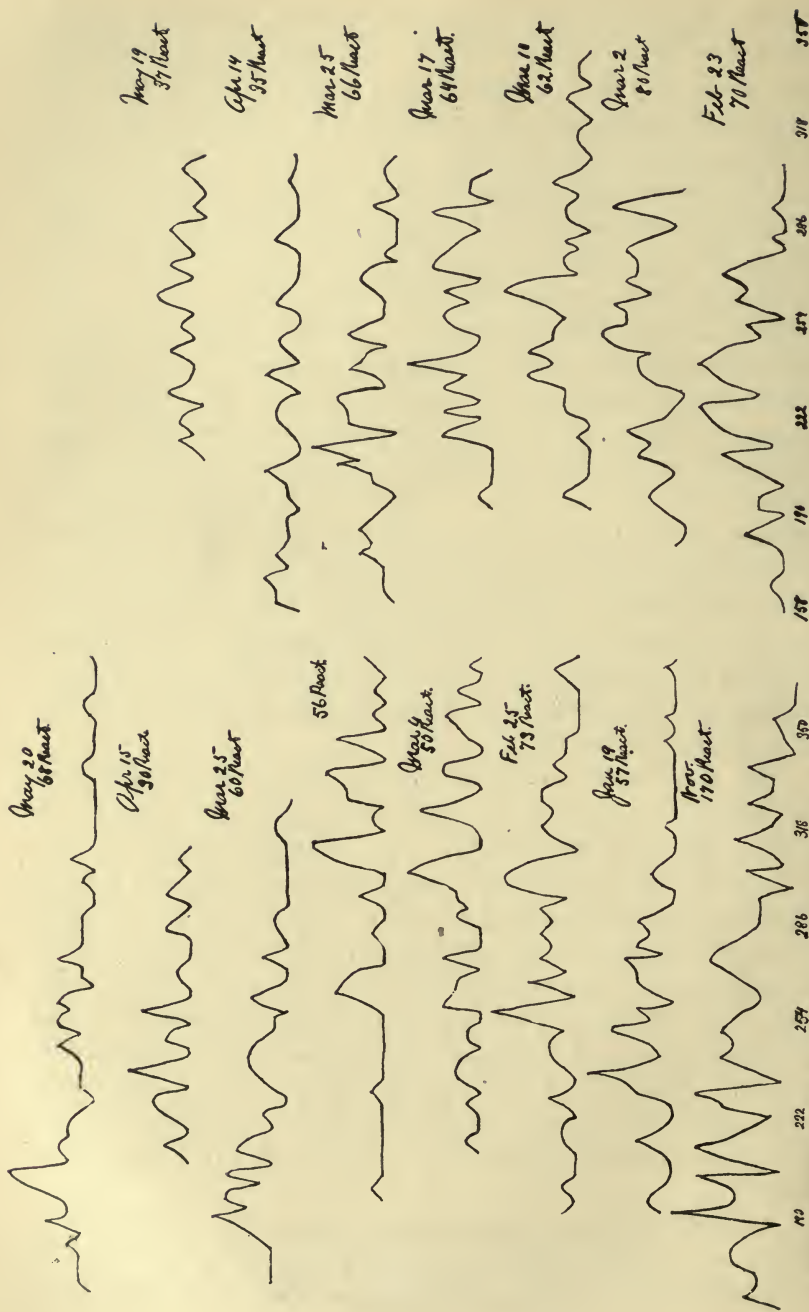


FIG. 2.

FIG. 1.—The vertical scale shows the number of reactions within  $2\sigma$  of the time shown by the horizontal.

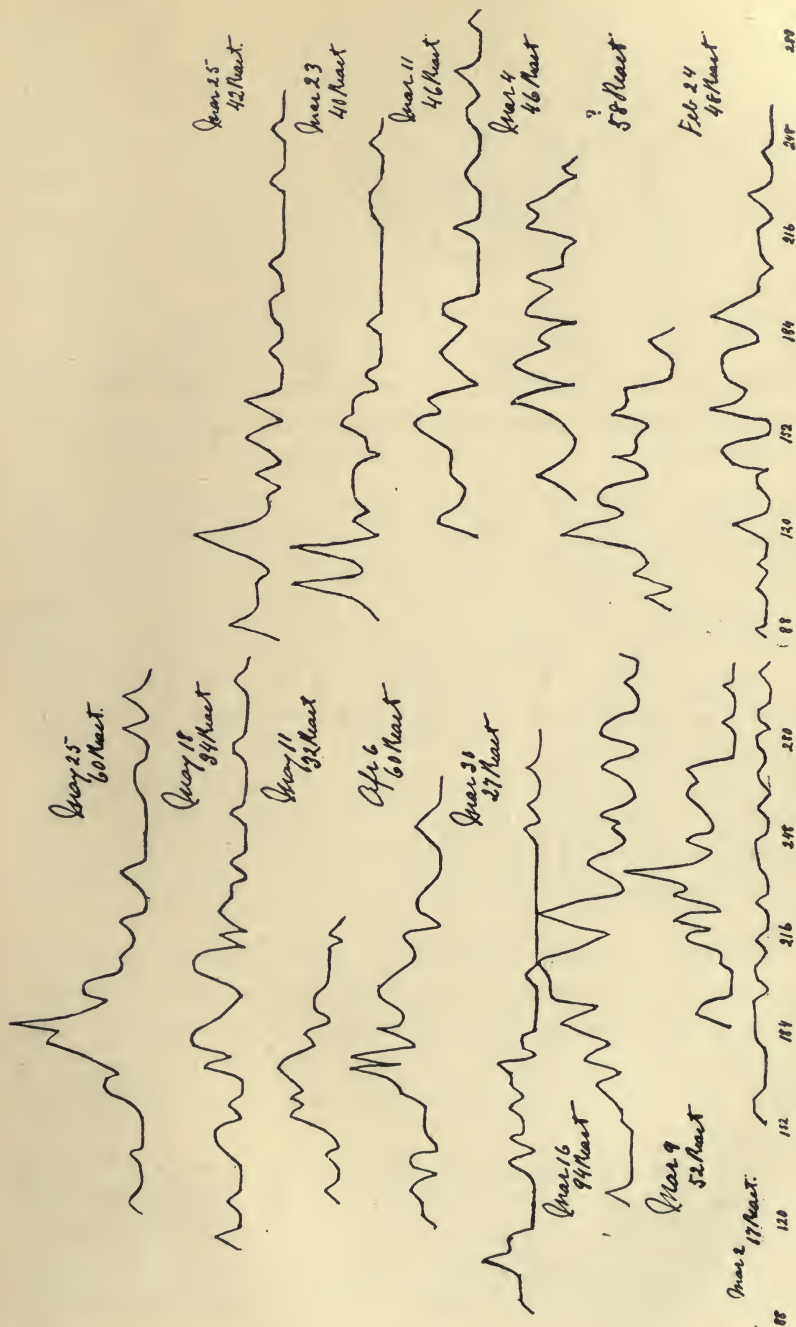


FIG. 4.

FIG. 3.



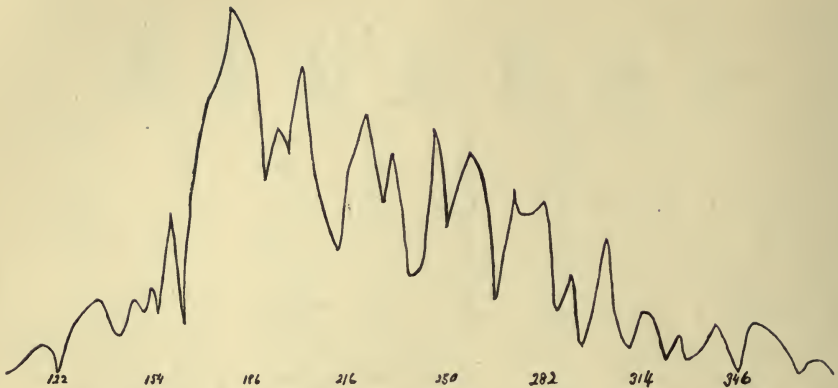


FIG. 5.—This curve shows the distribution of 518 reactions taken from 5 different subjects.

ones again. All the subjects showed this peculiarity. The reason is that at first the attention is on the experiment, and the subject in a condition of expectation, and we therefore have something near the conditions of the ordinary simple reaction. Later he learns to keep his attention off the experiment, and the reactions are slow. Then the path gets worn smooth by habit, and the various stages of automatism commence, ending in a very quick reaction.

The subject's notes amply confirm this explanation, if confirmation is necessary. G notes for the first curve, which represents the results of three days' observations during November, that his attention was more or less on the experiment all the time.

For Jan. 19 we have the note, "Attention somewhat on experiment, but not enough to give any really voluntary reactions. No very fast ones, as when attention is on reaction; nor any very slow ones, as when I do not react and then recollect myself." The results of self-observation are amply confirmed by the curve.

Feb. 25 the reactions were judged to be about 'in between voluntary and automatic.' On Mar. 25 the automatism is considered fair, and on Apr. 15 'more automatic than usual.' On May 20 the automatism was judged to be very good, and he expressed a doubt as to whether he heard the stimulus distinctly before he felt the reaction.

It will be noticed that during the period between the reactions in which the attention was on the experiment, and the appearance of good automatism, the bulk of the reactions are above  $290\sigma$ . After automatism sets in the bulk of the reactions are below  $290\sigma$ , and on May 20, when for the first time a doubt appears as to there being a distinct interval between stimulus and reaction, there are a large number below  $230\sigma$ . The results from other subjects show that these peculiarities are significant.

Fig. 2 shows a similar series of curves from the subject D. Like G, the usual imagery of D is auditory and visual. She became automatic much more quickly, however, though remaining for a long time in the first stage. The first curve represents the result of the first day's experimenting. She had very little difficulty in keeping her attention off the experiment, and after a very little practice the reactions ceased to disturb her reading. Nevertheless, it will be noticed that even in her case we have a greater preponderance of short reactions in the first curve. The next curve shown is for Mar. 2. Her report was 'Attention first attracted by sound, reaction automatic.' The next curve shown is for March 10. The number of reactions below  $230\sigma$  is now at a minimum. She reported the reactions as seeming 'perfectly regular and automatic.' She always heard the stimulus first, and then the reaction followed, without an interval between, or any movement of attention, or effort. March 17 she was asked to compare the interval between the sound and the reaction, with that between the reaction and the click made by the key on striking. She found it difficult, but thought the second interval rather longer.

On March 25 I began giving the stimuli more frequently—every  $7\frac{1}{2}$  seconds on an average, instead of every 15. The subject's judgment was that the greater frequency increased the automatism. In one sense this is apparently true. It should be noticed though that in her case, as in that of G, the introduction of the more frequent stimuli is marked by an increase in the number of short reactions greater than that of subsequent dates. I am inclined to believe, therefore, though they did not notice it themselves, that the greater frequency at first had the effect of

drawing their attention to the experiment a little, and that this effect passed away later.

On April 14 she notes that in one place the reaction and the stimulus seemed simultaneous, and that some of the reactions seemed 'impersonal.' It will be noticed that corresponding to this note we have a large number of reactions below 230, and several in fact below 180. Impersonality was never noted by her again, though on May 25 she again observed some reactions in which stimulus and reaction seemed 'almost simultaneous.'

It is to be noticed that D's reactions were nearly always below 290. The long period in which the reactions were above this, shown by G, is absent in her case, owing apparently, to the almost immediate occurrence of automatism. The subject B, the third of this type, gave results similar to G. She was a long time in becoming automatic according to her report, and her reactions showed a majority above 290 $\sigma$  for a long time. With D as with G, further, 'simultaneous' reactions were noted with the reappearance in number of reactions below 230.

The indications from these three subjects are, then, that the reaction time for automatic response to sounds begins somewhere in the neighborhood of 290. A reaction time longer than this indicates that some effort of attention or will is necessary. There is no change in this subjective condition until we reach a region below 230 $\sigma$ , when apparently a new type of automatic reaction begins. To study this other type we must turn to the records from other subjects. The exact limits of the first type, as well as the significance of the different groups of reactions indicated by the curve within this general type, had best be considered later.

As to the character of this group of subjects, supposing that it does represent a type of person, there is not, I think, any good reason for thinking the difference between them and others other than one of degree. With time and proper methods they will, I believe, pass through all the phases of automatism. I made no special effort to hurry them, for I was more than willing that some of my subjects should remain in this phase, for its better study. Instead of trying to adapt the conditions of



the experiment to the habits of attention of the subject, I made them the same for all subjects. Naturally the differing habits of attention resulted in differing responses upon the part of the subjects. As the practice was infrequent—I had none of my subjects oftener than twice a week—these individual differences had free scope.

Group 2, Fig. 3 shows a series of curves obtained from M. M is of the visual motor type. The first curve, March 2d, shows results of the first day. He reported no trouble in reading during the reaction, but an undercurrent of attention on experiment. His attention was attracted first by the stimulus. The stimulus and the reaction sometimes seemed simultaneous. The time between the stimulus and the reaction usually seemed shorter than that between the reaction and the second click.

On March 9 he reports his reactions rather regular. The stimulus comes distinctly first, then his feeling of reacting, then the sound marking the completion of the reacting. On March 16th, for the first time, some of the reactions seem impersonal. In these impersonal reactions the second interval, that between the feeling of reacting—a muscular feeling in arm or finger—and the sound made by the key, seemed *shorter* than the first interval. In a few reactions he can recognize the stimulus before the reaction, but in many he doubts whether he would know the order of events but for former experiences. On March 30th nearly all the reactions feel impersonal. The whole interval between the two sounds seems shorter, but the interval between the stimulus and the reaction feeling is about the same as that between the reaction and the second click. On April 6 the reactions are still impersonal. He gets the stimulus by a memory after-image. The reaction first attracts his attention, and then he is aware of the whole thing at once, though in the totality thus present the stimulus seems to be first. It seems to be a 'succession of things all at once.' In the latter part of the experiment the reaction was sometimes all over before he knew it, and the whole thing came as a sort of memory after-image. May 11, 'sometimes the attention is first attracted by a funny feeling marking the completion of the reaction, a restless nervous feeling. On May 18 he reported a curious feeling which

was also noticed frequently by S. He knows the stimulus has come before he really hears it. It is a perfect imitation of an hysterical anæsthesia with 'clairvoyant' tendencies. The explanation is, presumably, that the sensory nerve current passes over into a reaction before rousing its usual response in the auditory centers of the cortex. The reactions on this occasion were only partly impersonal. They were frequently entirely over before he knew anything about it. On May 25th this characteristic was still more marked, being almost unconscious toward the end. The reactions were only in part impersonal. In some cases the stimulus and the reaction seemed all one; in other cases the reaction was almost simultaneous with the second click.

It will be noticed in this case that impersonal reactions do not appear until we have reactions below 180σ; that they are not judged to be nearly all impersonal until the great majority are below this point; and that when this ceases to be the case the reactions are again only in part impersonal.

Further, it will be noticed that the first type of simple automatic reaction that predominated in the reactions of D and G—that is, a personal reaction with the stimulus coming distinctly and clearly first—is not noted after March 9th, when reactions above 230σ cease to be prominent. The indications then agree with those obtained from D and G. The first type of automatic reaction stops at about 231σ. The impersonal reactions begin below 180σ. How about the interval? The reactions between 180 and 230 are sometimes characterized by 'simultaneous reactions,' but not always. When they first occur they have this peculiarity. Afterward, though they are very distinct from both the impersonal and the simple automatic, they are difficult to describe. The subject M, it will be noted, only observed really simultaneous reactions once, though throughout the experiments he noted reactions not belonging to the other types. D, another subject of this group, only experimented once. Like M, he became automatic very quickly. He reported many 'simultaneous reactions.' The other subject belonging to this group, the writer, S, had a similar experience. I noticed simultaneous reactions very frequently at first—over a longer

period than M and De, but relatively short—but very seldom afterward. On two occasions this simultaneity was so marked and striking that I stopped the experiment to find out the record. Both showed reactions of  $209\sigma$ . In general it is not possible to get a judgment of the character of an isolated reaction—one can only get a general impression of a number. Nevertheless, though absolute simultaneity is not frequent, reactions which feel very similar to these are frequent and perfectly distinct. They are the quickest feeling reactions—unless we judge the time by the interval between the two clicks. They are characterized by uncertainty about the order of events, and a general predominance in the mass of feelings composing the total reaction—stimulus, movement, etc.—of the muscular and innervation feelings. Before passing to the general discussion of the results, however, it will be worth while to consider briefly the third group.

Fig. 4 gives a few curves from the subject Ho. Ho is a poor visualizer, but has a good auditory and motor memory. He was rather erratic in his reactions, sometimes being very automatic, and at other times not so. The first curve shown is for February 24th. By that time he had settled down to greater regularity. He notes that his attention is first attracted by the reaction. Also, that throughout the experiment there is a slight feeling of tension in the arm. For March 4th he notes some impersonality. March 11th, sometimes simultaneous, sometimes impersonal. On March 23d, "Not as automatic as usual condition of expectation. Some simultaneous, very few impersonal. Second interval most marked." On March 23d the more rapid stimuli—every  $7\frac{1}{2}$  seconds—were first introduced. With Ho, as with the others, their first introduction is marked by a preponderance of shorter reactions. But Ho notes a disturbance of the automatism, which the others did not. I believe the explanation is the same in all cases, but that only in Ho was the disturbance great enough to be noticed. In this case, whenever the shortness of the reaction is due to *attention* being on the experiment, the *short reactions do not feel impersonal*. Simultaneous reactions are, however, noted, though there are but two or three reactions within the interval where they usually occur. Both facts are, I believe, significant.



The last reactions are noted as very impersonal. The first interval seems the longer.

Perhaps the most important thing in these reactions is the indication of a fourth type of reaction below 130. The subjective conditions corresponding to these low reactions do not seem to differ much from those of the third type. They are, perhaps, a little more strikingly impersonal, and the second interval is still shorter. But these differences might appear in the third type after practice had worn its path smooth and the subject had grown more accustomed to its observation. I am inclined to think, therefore, that the difference between the paths indicated by these two groups of reactions does not involve any difference in consciousness; that the change is entirely in the lower centers.

GENERAL DISCUSSION.—Until the facts are more clearly established I do not feel justified in taking up the time of the readers of the REVIEW with a full discussion of their significance, for this would involve the presentation and examination of a much larger number of curves, a very tedious discussion, and, in the end, still much doubt and uncertainty. This would be worth while only if no more conclusive evidence could be obtained. But as I believe that more extensive experiments will save this, the proper course seems to be to give only a brief statement of the most general conclusions to which the experiments have led me.

Above 290σ we have reactions in which some element of will appears. In the slowest there is an idea of the movement about to be made. In those nearer to 300σ there seems to be no *idea* between the stimulus and the reaction—nothing but a feeling of voluntariness, of somehow willing what takes place. This is not the feeling of effort mentioned as one of the elements of a sensory motor reaction in my paper on ‘Normal Motor Automatism.’<sup>1</sup> The feeling of effort does not appear in these simple movements, unless the subject gets tired. It is rather a portion of what we called the ‘motor impulse,’ and described as “a *mélange* of visual and kinæsthetic material, as well as other elements not easily described, and, perhaps, really a direct

<sup>1</sup> PSYCH. REVIEW, Vol. III., No. 5, p. 498.

consciousness of a motor current." The results of these reaction experiments permit, I think, a somewhat closer analysis of this motor impulse and the stages of its disappearance. The 'visual and kinæsthetic material' seems to disappear first, and then this peculiar will feeling. My chief evidence for this view is the statement of the subject G, on days when his reactions were largely between 280 and 340, that between the stimulus and the reaction there were 'feelings,' but no ideas or readily describable reactions.

Below 290σ we have nothing left of the motor impulse except the feeling of personal activity. In the typical reaction of this class the subject is resting quietly, when his attention is suddenly attracted by a sound—or, rather, he suddenly hears a sound, for there is no conscious movement of attention. Immediately after he *feels himself react*. Then he hears a click telling him that the key has been pressed down. During all this time he has gone on with his reading undisturbed. He is conscious of what has happened, but that is all. These reactions seem to correspond to the usual 'sensory reaction.'

The next type, from about 175 to about 225, is characterized by the prominence of the reaction feeling. When reactions of this type first appear their distinguishing feature is the simultaneity of the stimulus and the reaction. The subject's attention being fully on his reading, he is aware *at once* of a sound and a movement. He finds himself pressing a key at the same time that he hears a sound. Later he does not really hear the sound at the same time as he reacts. He is suddenly conscious of reacting, and later of two sounds. Of these sounds, the one seems to be a *memory after-image* of a sound made *before* the reaction, the other to be the *sensation* of a sound coming *after* the reaction. The explanation of this change seems to me to be this: In the first type the sensory current goes first to the auditory centers, where it awakens a response, and then to the centers, whatever they are, whose activity gives the reaction feeling, or the beginning of the reaction feeling, and then out to the muscles. In this second type the sensory current divides, part going direct to the reaction center, part to the auditory center, and rousing both to activity at about the same time. As

the new path gets worn less stimulus goes to the auditory centers, and they respond only after some time. To put it another way, with the establishment of the shorter path the attention gets more completely away from sounds. Now, whenever we fail to hear a sound immediately, and later turn our attention to it, we get it by a sort of memory after-image. This memory after-image has peculiarities of its own which enable us, or cause us, to apperceive it as such, and project it into its proper time relations, or what knowledge and habit would indicate to be its proper time relations. Thus, though the reaction is the first thing to come into consciousness, we apperceive the whole group of stimulus (perceived by memory after-image), reaction feeling and final click, according to previous experience and our knowledge of the particular circumstances. This view of the relation between the two types is in entire accord with the fact that subjects with active and sensitive auditory centers remain so much longer in the first stage than those whose motor centers are the more active.

In the third stage, the impersonal reaction, the last element of the motor impulse, has disappeared. In this type the reaction feeling is followed very quickly, if not accompanied, by the final click. Sometimes the subject heard the stimulus very distinctly before the reaction. Sometimes he is first conscious of the reaction, and gets the stimulus by a memory after-image;—but there is no doubt in his mind that the stimulus came before the reaction. What is the meaning of these observations? What has happened when the reaction becomes impersonal? The shorter interval between the reaction feeling and the final click, as well as the longer interval between the stimulus and the reaction feeling, seem to demand one, and only one, explanation. In the previous types the beginning of the reaction feeling was an activity in the cortex. In this the reaction feeling is purely a sensation from the muscles of the hand and arm. The sensory current must now go over into a motor reaction through the lower centers entirely, or, at any rate, without awakening any response upon the part of the cortex. To this extent then I believe that the theory advanced by Miss Stein and myself as to the origin of the feeling of personality is fully confirmed by



these experiments. The reaction becomes impersonal when the last center that contributes anything to consciousness drops out of the sensory motor path, and this center contributes nothing but this feeling of personality.

When we come to inquire more carefully into the identity of this center difficulties arise. The reaction feeling is the same in the impersonal and the personal reactions. It has changed nothing but its orientation, so to speak. It is felt in a different relation to the personality and the stimulus. The sensations are the same. How is it then that in the personal reactions the whole reaction feeling is timed by the part of it which simply gives its personal coloring? This fact suggests the view that this last center, which gives the personal relation, is a kinæsthetic center, and includes a feeling of the reaction identical with that furnished by return sensations alone. But this view in turn has, it seems to me, grave difficulties. All the kinæsthetic part of the sensory motor path seemed to have dropped out before the first stage of automatism. Moreover, in the personal reaction one is not conscious of both the reaction feeling and the return sensations. It is necessary, therefore, to suppose that the two fuse, though occurring successively. But if we admit that nervous disturbances separated by such an interval of time may fuse into one presentation, the necessity for supposing the center giving the personal feeling to be kinæsthetic ceases. The most natural supposition, then, seems to be that it is a motor center; and that its activity gives the personal feeling to the sensations that follow. I do not mean that the activity of the motor centers gives a consciousness of personality alone. The feeling that one has reacted is not a feeling of personal activity plus a muscular feeling. It should rather be said that *when the sensations from an arm movement are preceded by a discharge of the corresponding motor cells of the cortex they are felt to be personal*. The activity of the motor cells is thus responsible for the resulting state of consciousness taking this form. The impersonality of the reaction, or its personality, as the case may be, is not part of the reaction feeling, but a peculiarity of the whole state of consciousness in which the reaction feeling is represented in all its relations to the stimulus and the second click,

and to the reaction. It is this characteristic of the whole state of consciousness that is determined by the presence or absence of the activity of the motor cells.

As to the fourth group of reactions, if it exists, it must correspond to a still shorter path. The neuron whose dropping out marks the difference between this group and the preceding apparently furnishes nothing to consciousness, and is presumably outside the cortex. On the other hand, though, should it be thought that the feeling of personality comes from a kinæsthetic center, and that this is anatomically distinct from the motor zones of the cortex, the way is open to regard the fourth type as the first purely 'extra-cortical.' In the present state of our knowledge of the finer anatomy of this sensory motor path and the meagerness of these experiments it would be unprofitable to discuss further the correlation of the different types of reaction with known sensori-motor paths.

As to the third question, the relation of attention to reaction time, these experiments show that all types of reaction are possible without the attention being on any part of the reaction—in so far, that is, as we take the length of a reaction as an index of its type. They further indicate that the will has nothing to do with the ordinary reaction, its function being confined, after a little practice, to placing the sensori-motor path in a condition favorable to rapid reaction. The muscular reaction is practically a reflex—as the Leipsic school contend—and the sensory reaction is at least automatic.

Professor Angell's<sup>1</sup> view that the ultimate effect of practice is to reduce both types of reaction to the same time, seems to me to be confirmed by these experiments. Professor Baldwin's view, that the subject's habits of attention, as reflected in his usual imagery, is an important factor in determining his behavior in reaction experiments, seems also to be in accord, though my experiments do not throw any light on the more specific suggestions made by him as to the exact way in which these habits influence the simple reaction.<sup>2</sup>

My observations on the earlier reactions, when the subject's

<sup>1</sup> PSYCHOLOGICAL REVIEW, May, 1896.

<sup>2</sup> PSYCHOLOGICAL REVIEW, 1895, p. 259.

attention was still in part on the experiment, would lead me to believe that the principal effect of attention in this case is to bring the entire motor mechanism into a condition of heightened sensitivity. As a result, when the stimulus comes, all the paths, or many of them, are used. The reaction time is, of course, the time of the fastest; but the current also traverses the others. On this account the reactions never feel impersonal, but do very often feel 'simultaneous.' The motor cells always respond before the return sensations from the reflex reaction have arrived, and give the reaction a personal feeling, even though, in fact, it is reflex. But the division of the current between the paths of the first and the second type is the most favorable condition for 'simultaneity.'

Before closing, a few words may be said concerning the smaller groupings shown by the curves. Though in the curve representing a single day's reactions it is to be expected that some of these groups are mere matters of chance, this explanation will not hold for large numbers of reactions. In fact, a glance at the curves will show a great deal of uniformity in this respect, showing that even as few as thirty or forty reactions will give reliable groupings. Especially is the location of certain of the minima very constant from day to day. Apparently the changes in reaction time due to practice, and even the differences between one individual and another, are due primarily, if not wholly, to the relative preponderance of different groups, rather than to change in the time corresponding to the same group.

Fig. 5 shows a curve obtained from the reactions of five different subjects, during two weeks in May. I select this period because both subjects and apparatus were fairly constant in their behavior throughout it. It will be seen that the groupings are by no means destroyed by this combination of the results from several subjects and on several different occasions. More heterogeneous selections of results also continue to show the grouping in a very marked manner, but not so satisfactorily as this.

It will be noticed that much of the grouping shows a large group separated from its neighbors by deep minima, which is divided in turn into two groups, separated by a much slighter



minimum. This smaller grouping I do not consider reliable, as it may be largely due to the chronoscope. The larger groupings can hardly be so explained, and since they are not marked by differences in consciousness they presumably represent differences in the sensori-motor path outside of the cortex. The detailed discussion of this subject, however, I reserve until I can present fuller and more exact results.

In concluding, I wish to express my thanks to Professor Münsterberg and to my fellow-students in the Harvard Laboratory, for cordial coöperation and assistance.

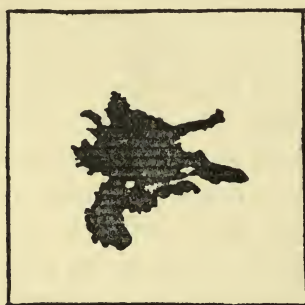
## RECOGNITION UNDER OBJECTIVE REVERSAL.

BY GEORGE V. N. DEARBORN,

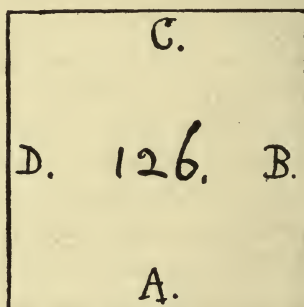
*Columbia University.*

This research was carried on in the Harvard Psychological Laboratory during the first five months of 1898. It was undertaken for the purpose of determining the facts as to the relative ease of recognizing objects when seen a second time, but under various degrees and modes of turning or reversal in a plane at right-angles to the line of sight. Knowledge of these conditions has value and interest to psychologists on more than one account, for the problem of recognition is inter-related with the whole theory of space perception and with that of vision in general, while the curious relations in which right and left are apperceived by the subject are herein also implicated. The comparative ease and accuracy of the recognition of objects appear to be the sole criterion by which the relative naturalness of seeing, so to say, may be reduced to figures and so to scientific exactness, for we are all so fully accustomed to seeing things in any possible mode or degree of reversal, both objective and subjective, that comparison with a normal position in each case, with judgments by various subjects and in very numerous cases, is the only practicable means to reliable information on the subject. But in order that such recognitions may approach the threshold difficulty many objects quite unfamiliar to the subjects must be employed, yet objects sufficiently like each other to allow of reasonable comparison. These conditions, of a very large number of unique and unfamiliar objects easily made and handled, and comparable in all respects, are well satisfied in the choice made of the essential apparatus of this research, namely, the blots of ink, whose usefulness in Psychology was suggested by the writer in the REVIEW for May, 1897, p. 390, and illustrated and employed in a research into imaginations in the

*American Journal of Psychology* for January, 1898 (Vol. IX., No. 2, p. 183). In the present case these blots were made, each unique, to the number of about four hundred, on bits of white paper 4 cm. square and each attached to a card of thick pasteboard of equal size and shape; these were kept, arranged in order, in long, closely-fitting metallic cases. The blot-cards were numbered consecutively by figures on the backs, while the four edges of the card were lettered respectively A, B, C, and D, the arbitrarily chosen normal position of the blot or character being that in which A was at the bottom or, when lying flat, nearest the subject; similarly for the other letters, each represented a quadrant of reversal from the norm. The front and back of blot 126 are here reproduced, actual size, as an example.



Front.



Back.

Besides this regular series of blot-objects, each quite unique, there were employed twenty-one pairs of blots in which the components differed only in that each was to its mate as the right hand to the left, the so-called mirror-reversal. These were numbered and lettered similarly to the rest, with an R preceding the number. These ink-blots, thus prepared and marked for exact determination at all times and in whatever position, constituted really the simple apparatus of the experiments.

The schedules by which the characters were arranged invariably in series and sets of series, and in that order successively exhibited and judged upon, were made out by their numbers alone—that is, without any regard whatever to the



form or other quality of the blots themselves. They thus being taken quite at random in preparing their order, there was no possibility (because of their large number) that any differences that might exist in their suggestibility, ease of being remembered, similarity of successive characters, etc., should obtain, and so vitiate their proper recognizability; for 'chance' is in such a case, in the long run, a better safeguard than any deliberate selection by an individual could be.

To secure the requisite precision and a record of the times of observation required for each judgment, a simple and ordinary electrical apparatus was arranged, which may be described as follows: On the subject's table convenient to his left hand were fixed two keys, each actuating a pen tracing on a slowly revolving smoked drum, the left line indicating always the 'yes' judgments and the right the 'no' judgments. Between these record-lines, an inch or less apart, a time-line marking seconds was traced by a similar pen worked by a Lough electric pendulum. Thus in small space there was kept a complete record of every judgment, both as to its quality and as to the exact time its judging required. Subjective notes were also regularly written with the other records on the drum. Convenient to the right-hand of the subject, and piled face-up in a frame made to fit, the various blot-cards were successively exposed by the subject and judged upon.

TABLE I.

SET V.									
Series 41	Series 42	Series 43	Series 44	Series 45	Series 46	Series 47	Series 48	Series 49	Series 50
281	R166	R166 <sup>1</sup>	299	306	316	320	327	334	341
282	282C	292	300	307	317	321	321B	335	342
283	288	293	293D	308	308C	322	328	328C	343
275B	R167	R167 <sup>1</sup>	301	309	318	323	323B	336	336D
276B	289	294	294D	310	R169	R169 <sup>1</sup>	329	337	344
284	284C	295	302	311	319	324	330	330C	345
285	R168	R168 <sup>1</sup>	303	312	R170	R170 <sup>1</sup>	331	338	338D
286	290	296	304	313	313C	325	325B	339	346
279B	291	297	305	314	314C	326	332	332C	347
287	287C	298	298D	315	R351	R351 <sup>1</sup>	333	340	340D

The 368 blots employed were arranged in successive series of ten, with ten such series in a set, one set being the number

judged upon at each day's sitting. The plan being to expose to the subject certain of the blots twice, or sometimes thrice, in various quadrants of reversal, the five sets were arranged in a manner best conveyed by the representation of an actual set-scheme here. Cf. Set V., reproduced in Table I. This represents the invariable order in which the series of blots were presented to the subject, always with an accurate interval of *three minutes* between the beginnings of the successive series. Thus the time which elapsed between perception of the character in its normal position and the judgment as to recognition was in each case nearly constant. It will be observed that, with the exception of Series 45, in which all the blots are new to the subject as a test, 30 per cent. of the object-figures are repetitions, a fact, of course, quite unknown to the subject, as were all other details of the schedule. In this set Series 41 will be seen to have its quota of repetitions like the rest, but judgments upon the three have only secondary interest, and are not counted, because seen perhaps days, instead of the regular three minutes, before. In Series 42, then, it is evident that the second, sixth, and tenth characters were repeated and quite inverted, as the *C* in each case indicates. In Series 43 the first, fourth, and seventh blots were repetitions; but here the normal (*A*) positions of the mirror-reversal were compared, as also is the case in Series 47. In Series 44 and 50 the degree of reversal was  $270^\circ$ , or three-fourths (*D*); and in Series 48,  $90^\circ$ , or the *B* reversal. By such a degree of irregularity in placing the repetitions in the series all chance of suggesting any regularity to the subject was avoided.

The instructions given to the subject were as follows: "Make your judgments yes or no in answer simply to the explicit question, Have you ever seen this blot before? Make your judgments only when a feeling of *certainty* is in consciousness equal to that attained from the comparison of two blot-characters, one seen just before and the other never seen before. When ready, make your judgment reactions with equal energy and promptness in all cases, so that the mere time-reactions, as a constant function, may be disregarded. Let a quick double reaction on a key indicate extreme certainty, and a prolonged pressure a

proportional degree of doubt. Read or converse between series, so as not to review in imagination the blots just seen. Report all possible subjective notes of interest." Thus the subject knew nothing of any reversals, of the number of repetitions in each series, nor of the purpose of the research even; he or she merely answered yes or no to the question as to recognition, when 'certain' of his or her judgment in that regard. Within the period of three minutes no incentive to hurry was suggested.

TABLE II.

REPEATED BLOTS.

SUBJECT.	REVERSAL POSITION.	A.	B.	C.	D.	R'A.	R'C.
I.	Recognitions. Per cent.	61	30	51	16	54	57
	Whole No. cases.	33	33	97	19	37	14
2.	Recognitions. Per cent.	33	24	39	15	45	25
	Whole No. cases.	12	21	61	14	22	4
3.	Recognitions. Per cent.	91	73	76	60	73	40
	Whole No. cases.	12	15	41	10	15	5
4.	Recognitions. Per cent.	62	25	29		40	28
	Whole No. cases.	21	12	35	1	10	11
5.	Recognitions. Per cent.	91	17	53	75	55	40
	Whole No. cases.	12	12	32	4	9	5
6.	Recognitions. Per cent.	55	33	43		50	0
	Whole No. cases.	9	6	23	1	4	5
7.	Recognitions. Per cent.	70	67	52		50	33
	Whole No. cases.	10	6	22	1	4	6
8.	Recognitions. Per cent.	0	17	33	0	0	
	Whole No. cases.	3	6	9	3	6	0
9.	Recognitions. Per cent.	67	100	83			
	Whole No. cases.	3	3	17	1	1	2
Average per cent.		70	43	51	33	46	32

The seventy blot-cards required for one sitting being, then, arranged in order on a table behind the subject seated at the apparatus, the method of procedure was, in brief, constantly as follows: The time-pendulum being in action, the first series of



ten blots was arranged according to the scheme for that set (as exemplified above) and placed in the holder face up, but covered, before the subject, whose left hand covered the judgment-keys. At a given signal, whose exact temporal position was carefully marked on the time-line, the subject lifted the blank covering card and exposed the first blot, reacted yes or no in the proper manner when his judgment was made, turned the used blot-card face-downward near by (so that marginal retinal images would not interfere), then immediately proceeded to the next, and so on through the series of ten. After an interval of three minutes from the time of beginning on the first series the second was begun, and so on through the set for the day. This number of judgments, although occupying only thirty minutes, was found quite sufficient for the best work of the subjects, they generally reporting the judgments very tiring, especially those who are 'motiles' or 'audiles' in imaginal type.

Nine subjects were employed in these experiments; two of these were professors of philosophy, and the rest students and instructors in the Laboratory, one being a student of Radcliffe College.

The figures which represent the more interesting part of the results of the research are given in the accompanying Table II., useful as a matter of record chiefly. Altogether they represent over 2800 judgments (and none too large a number); of these, 30 per cent. are on repetitions. In the tables, A indicates

TABLE III.

UNREPEATED BLOTS.				
SUBJECT.	"RECOGNITIONS." (FALSE.)		NON-RECOGNITIONS. (TRUE.)	
	PER CENT.	CASES.	PER CENT.	CASES.
1.	21	126	79	466
2.	19.5	62	80.5	256
3.	51	133	49	126
4.	22	61	78	215
5.	27	52	73	144
5.	27	35	33	96
7.	24	32	76	101
8.	16	10	84	54
9.	65.5	40	34.5	21
AV.	30.		70.	

normal position of blot; B,  $90^\circ$  reversal over toward the left; C, inversion; D,  $270^\circ$  reversal over toward the left; R<sup>1</sup>A, erect mirror-reversal; and R<sup>1</sup>C, inverted mirror-reversal. The last tabulation of figures (Table III.) gives the numerical details of the judgments on the blots exhibited to the subject but once, but judged as to recognition in the same manner as the rest. Of this class there were somewhat over 2000 judgments, or 70% of the whole number of exposures. It will be noticed from this second table of result figures that seven of the nine subjects judged that they recognized approximately one out of every five of the blots which in reality they had not seen before, about 22% of their so-called 'recognitions' of these unrepeated blots being mistaken. Of the two remaining subjects, one (number 3), with 259 of this class of judgments, 'recognized' over 51%, and the other (number 9), who, by the way, reported especially 'certainty in most cases,' thought that he recognized 65.5% of characters which he had never before seen. On the average the percentage of false 'yes' judgments was 30%. The cause of a part of this error rate is evidently to be found in the actual formal similarity which some of these chance blots bear to each other, a circumstance not, however, to be eliminated from any set of objects of necessity so numerous; indeed, in these characters this similarity is reduced to a degree which lends continual interest to their use. Cases of great doubt were most often, and not unnaturally perhaps, put on the 'no' line of judgments, a 'doubtful' key and record-line having been for a time employed, but of necessity soon abandoned, because through its over-use the research threatened to be vitiated.

Having now before us more or less complete the data of the experiments, let us try to make more plain the circumstances to which they relate.

The objective conditions of the research are obviously the simplest which are logically possible for a comprehensive study of the natures of reversal and of recognition, the mental confusion of memory blot-images, arising from the large number seen, serving only to reduce the stimulus to the threshold-intensity—a requisite of the method here employed. Owing, however, to this confusion, in the main the 'feeling of recogni-

tion' was by no means regularly present in any degree, and often not at all, recognition usually taking place by the suggested association in the reviewing consciousness of some cognitional fact that had formed part of the perception or feeling present when the blot was first seen as the norm. Most often, indeed, it was some precise fact, remembered in terms of number, or some extrinsic suggested resemblance, or even by some wholly external complication whose relation could not perhaps be traced. Very often the recognition depended on the recall of some very small portion of the blot, such as a peculiar point or knob, or some more than usually grotesque end or corner. Though small, the characters were so rich in detail that often the whole as such was not in any proper sense perceived. Recognition, however, most often depended on apperception, and not on any feeling of recognition, and could be classed as of the mediate, rather than the immediate or general variety.

Some of the subjective notes as to the various methods of remembering and recalling may in this connection have some interest, although it was not easy to throw light on a process which required only the brief times which the subjects usually deemed sufficient for a 'certain' judgment of a blot. Four, then, of the subjects reported that the characters instantly called up actual objects by association, and that their recognition occurred by this means. Two subjects, on the other hand, reported that such products of imagination did not appear in their cases. One subject (and he who made more judgments than any of the others) reported a constant tendency for the blots immediately to place themselves in certain classes as regards general form of outline, proportional size of their various parts, mode of shading, etc. Most of the subjects remarked the immediacy of the judgment oftentimes (obviously often being wrong cases of auto-suggestion), and in cases of great doubt, that the ensuing confusion made matters worse and judgment sometimes vain. Two subjects reported their method to be to 'count the tails' or projections of the object. The two subjects who made the greatest number of wrong 'yes' judgments made likewise the greatest number of right 'yes' judgments, one of these two being he who reported 'great certainty,'



and the other a man known in his college for his self-asserted unmistakability. The subject who made the smallest average of recognitions was a man noted as an unusually partial 'motive,' who often reported it most difficult for him to recognize the blots at all, although he made more judgments than any of the other subjects save one. This subject, with two others, also reported that he never apperceived reversal as such, while to others it was regularly in consciousness when it occurred. Although the same set of blots was judged upon by the same subject in some cases several times on different days often a week or two apart, no improvement was visible in the record of such a set, and no suspicion ever entered the subject's mind that this repetition was occurring. This was true in one case where the subject saw the same set six times with no storable improvement. Owing to this circumstance hundreds of blot-cards sufficed where else thousands would have been required.

The time required for each of the more than twenty-eight hundred judgments was exactly recorded; but it has appeared that so great is the complexity of the conditions subjectively and objectively, especially as regards individual differences and as to temporary mood, that nothing of interest in this direction can be given in precise numerical terms. The reaction time for the mechanical process of uncovering a blot, pressing the proper key and overturning the blot-card when used, was, approximately on the average, one second. The total times vary then from almost this period to often eight or ten seconds; the average time required was not far from three seconds (and this when nearly as many minutes was the outer limit). Length of judgment time seems to have no constant relation to accuracy, owing evidently to the quickly-arising confusion on introspection; yet the most accurate judgments on the average were made by the subjects whose time records are the most irregular, as occasional retrospection on the occurrence of a doubt would necessitate. The inevitable effort which most subjects make to produce a rapid record, despite remonstrances, did much here, as elsewhere in psychological experiments, to reduce the accuracy of the judgments; but as long as human nature remains as it is it will be so.

The purpose of the research was to determine the relative recognizability of objects erect and in various modes and degrees of reversal. Let us examine the results in this regard, and try to suggest their meaning, psychological and physiological, as far as may be. Of the blots repeated or reviewed in the normal or A-position, recognitions were on the average 70%, and this almost exactly corroborates the average percentage of right judgments as to unrepeated objects, 70% being there also the nearest whole number—in other words, the average of recognitions of erect repetitions and the average of non-recognitions of unrepeated blots exactly prove each other. This 70% is, then, properly the standard of the research, with which the averages under the various reversals are to be compared. This is the proven general personal equation, so to say, of recognition of these objects when repeated exactly, without any objective complications. Using this, then, as the standard of 100% (A-position, or normal), B-reversal, or a quarter-turn (in a direction opposite to the hands of a watch), gives 61.4% of recognitions; the C-reversal, or complete inversion, 72.8% recognitions; the D-reversal, or a turning of  $270^\circ$ , 47.1%; the R<sup>1</sup>A reversal, or erect mirror-position, 65.7%; and the R<sup>1</sup>C reversal, or inverted mirror-position, 45.7% of recognitions. In other words, it appears from the research (and this is the kernel of its interest) that *an object is recognized more readily when inverted than in either of the two intermediate portions of quarter-reversal, and more readily than in the erect mirror-position or that position inverted*—an object upside down appears more natural than when turned on its side or seen in a mirror. Furthermore, one-quarter reversal toward the left is more favorable to recognition than three-quarter reversal (important only for certain forms); while least favorable of the six positions compared in these experiments is the inverted mirror-reversal, most rarely encountered of them all in general experience.

These facts are simple, while their explanation in psychological terms is neither so easy nor so sure. Yet something in that direction may be suggested here.

The great Law of Habit, individual and inherited, seems in general to furnish sufficient reason why uncomplicated repeti-

tion—that is, when the blot-object is repeated in the normal position—should be more easily recognized than in any case of several, this being the condition ordinarily in experience. Experiments in which the experimenter wore for some weeks before his eye a lens which inverted his field of vision have proven that it is comparatively easy to get accustomed to objective inversion even of objects seldom or never ordinarily seen so reversed. Indeed, to the lowest orders of animal life inversion must be the rule of their experience, to them making no difference. It is easy to conjecture that a sufficient degree of atavism in vision is easily brought about even in cases like those of the present research, making inversion relatively natural; furthermore, it must be remembered, that the retinal image is an inversion of the object, a fact adding little here, save of possibility. It would be interesting to know from experiments if a person unfamiliar with either condition could not learn to read print upside down more easily than print made from type turned on their sides. At any rate, the perfect facility with which printers read directly from the type in any position as ordinarily set shows how easily reading under inversion becomes natural.

Again, on the other hand, vision of objects turned on their sides (or one-quarter reversed) is very seldom experienced, indeed, and especially in the case of unfamiliar objects. Nothing in organic structure or in physiologic habit affords practice in this sort of recognition. The longer axis of many of these blots runs vertically or else horizontally, and from the fact that the right halves of both retinae are 'supplied' by only one visual center, the left, and *vice versa*, it is obvious that a quarter reversal of these objects would involve more new brain elements than would their inversion, and so make them seem less familiar, often very likely, in the former case, stimulating both cortical sight centers as not in the latter case; at any rate, implicating else unused 'apperceptive cells' or regions.

As regards the apparent difficulty of recognition under three-quarter reversal over that in the case of one-quarter left reversal, it is pertinent, perhaps, that our almost universal habit in reading is to begin at the upper left-hand corner of the page or card, thence looking along the top; and similarly more or less in per-



ceiving all plane representations. This firmly-fixed habit doubtless holds in the perception of these blot-characters, the spot receiving the focus of attention at first being undoubtedly in general the upper left-hand quadrant of the object, or at least so in some degree. A quarter-turn then would present for recognition a more or less familiar percept, while three-quarters reversal by the same principle would offer to the attention a wholly new portion of the blot, a portion, in fact, at the first instant of viewing, quite out of range of the habitual perceptive field, and so, from this circumstance, less fully apperceived.

With the mirror-reversal, finally, all are fully familiar from early and constant perception of the hands, feet and limbs in general, and from considerable experience with the use of mirrors, both natural and artificial. Here again habit affords relations which our research only exemplifies. Here, too (and more exactly), the easily acquired habit of reading directly from type is an instance and an illustration. Add inversion to the condition of common mirror-reversal, and the most complicated position of the six here studied is produced, a relation to the subject practically never experienced under ordinary conditions. Habit here has had no chance to produce an effect, and we have found that the percentage of recognitions is in this case the lowest of them all.

Further experiments should amplify these results, employing yet more involved relations between subject and object, varying them indefinitely. Especially would it be of interest to know if subjective reversal of various sorts would bring out the same results as to the habits of our seeing. Indeed, subjective reversal would seem to be a field fertile in many respects, both physiological and psychological.

## SHORTER CONTRIBUTIONS AND DISCUSSIONS.

### A LECTURE EXPERIMENT IN HALLUCINATIONS.

An experiment to illustrate a popular lecture must be striking, quick and sure to work. As it is not always easy to tell beforehand whether an experiment will answer these requirements, the following scheme for the production of a hallucination of smell may be worth recording. I had prepared a bottle filled with distilled water carefully wrapped in cotton and packed in a box. After some other experiments I stated that I wished to see how rapidly an odor would be diffused through the air, and requested that as soon as anyone perceived the odor he should raise his hand. I then unpacked the bottle in the front of the hall, poured the water over the cotton, holding my head away during the operation and started a stop-watch. While awaiting results I explained that I was quite sure that no one in the audience had ever smelled the chemical compound which I had poured out, and expressed the hope that, while they might find the odor strong and peculiar, it would not be too disagreeable to anyone. In fifteen seconds most of those in the front row had raised their hands, and in forty seconds the 'odor' had spread to the back of the hall, keeping a pretty regular 'wave front' as it passed on. About three-fourths of the audience claimed to perceive the smell, the obstinate minority including more men than the average of the whole. More would probably have succumbed to the suggestion, but at the end of a minute I was obliged to stop the experiment, for some on the front seats were being unpleasantly affected and were about to leave the room. No one in the audience seemed offended when it was explained that the real object of the experiment was the production of a hallucination.

Hallucinations of temperature or pain are easily induced by suggestion in susceptible individuals by the use of magnets, though the experiment is not suitable for lecture purposes. It is, of course, necessary that the subject should have hazy ideas about magnetism, but it is unfortunately only too easy to find such persons. The 'magnet' need not be magnetized, but should have plainly marked poles and the suggestion be conveyed by suitable 'patter', to use a conjurer's phrase. Sensations of heat may be produced by the north pole of the magnet,

and cold by the south, or one pole may be made to give a tingling or smarting pain in the right hand and side of the body, and the south pole on the left, or any other such scheme not too complicated. The illustrated magazine articles of the effects produced on hypnotized subjects by Luys, with magnets and sealed tubes of chemicals, are useful to reinforce the suggestions. Of course, the deception should be thoroughly explained after the experiment, not only because otherwise the subject sometimes complains of pain in the hand worked upon, but also in order that the experiment may serve as a lesson to the subject no less than to the spectators.

Slight hallucinations of sound are easily induced; but I have never succeeded in getting un hypnotized subjects to see red and blue flames on the poles of a magnet, or in obtaining any similar hallucinations of sight. Simple experiments in suggestion on persons in a normal state are generally better for demonstration than the more striking results obtained in hypnosis.

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#### PROFESSOR HYSLOP ON MYSTICISM.

In the last number of *THE PSYCHOLOGICAL REVIEW* Professor Hyslop criticises a paper on Psychology and Mysticism which I published in the *Atlantic Monthly*, and have since reprinted as the last essay of my recently published book 'Psychology and Life.' My paper was for him 'one of the most amusing documents that he has ever had the pleasure of reading.' I have not the slightest desire to disturb this happy mood of amusement by a serious defence against his attacks. A scientific defence or discussion must have as its aim that the opponent shall understand and agree with me; but I feel myself so absolutely free from this ambitious aim that a discussion is really superfluous. In regard to only one passage of my paper does he claim that he does understand what I wish to say and would agree with me; it is my reference to communication. "As to what Professor Muensterberg may intend by this description of the communication of ideas I can well imagine. But I can do it only by having some knowledge of the process myself, and not from any statement that he makes." And then he goes on to interpret my meaning in a way which is, in every respect, the exact opposite of my thought, and which would deprive my arguments of all meaning. If he had not found anything in the paper which he believed himself to understand, I



should, perhaps, have taken the trouble to enter into a discussion that he might feel that he understood me. But after this test case I know that we think with a different logic, and I prefer that my statements continue to be for him 'blank nonsense.' I, therefore, do not argue, but wish merely to put straight a few facts which Professor Hyslop mentions as if he objectively reproduced my own words and statements, and where the reader of his criticism might believe that I am truly represented.

Professor Hyslop says: "His reason for not making a personal investigation into this question is that it is not 'dignified to visit such performances' as Séances!" That is all, and what follows are merely exclamations of contempt for such an utterance. My text says this: "I consider it undignified to visit such performances as one attends a variety show for amusement only, without attempting to explain them." Is it really possible not to see the difference between my statement, with which every decent scientist will agree, and his false denunciation, which must make me contemptible to every scientific man?

Another illustration: "Professor Münsterberg did not distinguish between the relevancy of the various alleged phenomena that he was criticizing; table turning, telepathy, clairvoyance, hypnotism and what not were lumped together with no more conception of their differences than is usually displayed by the spiritualist himself." This is, indeed, very bad on my part; but the reader will become a little milder if he chances to take the trouble to open my article, and convince himself that more than half of the paper is expressly devoted to the clean discrimination of these and similar conceptions, and to the disentanglement of hypnotism from the rest.

A third illustration: I had said that up to the last summer vacation, in which I read systematically telepathic and spiritualistic literature: "I had not really studied all the recorded Phantasms of the Living and all the Proceedings of the Societies for Psychical Research, and I am afraid I had forgotten to cut the leaves of some of the occult magazines on my own shelves." Out of this material Professor Hyslop makes a confession, on my side, that until the last summer vacation I felt guilty of forming and stating opinions on this subject 'before reading its literature.' Because I have not read 'all the reports' and 'all the proceedings' I have not read the literature. If I really did not care to read the literature, why did I then subscribe for the occult magazines on my shelves? And immediately after it, Professor Hyslop says that he himself thinks that there are not twenty-five volumes in existence on this subject that any sane man ought to read

at all. As all the reports fill hundreds of volumes, he thus says clearly that a sane man ought not to read them all; and yet because I say that I had not read them 'all' he denounces me for confessing that I formed opinions 'before reading the literature.'

I do not care to go on; the other remarks are in the same spirit. Professor Hyslop says about me: "He thinks the scientist is trained to an instinctive confidence in his coöperators;" and he answers: "A man who cannot protect himself against fraud must not expect his opinion to be worth very much." I think both sides are correct here. I think, indeed, that a scientist is trained to an instinctive confidence in his coöperators, and I for one am inclined to consider in this sense even my critics as my coöperators, expecting that in spite of disagreement they will quote me correctly. But if the distortion transcends certain limits, I think Professor Hyslop is right in demanding that the scientist ought to discover it, and thus to protect himself in spite of his instinctive supposition that such things are impossible.

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## PSYCHOLOGY AND LIFE.

The appearance of Professor Münsterberg's book with the above title calls attention afresh to the various points which have been criticised as the chapters have appeared in the *Atlantic Monthly* and elsewhere.

Professor Münsterberg refers to my former reply to one of his articles (not reprinted) as an unjust criticism, and one which leaves his opponent still unreconciled. I hasten to assure Professor Münsterberg that his subsequent article on Education, reprinted in this collection, fully grants all I ever thought of asking in that criticism, and much more.

I do not think it wise, at present, to try to teach experimental psychology in the high school. At the same time I wish to protest against the fundamental position of this whole book, in the hope that certain other points of difference may be as happily adjusted.

I am certainly in hearty sympathy with the great questions at stake. Professor Münsterberg is to be congratulated upon the whole-hearted way in which he emphasizes the realities of life, of the will, of feelings and their values, over against the mechanical, lifeless forms of which we see so much in current psychology. But what higher law makes it necessary to set these realities outside of psychology?

Who determines the limits of our science, and says we shall not include in it anything not fully explained by the law of causation? Who says the biologist may not stand face to face with the facts of life even while he is a scientist?

Undoubtedly the feelings of effort and strain connected with will acts are sensations; but, after taking them away, very much is left. Why, then, is it necessary to step outside of our text-book of psychology to say so, and to call upon the student to go to real life to see for himself what mind is?

The same is true of the emotions. The pleasures of expanding chest and relaxing muscles as one watches a sunset are sensations; but something remains after these are subtracted, and psychology has a right to call attention to the fact.

If necessary, in order to admit these subjects to psychology, I should be willing to question the fundamental propositions in Professor Münsterberg's epistemology which makes it necessary for him so to limit psychology. The world of things and the world of ideas are not two sides of the same thing in any sense in which the world of will and feeling is not also a side of the same reality.

All the history and traditions of American psychology call for a study of real life. That is what has given psychology the place it has always held in our colleges and universities; what gives it its firm position among the sciences to-day. Psychology is dealing with real life, and is able to make that life richer and fuller. Experimental psychology's recent development rests upon that very fact.

No sensationalism or mechanical theory of association will ever take the place of a Hopkins, a Porter, or a McCosh.

Professor Münsterberg's insistence upon the realities of the inner life is but one sign of a returning of psychologists to real life. Let us make our psychology broader and deeper, not give it up altogether, if this, his new view, prevails.

We might go back to the old name, mental philosophy; or we might adopt some new name, like ethology, which has been recently suggested. But the word psychology has proved its right to remain. In the minds of men at large it does stand for reality, and that is why the world is turning to psychologists for solution of the problems of life. We ought not to refuse to answer while we settle questions of terms or the imaginary limits of our science.

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A REPLY TO "THE NATURE OF ANIMAL INTELLIGENCE AND THE METHODS OF INVESTIGATING IT."<sup>1</sup>

My first duty is to beg the reader's pardon for a certain personal tone in this discussion. As Professor Mills has mentioned Dr. Thorndike twenty-nine times in his article, this reply will of necessity contain the word 'I' oftener than one would wish.

There are two sorts of assertions in Professor Mills' article: first, a number of important objections to a certain method of studying animal psychology; second, a number of attacks on my 'Experimental Study of the Associative Processes in Animals.'<sup>2</sup> The former I am glad to have the opportunity to discuss, because they should be of real interest to all comparative psychologists. The latter can be safely left to the judgment of anyone who has read the monograph itself, and will be taken up here only because that monograph has probably been seen by only a few of the many who have read the attack upon it.

Let us turn first to the important objections to my method of studying the formation of associations in animals. I say my method, because it seems likely to be thought of chiefly in connection with my experiments, though Lubbock used practically the same method with insects. It is, in fact, odd that Lubbock's recommendation as to insects was not sooner followed with mammals. He says, "In order to test their intelligence, it has always seemed to me that there was no better way than to ascertain some object which they would clearly desire, and then to interpose some obstacle which a little ingenuity would enable them to overcome" (Ants, Bees and Wasps, N. Y., 1896, p. 247). He used food as the 'object,' as I did, and interposed mechanical obstacles as I did.

Professor Mills' weightiest objection is that, when confined while hungry in such boxes and pens as I used, the dogs and cats were in a 'panic-stricken' condition and, therefore, temporarily lost their normal wits. Now, it is true that in many of the trials with cats and chicks, notably the first ten or twenty trials with each animal, there is often, as I fully noted, great violence and fury of activity. And this *might* be the result of mental panic, and so might be a sign of a loss of normal mentality. But the animals (the dogs and some of the cats) which did not display this excitement and fury did not display any variation in the results toward more intelligence. Nor did the animals

<sup>1</sup>By Professor Wesley Mills, pp. 262-274 of the May number of THE PSYCHOLOGICAL REVIEW.

<sup>2</sup>*Animal Intelligence*, Monograph Supplement, No. VIII., to THE PSYCHOLOGICAL REVIEW.

which showed certain results in the experiments of which confinement in small boxes was an essential feature show any variation from those results in the experiments (see pp. 87-91 and 96 of the monograph already cited) in which there was no excitement, no different activity from that shown all the time. In these experiments the cats were in the big cage which had been their home for weeks.

Furthermore, it seems unlikely that in the case of the animals which had already been the subjects of two or three experiments, and which had been in such boxes a hundred or more times, the violence and fury of activity could have been the result of fear or in any way a sign of its presence. For, as was stated in the monograph, such animals which have been made during a number of trials to crawl into these boxes which Professor Mills supposes were so disturbing to them, *habitually of their own accord went into them again and again.* Nor did they try to escape when I picked them up to drop them in. In the experiments in which I moved the animal's limbs, putting him through the movements, there was after from 0 to 12 trials no fear of my handling. (See p. 68 of the monograph.)

In short, all evidences of panic may be absent without any change in mental functioning, and the only cause of mental panic which would seem probable, namely, *fear*, was certainly not present in the greater number of the experiments. So I feel bound still to maintain the account given in the monograph, and attribute the animal's fury of activity not to mental panic, but to a useful instinctive reaction to confinement. It should be remembered that even in the midst of the utmost activity the cats would take instant advantage of any chance to escape which appealed to their instinctive equipment (*e. g.*, the widening of an orifice). It should further be remembered that the most violent animals did the most pseudo-intelligent acts. If any one of the eight or ten psychologists and biologists who saw the experiments in progress had seen signs of mental panic in the animals I should have inserted this discussion in the monograph. But I venture to think that if Professor Mills had repeated five or six of my experiments he would have discarded this mental panic objection.

The next important objection is that the surroundings were unnatural. I myself long since criticised my method on these grounds,<sup>1</sup> and I am and always have been ready to admit that an animal may be able to reason with certain data, to imitate certain acts, and yet be unable to reason with the data with which you confront him or imitate the act you present as a model. For that reason I chose varied acts,

<sup>1</sup>See *Science*, Vol. VIII., No. 198, p. 520.

very simple acts, trying each with different animals and making many of them approach very closely to acts common in animal life, and making others practically identical with acts which have been recorded as proofs of high mental ability in animals (vide the experiments with boxes C, D and G). We have seen that so far as the mere being in boxes is concerned the animals soon got used to it, did not fear it, and presumably could and did use their mental powers while in that situation. If Professor Mills had specified some particular situation as unnatural, and argued in concrete terms that its remoteness from the ordinary conditions of animal life made it unfit to call forth what mental functions the animal had, I should here either try to show that it was fit to call them forth or confess that from the animal's conduct in it no conclusion could be drawn save the one that the animal's mentality was such as was not aroused thereby. Even this one conclusion would be valuable. Even if we had to say, 'all that these experiments prove is that these circumstances will not cause the animal to manifest memory, imitation, etc.,' we should be saying a good deal, for the advocates of the reason theory have pretty uniformly given as evidence the reactions of animals to novel mechanical continuances.

Professor Mills does not argue in concrete terms, does not criticise concrete unfitness in the situations I devised for the animals. He simply names them unnatural. Moreover, it would seem that he makes this word face two ways. When talking of my experiments, he uses the word in the sense of novel, unfamiliar to the animal. When arguing that my conclusions are wrong, he uses the word in the sense of beyond the limits of their mental functions, abhorrent to their normal intellection. Of course, the former may be true and the latter false. The fact that cats are not ordinarily treated as mine were does not imply that my cats could not and did not come to be at home in the life I imposed on them to such an extent that they could use therein all the general intellectual functions they possessed. Professor Mills himself has based statements about the presence of certain mental functions on the conduct of a kitten in gaining a certain resting-place (in a bookcase, if I remember rightly), in spite of mechanical obstacles interposed. The situation here coped with is as 'unnatural' as that in a majority of my experiments.

The general argument of the monograph is used in all sorts of scientific work and is simple enough. It says: "If dogs and cats have such and such mental functions, they will do so and so in certain situations and will not do so and so; while, on the other hand, the absence of the function in question will lead to the presence of certain



things and the absence of certain other things." To provide the 'certain situations' was the task my experiments undertook. It is mere rhetoric to damn the whole argument with a word, 'unnatural.' The thing to do is to show the error in the logic or the disturbing factor in each experiment, to repeat the experiment minus that factor, get opposite results, and so refute my claims. Dr. Kline has in one slight case gained results by the use of more 'natural' surroundings and his results agree with mine. (See *Am. J. of Psy.*, Vol. X, pp. 277-8.) I may say here that Dr. Kline has in this article treated of fear and novel surroundings as disturbing features in my experiments more discriminatingly, perhaps, than Professor Mills, and that this paper is intended to be an explanation which will satisfy his criticisms as well as those of the latter.

Observational records are, as I said in the review in *Science* which has already been quoted, of very great value; but the fact remains that the host of observations so far collected, including the large number of Professor Mills' own to which he refers on page 264, had not provided us with agreement about the presence of a single general function in animal consciousness that was in dispute. I tried, therefore, to devise situations in which the conduct of the animals might be really illuminating. It would seem that Professor Mills allows that if the experiments were only free from the disturbing factors we have been talking about, the conclusions reached would be probably true, for he does not criticise the logic of the deductions. Now these conclusions are so far reaching that I am reviled for even pretending to have made such important ones. But this goes to show just that the method will, if we can show that these factors are not present, or can modify the method so as to exclude them, get us somewhere psychologically. So my general plea for experiments in animal psychology is that they at least pretend to give us an explanatory psychology, and not fragments of natural history.

Finally, just as in experiments like mine you may miss the truth by some mistake you make in picking the circumstances, the situation to test the presence of a function, so in the mere observation of the habitual life of animals or the experimental regulation of their ordinary activities, you may miss the truth by mistaking instinctive for imitative acts, associative for rational acts, permanent associations for memories. For instance, Professor Mills offers in his article, as a proof of the presence of an imitative faculty, an act (p. 268) which might very possibly have been the result of the instinct to follow common to so many young animals, so far as one can judge from his account—

“a student of McGill University has communicated to me the fact that a kitten which could not be induced to jump over an object placed before it, did so only after seeing the mother do it, and after that there was no more trouble in getting it to perform the trick.” We shall see that another observation, that of the dog and the tree, which Professor Mills quotes to refute me, may have suffered in the interpretation.

Of course, it is clear that the psychological story told by correct experimentation will not conflict with the story told by correct observations reported correctly at first, second or tenth hand. But I am not yet sure that any trustworthy observation about the interpretation of which there is general agreement, conflicts with the results of my observations under test conditions in such a way as to render necessary the presupposition that in them there was some vital flaw. Such refutation of them may come, but Professor Mills does not seem to have brought it.

So much in general defence of the methods I used. It may now be permitted to mention some matters of detail: Professor Mills finds in the printed report of my experiments signs of conceit and of lack of ‘respect for workers of the past of any complexion.’ For psychological interpretations of the sort given by Romanes and Lindsay I certainly had and have no respect, though, of course, I esteem them for their zeal. But I cannot see that the presence or absence of megalomania in me is of any interest to comparative psychology. The monograph in question was not a presentation of personal opinion, but of certain facts, the accuracy of which, and of certain impersonal inductions and deductions, the logic of which, should be attacked impersonally. The question is whether certain facts exist and what they mean, and does not concern the individual psychology of any person.

Professor Mills’ humor in making believe that because I characterize Lloyd Morgan as the ‘sanest’ of comparative psychologists, I think of them all as insane (p. 263), seems a bit disingenuous in view of the fact that his article will probably be the sole source of information about my book to a large number of people. Of course, when I wrote ‘sanest,’ I meant sanest. Had I meant ‘least insane’ I should assuredly have so written. On page 264 our author says, ‘He’ (Dr. Thorndike) ‘comes very near to the belief that they are automata pure and simple, though this he does not assert in so many words.’ This, I may be permitted to say, is an absolute misrepresentation. In every associative process discussed in the book I find present as an important element, *impulses*, and impulse I expressly define as ‘the consciousness accompanying,’ etc. (p. 14). Again, I speak everywhere

of the *pleasure* resulting from the attainment of freedom, food, etc., as stamping in the connection between sense-impression and impulse. So, also, I speak everywhere of the sense-impression as the starting-point of the mental association. As a fact, *mental* processes are mentioned throughout the whole discussion. The one place where I frankly offered opinion in addition to fact was where I also attributed *representations* to animals: 'my opinion would be that animals *do* have representations, and that such are the beginning of the rich life of ideas in man' (p. 77). Again, after an attempt to 'describe graphically \* \* \* the *mental* fact we have been studying,' I say (p. 89): "Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind."

On page 264 Professor Mills talks as if I were trying to answer the question as to whether the animal mind was comparable to the human mind, and to answer it in the negative for the sake of exalting the human mind above the realm of natural evolution. The reader of the monograph will remember that one of the results of the study was the attainment of a possible mental evolution of an entirely natural sort. I never tried to answer the question, 'How far does the mentality of a dog or cat equal that of man in general, genus homo,' for such a question seems to me fruitless. It is like asking how far is  $z$  like  $x$ . The mentality of man *in general* is an unknown quantity, has a lot of possible values and so cannot be well used as a measure of anything. Any answer to it will be partially false and partially meaningless. Whether cats infer and compare, whether they imitate as present day adult human beings known to psychologists do, whether they form associations minus impulses of their own, are clear, answerable questions. Such I tried to answer. To say or to prove that the human mind of Europeans of to-day comes by continuous evolution from the animal mind does not make the latter any higher, endow it with a single new function nor alter it one whit. The protozoa are not at all different from what they were before after we call them the ancestors of the vertebrates. And one is free, it seems to me, to find out about questions of descriptive psychology, as well as of morphology, without meddling with questions of classification.

On page 265 Professor Mills rebukes me for considering hunger the strongest stimulus to animals. Of course, I did not so consider it, and I am not aware of anything in the monograph which even looks as if I did.

Again, on this same page he misrepresents me by quoting a sentence



without its context and, indeed, with comments which positively give a wrong notion of the context. The sentence is: 'the question of whether an animal does or does not form a certain association requires for an answer no higher qualification than a pair of eyes.' This sentence, as anyone may see by reading pages 5, 6 and 7 of the monograph, refers to the particular associations involved in learning to escape from boxes. And whether an animal does or does not learn to escape from a box certainly can be observed by anyone with a pair of eyes. And as the text clearly states, it was just because I did not wish to impose on any one my own opinions or even observations, because I wanted to use a method which any one else could employ and gain results which any one else could verify or refute, that I planned experiments which depended, so to speak, on impersonal eyes, eyes in general, for many of their results. I unhesitatingly affirm that so far as the facts of escape or non-escape and the time records (and the sentence concerns nothing else), Professor Mills or any one else would have kept just the same records as I myself did—that his eyes would have seen no more nor less than mine.

On page 267 I am accused of sacrificing particulars about facts for the sake of rhetoric, again on the basis of an entirely misrepresented quotation. On pages 38 and 39 of the monograph I say that henceforth I shall frequently use the word 'animal' or 'animals' when I mean to make statements only about the particular score of animals which were the subjects of my experiments, as "really I claim for my animal psychology only that it is the psychology of just these particular animals." After giving one reason for this verbal usage I add, "my second reason is that I hate to burden the reader with the disgusting rhetoric which would result if I had to insert particularizations and reservations at every step." Professor Mills quotes, omitting the first five words, and giving the impression that I generally omitted details so as to have good paragraphs or something of that sort, whereas the only 'particularizations' to which I objected were such as saying, Cats 1 (8-10 months), 2 (5-7 months), 3 (5-11 months) etc., up to cat 13; Dogs etc., etc., did not do so and so every page or two, when by means of this little note upon verbal usage the reader could on each occasion interpret the word 'animals' to mean "the particular animals which he observed, not necessarily all animals." The rhetorical excellence thus gained requires absolutely no sacrifice of fact of any sort.

If I were sure that Professor Mills would enjoy a bit of jocularly, I should reply to his explanation of the failure of my animals to imitate, by his own failure to imitate Professors James, Ladd, Hall and

Cattell, by saying that it was a good explanation, that they, like him, did not imitate because they could not. His whole discussion of my views on imitation should, in fairness, be accepted only after a careful reading of what the monograph said on that subject. There is room in this reply for only one more comment, on another matter.

To prove that dogs have memory in the sense of the ability to "refer the present situation to a situation of the past and realize that it is the same" (the meaning taken in the monograph), Professor Mills tells us of a dog which stopped at a certain tree, up which he had, months ago, chased a cat, "looked up and behaved otherwise in such a manner as left no doubt in my mind that he remembered the identical tree and detail of the whole performance." I suppose this description of the effect on Professor Mills, beginning with the words 'behaved otherwise,' means that the dog barked at or jumped at the tree, or behaved as he would if the cat were there. It must be confessed that to a hardened disbeliever the argument, "the dog remembered because he behaved so that I know he remembered," seems hardly scientific; but supposing that the description means what we have suggested, it still does not prove that the dog felt a memory of previous incident. At the table this morning I took hold of a cup, raised it to my lips and drank, acted toward the cup just as I did a month ago, but I had absolutely no memory in connection with the act. Indeed, if the dog really remembered the previous chase, he would have good reasons *not* to stop at the tree and act as if a cat were there. Let us suppose that Professor Mills and his dog were both out for cats; that they chased a cat to a tree; that the dog barked, etc., at the foot; and that Professor Mills, running up, shot his gun at the cat. Next month they come along toward the tree. Now, suppose that Professor Mills should run up and shoot his gun as he did the other time. Would we think he remembered his chase of a month before? No! we would think that he had gone daft, or had *forgotten* that the cat was there a month ago. Such an act would be the natural result of a permanent association between the sight of that tree and certain impulses, or of an ill-defined representation; but it would be one of the last things to expect as a result of a memory of the previous occasion.

This reply should close with an apology. Discussions of method and argument over results are likely to be less profitable and much less interesting than new constructive work. This reply was, however, necessary because of Professor Mills' eminence as an observer of animals, and because of the importance of getting at the truth about the

possible disturbing influence of fear and novel surroundings in certain convenient and, if legitimate, illuminating experiments.

[NOTE.—On page 268 Professor Mills has put ‘to the laws of nature’ instead of ‘to the laws of its nature,’ which means something rather different.]

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## NOTES ON AFTER-IMAGES.

### LOCATION OF AFTER-IMAGE.

The following *Experiment 1* was made while I studied at Princeton, January 26, 1895. With an ordinary students' stand-lamp, I closed the left eye, shaded it with the hand, and gazed steadily at the flame until an exceedingly strong image was secured. Then, closing this eye and likewise covering it with the hand, I secured a strong image with the left eye.

Then, with a large piece of cardboard the eyes were shaded from the lamp-light and the after-image of the right eye was projected upon the wall, which was of a light shade. While this image was complementing from green to red, and at just the time the red was well produced, that eye was closed and the image of the left eye was thrown upon the wall, which image was found to be green at the instant that of the right was red. In like manner, when the image of the left eye was complemented into red, and the image of the right eye was at that time found to be green. Opening and closing the eyes alternately, it was found that each eye had its own independent after-image.

*Experiment 2.*—Proceeding as before in securing the after-images opposite in color for the eyes, the left eye was closed and the image of the right eye was projected on the wall. When this after-image had changed to red I projected the after-image of the left eye upon that of the right, that of the left at that instant being green. The combined image appeared green. Upon closing the left eye, or upon shifting its image to the left so as to make two separate images, it was found that the image of the right continued to be red while that of the left was green. The reverse was likewise accomplished. With sufficiently strong images this shifting of images into and away from each other proved an exceedingly interesting and beautiful process.



The above experiments, if taken alone, seem to indicate quite decisively that the after-image pertains to the retina of the eye. Mr. McCurdy, who frequently studied in my room, upon being informed of this experiment, tried it and obtained the same result, and likewise felt satisfied with the evidence of retinal location.

#### AFTER-IMAGE AND TEMPERATURE.

The following describes what was rather an experience than an experiment, since it conducted itself, and that so impressively that I was enabled to chronicle it in detail after going to my room.

It is necessary to explain that, while studying in Chicago, I was accustomed to public speaking each Sunday evening, and finding that a double bath—that is, a hot bath succeeded by a cold one—proved beneficial toward reducing nervous excitement following on the effort of speaking, and conducive to sleep, it was habitually practised.

On an evening in March, 1898, while lying in a bath as hot as I could well endure, my eyes being closed, I noticed a very lively after-image. I presume it had been caused by looking at the gas light in the bath room, although unconsciously. Its peculiar shape and brilliancy attracted my notice so much that I became interested in its life history. Its shape was that of a heart and its color that of the gas flame recently lighted. Besides its peculiar form, another novelty was the trimming of green globules which embroidered the image. While attending this feature I became aware that the image, instead of diminishing in intensity, as becomes the normal after-image, was growing more intense and brilliant. At the time I had become so warm that perspiration stood out on my face and forehead. As I watched, the globular fringe began to shift around to one side—the left side—and to thicken there into a kind of knob. At about the same time another small after-image of exactly the same color and shape as the former image began to form in the right center of the latter. It must be noticed here that the old image persisted in remaining the same color and refused to complement itself. The second image grew rapidly; and now a strange thing took place, namely, the small image moved closer outside to the right of the older and larger, and increased to about the same size. Then both images changed position, rolled over, as it were, upon their sides, with their niches toward each other. The green fringe of globules now concentrated in each image at the niche and the two images began to coalesce. First, the marginal perimeters remained distinct between them, then merged into one separatrix, but eventually disappeared, leaving but one after-

image, with a core, as it were, in the center. At this point the image was much larger than the first image had been, and more intense than any I had ever previously observed. And, strange to say, the color persisted without complementing. That is, in general, for the color had gradually shaded into a beautiful pink, while the center was a sort of apple green. In fact, the appearance of the image at its zenith resembled a large pink candy apple with its green center toward the eye. At this time I was suffering from the heat. Turning on the cold water, the bath began gradually to cool. With the decrease of temperature the size and intensity of the after-image reduced. By the time the bath was reduced in temperature so as to feel decidedly chilly, it had entirely disappeared. The last glimpse I got as it was fast paling—there was an orange colored daub across the left center. When the water was really cold I not only could not get a return of the image, but could get but a very poor after-image by repeatedly gazing at the gas flame.

I judge from this experience that the high temperature of the bath caused a rush of blood to the periphery of the body and so to the end organs of the optic nerves, stimulating the retina, so that feeble impressions were wrought up to remarkable intensity.

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## PSYCHOLOGICAL LITERATURE.

*Truth and Error, or The Science of Intellection.* J. W. POWELL.  
Chicago, The Open Court Publishing Co.; London, Kegan Paul, Trench, Trübner & Co. 1898. Pp. 423.

It is a precious discipline for the schooled laborers in psychology and philosophy that they must submit to learn from minds that obstinately refuse to learn from them or their masters. Mr. Powell's book has the freshness, suggestion, courage and masterful quality that we associate with the thinkers who inaugurate 'periods' and who are unoppressed and unchastened by a long dismal vista of strenuous and largely ineffectual thought behind them. He has striking conceptions, and often admirable expression, but he will not learn his a, b, c's. He has the familiar refrain of condemnation for 'metaphysics' and those who are deceived by words, but his own unguarded reliance on words, joined with an arbitrary definition of them, is not encouraging. For instance, 'metaphysics' itself. "In modern times those who hold that noumena are inexplicable, that is, unknown and unknowable properties, call themselves 'metaphysicians.'" Mill, Spencer, Aristotle, Plato, Newton, Berkeley, Kant and Hegel, are freely refuted; but the author's insight into other men's thoughts and methods falls considerably below his confidence. The strictness and prudence of his own reasoning—his philosopher's conscience about assumption and assertion—might be pointedly illustrated.

'The war of philosophy,' we are told, has been 'between Idealists and Materialists,' according, of course, to Mr. Powell's definitions. "The philosophy here presented is neither Idealism nor Materialism; I would fain call it the philosophy of science." It is realistic in the modern, and, one is tempted to add, in the scholastic sense. He takes his first principles avowedly from empirical science and seems to be untroubled by the scruples of epistemology. "I shall propound the hypothesis that consciousness inheres in the ultimate particle, and attempt to show that it [*i. e.*, the hypothesis] harmonizes the principles of psychology." "An ultimate particle, and hence every body, has five essentials or concomitants, these terms being practically synonymous. \* \* \* The essentials of the particle are unity, extension, speed, per-



sistence and consciousness, which are absolute. The relations that arise from them, in order, are multitude, position, path, change and choice, which give rise to number, extension, motion, time and judgment, as properties that can be measured. It has been pointed out that particles are incorporated in bodies through affinity as choice, and by this incorporation the quantitative properties become classific properties which, in order, are class, form, force, causation and conception." Unfortunately one must content oneself with these quotations. This philosophy of science is interesting; and if somewhat remote from both modern philosophy and science, it remains true that the book abounds in suggestive statement and clever expression, and furnishes striking illustrative passages to the student of that attractive but undeveloped subject, the psychology of philosophic speculation.

D. S. MILLER.

PHILADELPHIA.

*Human Immortality.* WILLIAM JAMES. Boston and New York, Houghton, Mifflin & Co. 1898. Pp. 1 + 170.

This little book of Professor James' is the Ingersoll Lecture for 1898. The author seeks to answer two objections of modern culture to immortality. The first of these is the proposition of Physiological Psychology that thought is a function of the brain. This general idea has been carried into detail by the hospitals and laboratories which have located special forms of thought in special brain areas. Professor James asks us to accept this result for the sake of argument, and asks whether it compels us to surrender belief in immortality. Most persons imbued with the 'Puritanism of science,' he tells us, would answer in the affirmative. But this conclusion is not logically coercive, because the physiologist assumes that the only kind of 'functional dependence' is production, and supposes that the brain produces consciousness. But this overlooks 'permissive function' and 'transmissive function,' with which we are familiar even in the physical world. Professor James' thesis is that when we say that thought is a function of the brain, we are entitled to think of 'permissive' and 'transmissive' function. He considers the latter, which he illustrates physically by the keys of an organ, which transmit the air from the air chest through the pipes into the world, in certain special forms. Suppose that the whole universe of material things is only a 'surface veil of phenomena' hiding the reality behind, or a dome refracting the 'white light of eternity.' And suppose this dome, usually opaque to the eternal light, could at certain places grow less so, admitting to this

world so many restricted rays. Just so our brains can be conceived as thin places in the dome through which the genuine reality, the life of souls, breaks through into this world in restricted forms of finite consciousness, which would only cease in these special forms when the various brains ceased to exist. Thus our conscious life would depend on our brains, and yet an immortal life beyond the veil be possible. Professor James says in one of the notes that he takes the dualistic standpoint of natural science, because this objection arises on this plane. From this standpoint, he says, if we reject the notion that the brain produces consciousness, we have no other alternative but to believe that consciousness preëxists and is transmitted into this world of phenomena by the brains which give it its finite forms. He then shows that the idea of production is quite as metaphysical as the idea of transmission, and finally gives certain positive advantages of the transmission theory.

Several questions suggest themselves. There is space here only for two. The first has reference to the idea of 'transmission.' If it be admitted that thought is a function of the brain, and also that 'transmission' can be called a function, then Professor James might exclude all other theories than production and transmission without any refutation, on the ground that they do not make thought a function of the brain. But if the physiologist can reply that this idea of transmission of preëxisting consciousness by the brain does not make the former a function of the latter, it would seem that we have given up the proposition we were to have accepted, and the transmission theory would have to hold its own against other theories which hold an absolute beginning of the finite consciousness, such, for example, as those which say that it is created either absolutely or through the generation of parents. And if the brain passively transmits a preëxisting consciousness as a 'thin place in the dome' lets in light, it would seem as though it could not be said that thought is a function of the brain, if function is to have any intelligible sense. If this theory does not have the right any more than others to the support of the physiologist's proposition, it must take its place in the arena with the others.

A second question is suggested by the author himself: How does this theory help us to realize our finite and individual immortality? Our finiteness seems to be a part of the warp and woof of our personality; and if when the brain, the organ of this finiteness, vanishes, our spirits revert to their original source, what is to become of our personal immortality? Professor James admits that these are vital questions, but declines to enter into the discussion of what he calls

'these higher or more transcendental matters.' He merely says by the way, that if, as the philosophers say, 'all determination is negation,' it might prove that the loss of these particular determinations is not a matter for regret, and that they are not worth keeping. But what if all determination is not negation? What if those elements of our finite personalities are positive and worth keeping, and their loss a 'matter for regret?' It would then seem that this theory would not help us greatly in the question of immortality. If it is to be a valid argument for immortality it would first have to prove that 'all determination is negation.' Nor does it relieve the matter that Professor James says in a note at the end of the book that it is not necessary to identify the preëxisting consciousness, which this theory presupposes, with the Absolute of transcendental idealism. Even though the theory only requires that consciousness preëxists 'in vaster entities' than our finite spirits, we lose ourselves just as much in the bosom of these 'vaster entities' as we would in that of the Absolute.

The second objection to immortality, for which there is little space here left, has reference to the incredible number of beings which must be immortal if we hold fast to our belief in immortality. Professor James shows that this is a fallacy resulting from our failure to realize the inner significance of these alien lives, which is as great for them as that of our own for us. Moreover, we cannot say that God has no need for these lives because we ourselves have not. If God suffers us, surely we can suffer one another.

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PRINCETON.

*Essays on the Bases of the Mystic Knowledge.* E. RECEJAC.  
Translated by SARA CARR UPTON. New York, Charles Scribner's Sons. 1899.

This exceedingly interesting and suggestive book, which has been well rendered into English, may be characterized as a search for the Absolute through the mystic intuition. 'Reason is in possession of too much light,' the author says in his introduction, 'to be able to remain quite at ease in the region of clear ideas, but not enough to know first principles of actual knowledge. In this penumbra who can trace the exact limit of perceptions and say where the true disappears in the probable, where the probable vanishes in illusion?' The author holds the common ground of mysticism with reference to the inability of pure reason. It is impossible to grasp the highest truth by a rational act, or to reduce it to the form of definite conceptions. The Absolute can only be



grasped by a species of inspiration, and the highest truth transcends ideas and is only expressible in symbols. The author is at the same time a positivist and an agnostic, and yet denies that science is the only organ of knowledge. The 'Heart,' by which is meant a synthesis of freedom or moral spontaneity and imagination acting under the regulative categories of duty, constitutes an ultra-rational and ultra-scientific organ of truth. The relation of mysticism to science, the author argues, is purely negative. Mysticism, when it understands itself, does not encroach on the territory of scientific knowledge. It admits and leaves it to itself, and claims the power of discovering, through its own organ, truths that are inaccessible to science. The mystical object is not ontologically transcendent. The Absolute is nowhere but in consciousness. But it is to be reached only by a consciousness raised to a high degree of intensity, which, by an act of 'excess' or 'disinterestedness,' or 'self-alienation,' transcends its ordinary plane of intellection and moral egotism, and in this act of 'transcendence' becomes, for the time, identical with the Absolute and attains to supersensuous, absolute truth. This apprehension is not intellectual, however, and cannot be represented in terms of ordinary knowledge. It can only be expressed in symbols, and these must also be the creation of the excited consciousness in which the intuition takes place. The mystical symbol cannot, therefore, possess universal value, like the principles of rational knowledge. How, then, is mysticism to be guarded against enthusiasm and subjective caprice? A negative criterion arises out of the relation of mysticism to science. Mysticism must not enter the preserves of science. When it essays to occupy fields open to science it becomes false mysticism and is to be condemned. But the most important criterion is positive. The 'Heart' must be impelled by the motives of pure morality, and the result of its mystical act must submit to be judged by the laws of duty. It must be tributary to the moral good. The author is here a disciple of Kant, as he is Kant's disciple in accepting as final his condemnation of metaphysics. This symbolic knowledge, though not amenable to the tests of that which makes the claim of universality, is not without its own appropriate canons of self-criticism.

The discussion of the book is divided into three parts. In Part I., entitled "The Absolute," the problem is how the Absolute is to be apprehended; the first chapter being devoted to various defective mental attitudes toward the Absolute, while the second treats of the mystic consciousness as the only organ for the real apprehension of absolute truth. Part II., entitled "Symbols," treats of the mode of ap-

prehending and expressing the mystical intuition, while Part III., under the title of "The Heart," deals with the moral and religious aspects of mysticism. It would be impossible, in the limits which must be observed in this notice, to follow the author into any of the details of his discussion. One is impressed with the general sanity of the discussion and the fine irenic temper which pervades it, as well as with the author's intelligent appreciation of the results of modern investigation. The book embodies an attempt to bring a very recalcitrant theme within the sphere of critical treatment. The phenomena of mysticism are treated mainly from the psychologist's point of view, and it is from this standpoint chiefly, therefore, that the attempt is to be judged. The psychological interest and value of the author's work seem to me to be unquestionable, though the extent to which mystical phenomena are open to psychological treatment is a question on which difference of opinion is likely to prevail. The proposal to substitute 'Mystic Positivism' for rational theology or metaphysical idealism touches some of the great issues of the ages. Whilst free to admit my own scepticism as to the adequacy of the substitute and my persistent adherence to a larger faith in reason, I am yet of the opinion that the author has performed an important service to philosophy. To one who is foolish enough in these degenerate days to be troubled about the ultimate problems of life and destiny the book is refreshing as well as illuminating. It proves that the search for the Absolute has not yet become antiquated, and it leads one to think that philosophy may possibly have something important to learn from the mystics.

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*Psychologie der Veränderungsauffassung.* L. WILLIAM STERN.  
Mit 15 Figuren im Text. Breslau, Preuss und Jünger. 1898.  
Pp. viii + 264.

Ever since the acceptance of the dictum *Semper idem sentire ac non sentire ad idem reverunt* (Hobbes), psychologists have been searching the multiple variations which crowd in upon consciousness; and, of late, these empirical facts have had a semblance of scientific treatment in the so-called 'law of variety' (Hamilton) or 'law of relativity' (Wundt). Change as objective sequence, and change as having meaning, have given no end of trouble to clear thinking, whether in metaphysics or in science. The work under review presents itself with the avowed purpose of bringing together the facts and meaning of change from the psychological point of view.

Noting the historical and metaphysical importance of the concept of change and its congeners, the author passes to the problem which change offers to psychology, formulating it thus: "To exhibit all the forms which the apprehension [*Auffassung*] of change can assume in universal thinking (including the interrelations of them one to another) and to describe the ideational contents of these various forms of apprehension" (p. 5). Change has many different aspects: as, quantitatively, the increase or decrease of substance, the heightening or lessening the intensity of an experience, the enlargement or diminution of its extensity, the improvement or deterioration in value, and variation in rapidity of change; secondly, the type or quality of change, as transition, transformation, interchange, beginning, progression; thirdly, the local direction of change, as movement, transition, process, etc. In its highest orders change appears in the form of development, history and mathematical functions. To this descriptive problem there is attached another quite as important, which represents the last stage of differentiation that psychology reaches, viz.: the causal investigation of the nature, origin, amount and law of the apprehension of change (p. 11).

This double statement of the problem is inadequate without a definition of *Auffassung*, the second member of the title. The author finds difficulty in defining precisely what is to be included in this term. His is distinctly not the problem of the 'perception' of changes, nor of the effects of objectively changing stimuli upon the senses; but rather of the manner how a certain form of our ideation and thinking is constituted (p. 12 f.). *Auffassung* implies the complex, discriminative, psychical activity which meets all varieties of stimulation and issues in all forms of judgment (pp. 120 f., 138). The monograph is thus an extended study of sensation, perception and mental activity as complexly involving change, its fundamental thought being that the active functioning of consciousness is the only hypothesis which offers any aid for the solution of the technical difficulties involved in attempts at harmonizing our varying experiences of changes (p. 255). Changing, rather than constant, impressions alone are the conditions for awakening this functioning (p. 140). In every paragraph, almost, there is a steady mindfulness of this two-fold problem.

The monograph is divided into two parts. Part I. (pp. 19-73) treats of 'the origin,' and Part II. (pp. 78-256) of 'the fineness of the apprehension of change.' The former is purely qualitative; the latter, more quantitative in its treatment. In Chapter I. there is given a discussion of the sources of changes as found in Perception, involving the



psychological, rather than the punctual or mathematical, present in consciousness. Fixed and gradually changing stimulations, and the changes which the subject may induce in its own conscious stream and in the members of its body, serve as the varied starting-points whence change is apprehended. In every act of perception which is essentially extended in time there is an intuitive apprehension of change. Otherwise Reproduction and Comparison (Chapter II.) could not take part in bringing about those ideas which have change as their characteristic category. Feelings of recognition accompany these purely intellectual processes (p. 54). The comparison of varying changes, whether minute or extensive, is given a very high function, its greatest importance being to promote awareness of specific stadia in any series of changes which involves more than two members; as, for example, in the growth of a plant we are able to detect 'phases' now and then. This process is essentially involved in consciousness's recognition of itself and its past. (In this process, also, S. finds 'the most important psychological root' of the idea of *Ding an sich.*, p. 67.) These 'phases' are resting-points, boundary lines, which are necessary as soon as we attempt to *fix* the changes by thought, word or number (p. 72).

The analytical distinctions not only aid the later examination of the measurement of change, but the quantitative analysis tends to complete and reënforce them. Part II. gathers, in critical fashion, the results in this 'relatively young field of experimental psychology.' Chapter III. describes the Technique concerning the few special pieces of apparatus which have been used in measuring stimulus changes in brightness and color, in pitch, intensity and direction of tones, in pressure, thermic and other changes due to chemical and mechanical stimulation. The mechanical devices used in the studies by Preyer, Hall and Motora, von Frey, Seashore, Stratton, and by S. in his earlier studies on brightness, movement and tones, are noted in this connection. The latter part of this chapter discusses the two groups of methods of getting at the fineness of discrimination of changes, viz. : judgment and reaction. In this matter of method S. finds a field of questions that is new and unexplored (pp. 102 f.), and offers suggestions for reducing varying affirmative and negative judgments to quantitative values, which he regards as the more desirable results in experimental tests (pp. 97 f., 91).

Chapter IV., entitled *Psychical Excitability for changes and its Laws*, is by far the most important part of the monograph, both as to bulk (pp. 119-256) and contents. Its first section is occupied with a

search for a technical term which shall be generic to sensibility and discriminability. This term is *Erregbarkeit* (pp. 119, 124), which means the collective reaction of the soul upon external stimulation. The development of this position brings S. to a critical rejection of Preyer's view that sensation is merely a function of stimulus changes (pp. 144, 157 f.). The remainder of the chapter endeavors, by a careful examination of the conditions of psychical response to external objects (§§ 9-11), and later by a review of the modifications of these conditions in the special senses as revealed by the somewhat conflicting results of the experimental studies mentioned above (§§ 12-14), to reach a special law generalizing psychical excitability to changes. (It is noteworthy that S. does not find Weber's Law to have any great bearing upon his problem, p. 131, note.) All senses, except that of temperature, readily yield to the propositions to be mentioned later. The perception of heat and cold and their changes seem to defy the alleged uniformity of conditions for the apprehension of changes, leading to a suggested hypothesis of heat and cold being relations only. Experimental data in vision, hearing, pressure and the modifications of constant and transition-sensations due to fatigue, surprise, expectation, etc., are given a careful examination in the interests of the special formula sought to generalize the facts of the variations in the rapidity and other features with which stimulus changes affect the perceptibility of those changes. By his earlier investigations S. has entitled himself to this critical comparison of the data in this special field (PSYCH. REV., II.: 313 f., V.: 98 f.). This composite review is suggestive and appreciative, even when critical of the work of others—which is by no means uncommon.

The '*Hauptgesetz*', mentioned above as the objective point of the treatment of the quantitative values, is slowly reached and presented in sections, pertinent to the specific conditions and the type of sensation and reaction. In the serial order its parts read thus: 1. "It is not the absolute value of the excitations always present in sensory nerves, and radiating from there to the motor fields, to which motor nerves respond with a reaction; but it is rather the change in this value from moment to moment" (p. 145). 2. "It is not the absolute value of the stimulus affecting motor nerves which produces a movement, but it is rather the change in this value from moment to moment" (p. 145). 3. (Physiologically) "A nerve stimulation may become a specific cause for the performance of physical and psychical activity only when the stimulation is a changing one." 4. (Or, psychologically) "A



sensation may become a specific cause for the performance of physical or psychical activity only when it is apprehended in the process of changing" (p. 158). 5. "The incitation to the physical or psychical reaction varies directly with the rapidity of the change in the sensation" (p. 211). In a series of tone discriminations, varying from  $\frac{1}{4}$  to 2 vibrations, lasting 2, 4, 6 and 8 seconds, S. found that the most favorable time for detecting the amount of increase and direction of stimulation was 6 seconds' duration (pp. 189-195). On this basis he offers the law of the most favorable time for apprehending changes. 6. "If a changing stimulus is persistently observed, certain favorable stadia will be found within the observation time in which the capability of perception (the tendency to complete a judgment—or motor—reaction) is especially strong. Since, within such a favorable time changes of varying rapidity can be perceived, the slower changes which, up to that point of time have acquired only a lesser extent, are relatively more favorably placed" (p. 211). 7. (In addition to 2) "The greater the rapidity of change in the stimulus the greater is the incitation to motor activity" (p. 213). 3 and 4 express the law of change in its best forms. The actual experimental deviations from the law, noticed at length by S. (p. 224f), must be omitted.

The law is less suggestive in its formulæ than in the discussions which point the way to it. Its formulation is rather defective in being so scattered. It remains isolated, finally, and is not exactly brought into harmony with his problem as defined in terms of apprehension. And, furthermore, the attempts at emulation are rather too pronounced. On the other hand, in its review of the few experimental studies, the monograph can well serve as a hand-book. The detailed analysis of many facts and relations brings them up to the point where only special investigation can carry them forward. This is one of the most valuable features of this very circumspect treatment of the problems connected with the apprehension of change.

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*Psychophysiologische Erkenntnistheorie.* THEODOR ZIEHEN. Jena, Gustav Fischer. 1898. Pp. 105.

In the rush to epistemology the serious student may well question whether we are keeping ourselves aloof from a neo-scholasticism which threatens scientific method, on the one hand, and disables the efforts of thought by an apparent show of consistency on the other. Noetical theories, once unknown and unsought, are now so common that they



even serve the purpose of setting up standards for giving advice. Whether this highest court of Appeal in the kingdom of mind is not deserving of far more respect than is, by popular consent, manifest, can scarce be a matter of question. Consistency, however, is the chief emblem of this bar, and the essay under review presents itself with titular claims to a frank hearing. It reposes upon the earlier efforts of its writer in the field of physiological psychology, and thus comes with the promise and potency of unsuspected extensions in the domain of theory of knowledge.

In all exploration, results are in primary demand; and the author has spared the reviewer much by bringing in one section (§ 22, pp. 100-103) a 'dogmatic résumé' of his inquiries. Though opposed to the very spirit of epistemology (which the author recognizes), he ventures to give a succinct exhibition of the conclusions to which he has been led in his analyses. Freely rendered, this '*Lexikon*,' *so zu sagen*, of his theory of knowledge runs about thus:

"Sensations [*Empfindungen*] and ideas [*Vorstellungen*] are given to us." Both are summed up in the terms psychical processes, or the psychical. Non-psychical is a meaningless term. Things, my *ego*, *alter egoes* are ideas only.

On the basis of epistemological analysis each sensation is made up of two components, the residual factor, or the reduced sensation, and the  $\nu$ -component. [The ' $\nu$ ' factor is the primary experience derived from the activity of the sense-organs, as tactile, visual, etc., p. 22]. The former factors have reciprocal relations, which can be expressed by universal laws. The totality of these laws is designated as the 'causal formula.' [This is the summary of the coördination of tactile and optical series mutually dependent—*e. g.*, in seeing my hand movements and pen movements spatially and temporally connected. This is the sequence of value to the natural sciences, p. 25.]

A certain group of sensations is designated as the group of  $\nu$ -sensations. The residual factors of these sensations work first reciprocally with the residual factors of the other sensations which can be expressed by the causal formula; but, secondly, they react independently upon these residual factors when they have undergone a change through the residual factors of another sensation.

These reactions are not arranged according to space and time. They cannot be expressed by the laws of the causal formula, but rather, in their entirety, by other laws (uniform fusions). The totality of these lawful fusions is designated by the term parallel-formula [*i. e.*, changes in sensation-complexes that are simultaneous and not successive, pp. 25-6].

The residual factor of a sensation, which causally effects in a certain degree and certain quality the constituent part of the  $\nu$ -sensation is altered by the parallel-reaction of the latter. The process of transition is designated by the term  $\nu$ -change, or individualization, the change itself as the  $\nu$ -component.

Epistemology is trying, with the help of natural science, including psychology, to eliminate these  $\nu$ -components, and to present the residual factor itself [*i. e.*, the 'object']. The idea of this residual factor is the resultant idea, or the reduced sensation. (Hence) the method of theory of knowledge is called 'reduction' [*i. e.*, thought must not turn to 'things' as external, but continue a consistent analysis of sensations, until it reaches that sensational *quale* which cannot be eliminated, p. 31 *f.* This residue is the nervous system, at times fibers, and again cortical centers, pp. 35, 59, 65]. The residual factors are only to be ideated.

The  $\nu$ -sensations are arranged in complexes, which are commonly called sensory nervous system.

All sensations are made up of  $\nu$ -components of a single complex of  $\nu$ -sensations; so far forth they are individually psychical; the residual factors can also be ideated only as psychical, but as universally-psychical.

Since the idea of the individual *ego* is itself an idea resulting from the process of reduction, the idea of a universal-psychical is neither contradictory nor meaningless.

Sensations, in so far as they all possess  $\nu$ -components, are also called object-sensations or stimulus-sensations.

With the omission of the  $\nu$ -components the object-sensation disappears. Its residual factor must be ideated as abiding.

Every reduction factor transforms itself into so many object-sensations—*i. e.*, is individualized just so often as it affects the  $\nu$ -sensation-complexes and experiences their reactions.

All sensations are positively only in space, at the place of the residual factor. Spatial and temporal series belong primarily to the reduced sensations (*i. e.*, the residual factor). It is influenced only secondarily by individualization.

In like manner, the quality and intensity of object-sensations are determined by the reduced sensations, and only secondarily by the  $\nu$ -components. Theory of knowledge, in so far as it demands universal ideas of reduction, is trying, with the help of natural science and psychology, to subsume the qualities and intensities under a single reduction-idea. As such a universal the idea of energy is to-day com-

ing under consideration. The idea of mass, inasmuch as it is to indicate more than a numerical factor, is contradictory or meaningless [*i. e.*, 'mass' does not carry one over into realism].

An affective tone, as a feature independent of the other characteristics, does not belong to the reduced sensation.

The difference between ideas and sensations consists in sensuous vivacity.

\* \* \* \* \*

Object-sensations always require, for their individualization, conjunction with ideas.

Ideas [like sensations] are simple or complex, individual or universal. [Ideas are only memory-pictures of sensations, p. 37.]

Ideas of Relation form a special group. They are just as dependent upon the  $\nu$ -sensations as are the other ideas. Like all other ideas, they are developed only from sensations.

Among these ideas of relation, those which have special epistemological importance are the six categorical ideas [not in the Kantian or Hegelian sense of the term 'category'] of likeness, similarity and difference, persistency, change and interchange. [*I. e.*, upon the sole condition of like sensations, simultaneous and in sequence, etc., the child builds up these rational ideas, by extracting those elements present in the varying sensation-complexes, pp. 7-15.] The relational idea of causality is based upon the relational idea of change. The reduction ideas of epistemology are the most universal ideas of sensations and sensation-relations.

In the formation of epistemological reduction-ideas the regulative principle is so to plan the reductions that a general similarity appears in the place of single similarities of the object-sensations and their changes. Our reduction-ideas are subject to a progressive development and selection, since object-sensations are never given us in their totality.

Ideas of ideas, hence ideas of reduction-ideas, do not exist.

Those reductions which unite with the epistemological basis are, in so far as the latter does not change, not further in need of reduction.

The causal changes within the  $\nu$ -complexes are often continued through the reduction-factors of sensations which are closely connected, spatially, with these  $\nu$ -complexes. They are designated as  $\mu$ -sensations, and correspond to the motor system of our bodies. These  $\mu$ -complexes affect, in their turn, according to the causal formula, the reduction-factors of ordinary object-sensations. These influences are called 'actions;' and they complete the circle of causal changes."



“Can there any good thing come out of Nazareth?” is a question pointing to an active field in philosophical inquiries. Ziehen replies, as the newly adopted apostle of old, ‘come and see,’ while he attempts to draw noetical values out of Nazarene sensations, sensation-complexes and sequent images. If there is any virtue in the doctrine of sensation, as it has of late decades crept into science and philosophy, then this essay is the most virtuous of all recent productions which aspire to truth and consistency. The specific problem of epistemology is presented as the merely *analytical* ‘reduction’ of the content of experience to certain forms, validated for the means and ends of empirical sciences, especially of empirical psychology. ‘Reduction’ may mean transformation, fusion, synthesis. In this writing it is presented as the chief and, in truth, only means of noetical achievement (§ 8 pp. 31–35). It purports to be such an analysis of sensations and the elimination of those accidental factors which leads the naïve thinker to affirm ‘things,’ and the scientist to end with extraspsychical forces. This gives rise to antinomies, which it is the special problem of noëtics to remove, and, in this instance, succeeds to the satisfaction of one person in rendering all factors and processes psychical. To this extent the author is almost mortgaged to a prejudice, while the reviewer frankly confesses to the privileges in the very opposite direction—namely, that an objective, impersonal analysis of psychical contents of the lowest, or, perhaps better, initial, order is not paramount to the demands which can and must be made upon any serious attempt to explicate the nature of what we men call ‘knowledge.’

That the analysis here spread forth in an exceedingly abstract, schematic, algebraic fashion is astutely regarded as adequate, may be seen from the following passages, which well illustrate the flavor of the essay, if not its detailed method of analysis:

“A special ‘function of judging’ does not exist.” For centuries, psychology, logic, and theory of knowledge have fruitlessly attempted to find in the features of judgment what is here for the first time clearly provided for in those preliminary fusional abstractions which engage simple ideas. “There is still less occasion for accepting other ‘faculties,’ beyond judgment \* \* \* such as reason, the source of syllogisms or intuitions, etc. Exactly at the place where philosophy has so often ventured the leap from epistemology into metaphysics, is the Calvary of the many ‘higher functions of soul’; here lie the *λόγος* and *νοῦς* and *φρόνησις* and *μανία* and *πίστις* of Greek philosophy \* \* \* and the reasons and pure ego’s and apperceptions of

modern philosophy. It is generally supposed that a connection with the extra-psychical must be sought for in the highest psychical faculties. This extra-psychical is a senseless term, and the highest psychical is already included in the formation of ideas [as detailed by the elaborated schemata]. Right here is the principal divergence of this theory of knowledge from the pathway of the earlier theories." The scope and duty of epistemology are thus greatly modified and simplified. We are no longer concerned "with finding the criteria of true judgment, of certainty, or whatever one may term it; but only with feeling our sensations and ideating our ideas with others, and with forming new combined general and relation-ideas, and among these specially reduction-ideas which correspond to the sensations. The only criterion is the agreement with sensations, the fulfillment of the expectations which become joined to the reduction-ideas" (pp. 85-87).

The somewhat more readily assimilated results of this 'new' theory of knowledge, based upon and strictly held down to the nervous system and its initial processes in consciousness, are neatly presented in the closing section (pp. 103-105) which anticipates (rightly) and attempts to set at rest 'the almost instinctive objections' which are put to his analyses. 1. If it is supposed that the 'residual factor' of this theory is identical with 'matter,' in the ordinary sense of the term, it is replied that 'matter' is a meaningless, metaphysical dogma, and, strictly speaking, the 'residual factor' is an idea which we employ in order to reach universal laws relating our sensations and ideas. The only aspect common to 'matter' and the 'residual factor' is 'change according to universal laws.' 2. If it is presumed that one result of this reduction of things and ego's to a world of purely psychical process is to undermine all laws from that of gravitation to that of electro-magnetic light, the objector is told that their validity remains unchanged. Their labels only are changed, and our manner of speech is altered so as to avoid all contradictory and meaningless terms. 3. If the critic fancies that inroads are thus made upon the principle of psycho-physical parallelism, he is reminded that 'the psychical series alone is given,' and is the only view which avoids the specious wit involved in the affirmation of two unlike but equally persistent series (Cf. his *Introd. to Phys. Psych.*, p. 301f. (Eng. tr.)). 4. Almost overcome, the critic finally gasps for the reinstatement of 'Metaphysics, the *a priori*, the forms of Intuition, the Categories!' He is left to get his assurance in the reply that "no room remains for them. We must limit ourselves to this procedure, to gather, compare and then reduce sensations scientifically in order to attain the

universal ideas of their relations. This labor is divided between the descriptive and mathematical natural sciences, psychology and theory of knowledge. Metaphysics, just as religion, has been only the historical precursor of these sciences. It would be better to relegate metaphysics and its younger sister, metapsychic, among the fine arts." 5. 'And these circumlocutory designations'—must we introduce them into daily speech, instead of the simple terms of our mother-tongue?—*e. g.*, using 'the residual factor of tree-sensation' instead of 'tree'? No. The exposition of epistemology is sesquipedalian for the very prosaic purpose 'of keeping removed those so often falsely added ideas' of realities! Thus the objector is bade to rest agnostically and nihilistically on the quiet bed of positivism, which simplifies ends and means without measure.

The spirit of this essay is extremely serious in its efforts to reduce objects by a bare-handed treatment of sensations, which are now identified with events in nerve fibers and ganglionic cells, and now with events in consciousness. There is an avowed attempt to make epistemology grow out of the soil of empirical psychology, as understood by the author in his *Leitfaden* of some nine years ago, *bien entendu*. What are the exact relations between the two branches of thought is not readily ascertained. Throughout the entire exposition in detail there is no advance beyond what *ought* to be treated under psychology as it actually is understood by most contributors to this field. Definite statements (pp. 4, 11, 58, 61, 65, 74, 85) are not steadfastly explicit upon this point. Psychology enumerates, analyzes and exhibits the development of our complex ideas, while theory of knowledge selects this or that idea, and presents the development of that which is significant for its purpose, only in so far as empirical psychology has not solved the problem. The basis of that selection, as personal, or logical, or objective, is not made plain. Again (p. 75), in tracing the formation of ideas of relation (which are not to be identified with the concepts employed in logical theory) the descent is made to the idea of 'sameness' as the given datum. The 'comparison' necessary, upon the repetition of similar sensations, is a gratuitous assumption; for it is frankly admitted that mere description is all that can be undertaken in this region of the inexplicable. On the whole, then, epistemology for the author means going back, here and there, of his specific psychological conclusions and endeavoring to make them intelligible by a further process of 'reduction' instituted in securing them. In this sense the essay is radically defective in not establishing, in a more clear-cut fashion, those differences in the two disciplines of which he gives promise in the beginning.



The tone of the work is not the most generous. It reveals an utter complacency in its Berkeleian idealism upon a sensory foundation (pp. 5, 59). The ingenuous, at times, and severely schematic treatment sweeps aside, in almost ruthless fashion, 'the insights' of other thinkers and 'the demands' of the problem of thought and reality as perceived by them. In the light of its initial claims perhaps this procedure is commendable. One feature of this analysis of knowledge is to simplify to the grade of algebraic imagination the erstwhile serious tasks of philosophical reflection. Thus it becomes an exceedingly pertinent question to ask how this literalistic chart would fare when confronting actual knowledge. Would one recognize and identify his cognitions on the basis of the analytic and explanatory clues offered in this essay? Indeed, there is lacking that admiration for the fact of knowledge, even on a neuro-sensory basis, which every analyst *ought* to feel. Another interesting feature in this essay is the attempt to square its results with those of Kant's theory of knowledge, implying the general validity of the Kantian position as an abstract expression of truth, which finds proper treatment only in the concrete (?) elaboration of this theory of knowledge. (Cf. pp. 50 +, 53-57, 72-86.) In this fashion there are repeated claims as to the exceeding advantage of this exposition over that of others.

The central question, perhaps, incited by this attempt at a theory of knowledge, which does not advance much beyond a detailed analysis of sensation-complexes (tactile, optical, and motor coming in for almost exclusive attention) is this: Can an historical analysis of certain of our residual experiences satisfy even a scientific study of what we find men calling 'knowledge'? The *Zielstrebigkeit* characteristic of every cognitive construction of an 'object' is a psychological exhibit which the epistemologist is compelled to recognize. (Cf. Baldwin, *Ment. Develop., Soc. and Eth. Int.*, pp. 249 f., 377.) It often is not the point *from* which, but the point *to* which, cognition tends that is the essential feature. This is more than primary motor-reactions (Ziehen., p. 875f), and must be adequately recognized. Other constructive tendencies might be pointed out which all cognition exhibits, but of these the essay takes no notice. The benefits of the task of this essay, then, is twofold: one, in indicating the fact that on *some* assumptions a theory of knowledge leads itself into a blind-alley, cutting off further philosophical progress; the other, a benefit in awakening our thankfulness for being shown the limitations imposed by its methods and content.

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*John Stuart Mill. Correspondance inédite avec Gustave D'Eichthal.* Avant-propos et traduction par EUGÈNE D'EICHTAL. Paris, Felix Alcan. 1898.

The correspondence between Mill and D'Eichthal is in these pages given to the public for the first time in a complete form. Many of the letters had been published previously in the *Cosmopolis*; additional ones, however, have found a place in the volume, together with two letters of Eyton Tooke to D'Eichthal. The friendship of Mill and D'Eichthal, which continued through a period of some forty years or more, presents many features of a most interesting nature, as disclosed in this correspondence. To the student of psychology an opportunity is afforded of noting the effect of an emotional temperament, as that of D'Eichthal, upon a coldly intellectual nature, as that of Mill, and, also, of observing the marked contrast between the French and English traits of mind. In these letters Mill exposes to a searching criticism the doctrines of Saint Simon as expounded by D'Eichthal and his friends; there is, however, a growing appreciation of the motives and purposes of the Saint Simonian school, evidently induced by the disinterested labors and self-sacrificing zeal of its members. While criticising their methods, Mill had only words of praise for the high humanitarian ideals of this school.

The strain of deep sentiment, which was a characteristic feature of Mill's nature, and yet hidden from the view of the world, is revealed in the letters which he wrote at the time of Tooke's tragic death, as also upon the occasion of the death of D'Eichthal's father. The correspondence, indeed, serves as a valuable appendix to the *Autobiography*, inasmuch as it throws additional light upon the inner life of the great logician, disclosing in that many-sided nature the elements which prove his love of humanity as well as his love of truth.

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#### GENERAL.

*Social Automatism and the Imitation Theory.* B. BOSANQUET. Mind, No. 30, N. S., April, 1899, p. 167.

The writer aims to point out, in this way-side preface to a forthcoming book, a fundamental error in the imitation theory of sociological psychology as an attempt to reduce to principle the behavior of individuals in a group. Secondary automatism suggests an analogy which throws light on political philosophy. Social life is necessarily

and increasingly constituted by adjustments which have become automatic, and are thus put beyond the range of discussion. In the resulting economy of attention the social mind is set free for new ideas. The routine of civic life, the use by the state of coercion upon the individual, and the function of punishment to awaken attention are explained in terms of this automatism. The biological principle of 'short cuts' is given application in tracing the transformations of stimuli and reacting apparatus in the world of volition. In society phenomena of identity and phenomena of difference are at once of prime importance, as is evidenced by the principles of imitation and invention, active under the forms of habit and accommodation. B. finds that repetition and similarity are only superficial characteristics of the true operative nature of social unity. No differentiation can be got out of the tendency to reproduce a copy *per se*. To introduce 'invention,' as explanatory leaves an awkward dualism. Baldwin's analysis of mental development is regarded rather as failing in its resolute repudiation of this dualism. The root of this, and other similar failures, is traced to a fallacy introduced by the influences of the atomic doctrine of association, or the repetition of similar units. Baldwin, in attempting to remould the theory, strains the idea of imitation by extending it to cover volition—the passing of an idea into fact, instead of limiting the process to mere reproduction of a copy. Nothing of serious importance happens by genuine imitation. All the business of society goes on by differentiated reactions. Every man in society is what he is through a law or scheme which assigns him an individual position, differing from all others, and identified with them precisely through these differences, by which alone he can coöperate with them. The error in question springs from working with similarity instead of identity (of factors and processes). Directly we introduce identity, difference falls into its place as an inherent aspect of the principle. Every action, without any exception, is, in principle, a difference within an identity. Relative Suggestion is a more adequate view of identity than Associationism, and B. finds in Baldwin's later writings a tendency toward the former.

EDWARD FRANKLIN BUCHNER.

*The Nature of Judgment.* G. E. MOORE. *Mind*, No. 30, N. S., April, 1899, p. 176.

This article suggests a theory of perception and knowledge which has avowedly much in common with Kant, differing chiefly in substituting for sensations, as the date of knowledge, concepts; and in re-



fusing to regard the relations in which they stand as, in some obscure sense, the work of the mind. The view which inclines to take the categorical judgment as the typical form, and attempts in consequence to reduce the hypothetical judgment to it, is attacked. A judgment is universally a necessary combination of concepts, equally necessary whether it be true or false. It must be either true or false; but its truth or falsehood cannot depend on its relation to anything else whatever—reality, for instance, or the world in space or time. Both of these must be supposed to exist, in some sense, if the truth of our judgment is to depend upon them; and then it turns out that the truth of our judgment depends not on them, but on the judgment that they, being such and such, exist. The truth or falsehood of this judgment must be immediate properties of its own, not dependent upon any relation it may have to something else. The existential judgment, which is presupposed in Kant's reference to experience, or in Bradley's reference to reality, remains merely a necessary combination of concepts, for the necessity of which we can seek no ground, and which cannot be explained as an attribute to 'the given.' A concept is not in any intelligible sense an 'adjective,' as if there were something substantive, more ultimate than it. It is not a mental fact, nor any part of a mental fact. Concepts are possible objects of thoughts; they may come into relation with a thinker; and in order that they *may* do anything, they must already *be* something. It is indifferent to their nature whether anybody thinks them or not. They are incapable of change; and the relation into which they enter with the knowing subject implies no action or reaction. It is a unique relation which can begin or cease with a change in the subject; but the concept is neither cause nor effect of such a change. It is of such entities as these that a proposition is composed. The difference between a concept and a proposition, in virtue of which the latter alone can be called true or false, would seem to lie merely in the simplicity of the former. What kind of relation makes a proposition true, what false, cannot be further defined, but must be immediately recognized. Existential propositions do not escape this description. We must regard the whole world as formed of concepts, these being our only objects of knowledge. Perception is to be regarded philosophically as the cognition of an existential proposition, and thus it furnishes a basis for inference. From this description of a judgment there must, then, disappear all reference either to our mind or to the world. Neither of these can furnish 'ground' for anything, save in so far as they are complex judgments.

EDWARD FRANKLIN BUCHNER.

*Time as related to Causality and to Space.* MARY WHITON CALKINS. *Mind*, No. 30, N. S., April, 1899, p. 216.

The phenomenal unity of different kinds of multiplicity is traced to the relations of time as controlling the categorical relations of causality and space. Heretofore, time and space have been treated in the same breath, much to the misfortune of each. Analogy is not taken as a guide in the treatment of the categorical complexities involved. The thesis of the paper is the assertion that time and causality are subordinate forms of the principle of the necessary connection of phenomena, and that the third and coördinate form of the category is reciprocal determination, not, as is often stated, space. Succession, and not duration, must be admitted as constituting the nature of the temporal manifold. The synthesis of manifoldness follows fundamental distinctions, involving two sorts of necessity: first, the dependence of synthesis in general upon ultimate unity; and second, of the moment upon the preceding moment. In this way it may be seen that time really belongs among the categories, as the irreversible connection of the irrevocable, relatively abstract manifold. The psychology of time-consciousness verifies the metaphysical doctrine. The awareness of more-than-one, possessing an inner connection, presents unanalyzable elements given immediately in consciousness. The causality connection is more easily applied to outer than to inner life, and thus remains subject to the temporal sequence. The spatial sequence is no fundamental category, or uniting principle, but itself one variety of the manifold to-be-categorized. Space, as a sense-quality or a notion, is clearly a construct of experience.

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*L'Éducation des Sentiments.* P. FELIX THOMAS. Paris, Alcan. 1899, p. 287.

The author, who has appeared before the public in other writings on philosophical and pedagogical subjects, presents us here with an analysis of the sentiments and emotions with pedagogical hints and suggestions as to the best methods of utilizing them. He combats the tendency in education toward excessive emphasis of the intellectual, and pleads the cause of the algedonic and volitional elements in our nature. Intellectualism tends to destroy the will and the pleasure-pain values of life. It is conduct and emotional value which makes life worth living, not creeds religious, philosophical or scientific. Pain

and pleasure depend upon the laws of vital rhythm. The appetites, desires, anger, fear, play, instinct of proprietorship, love of domination, curiosity, sympathy, pity, social inclinations, self-love, etc., are treated in turn.

The style is literary rather than scientific. Some very good suggestions are made, and the author generally strikes the right keynote in a happy manner. There is little justification for the neglect of recent American contributions on the same subjects. There is a good table of contents, but no index.

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*Il metodo deduttivo come strumento di ricerca.* GIOVANNI VAILATI. Turin, Roux, Frasati & Co. 1898. Pp. 44.

*Alcune osservazioni sulle questioni di parole nella storia della scienza e della cultura.* GIOVANNI VAILATI. Turin, Frat. Bocca. 1899. Pp. 39.

These two papers are introductory lectures in a course on the History of Mechanics, delivered by the author in the University of Turin. In the first Dr. Vailati discusses the value of the deductive method; he examines the history of discovery in mechanics, from Galileo down, and insists that many of its more important laws 'would still be unknown to man, at least in their generalized form, if he had not at his disposal another method besides that of observation and direct measurement.' Admitting the supremacy of induction, as a means of scientific discovery, the author, nevertheless, points out the important rôle that deductive reasoning has played from an historical standpoint. He believes that Bacon's diatribes anent the sterility of Aristotle's dialectic and the syllogism, were called forth by the excessive use of deductive methods in scholastic times, and would have been modified had induction received proper recognition in those days. The latter part of the paper is a discussion of the practical application of the various forms of induction and deduction to the discovery of scientific laws.

The second paper takes up the question of terminology in its relation to objective truth and the history of scientific thought. Dr. Vailati refers to the undue stress sometimes laid on the etymological significance of a word. He denies the *objective* importance of the distinction between definable and indefinable terms. The impossibility of defining a term may be due to the simplicity of the notion, as well as to its obscurity; in either case it is a subjective factor that distin-



guishes it from a definable term. The author works out for the benefit of his pupils a number of well-known principles underlying scientific discovery and the definition of concepts.

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PRINCETON UNIVERSITY.

*Pensare senza coscienza.* G. SERGI. (Reprinted.) La Rivista Moderna, Vol. II, Fasc. I. 1899. Pp. 18.

This is an amplification and in some respects an advance on the author's doctrine of the unconscious, as developed in his *Psychologie physiologique*. Professor Sergi starts out with the view that conscious thought is merely the last term in a series of unconscious brain states. In support of this theory he cites, from his own experience and others', numerous examples in which a problem has been solved or a train of reasoning worked out to a conclusion while the mind was occupied with something entirely different. In some instances the process extended over an hour, in others over a day, week, month or more. In his own case he finds many instances of this unconscious brain work proceeding during sleep; at one time it was so pronounced that he grew accustomed to read up the theme of any paper he was to write, and then immediately dismiss the whole question from his mind, without working out the plan of the paper; in the morning he would begin the writing at once with no hesitation or difficulty, the subject having apparently been analyzed and arranged for treatment during the night.

Professor Sergi reviews the theories of Kant, Leibnitz, Hamilton, J. S. Mill and Carpenter, on obscure ideas, subconsciousness and unconscious cerebration. He gives preference to Hamilton's view, that 'latent agencies—modifications of which we are unconscious—must be admitted as a groundwork of Phenomenology of Mind.' The author, however, goes further, holding that this "unconscious cerebral and physiological work constitutes the whole phenomenon, not merely one side of it, and that the consciousness of the phenomenon is merely its superficial revelation, which adds nothing to the essence and completeness of the phenomenon in question." He claims to solve the problem of psychological dualism, by making the physiological process the sole 'essence' and the state of consciousness a mere 'manifestation'. In spite of the brevity of the paper and the lack of novelty in its standpoint, it calls for attention on account of the new observational data which the author has brought forward.

HOWARD C. WARREN.

PRINCETON UNIVERSITY.

*Individual Memories.* F.W. COLGROVE. American Journal of Psychology, X., 1899; pp. 1-29.

This dissertation is based upon the returns of the Clark University questionnaires, and the results are presented in the usual form. 1,658 replies were tabulated upon a roll of paper one foot eight inches wide and fifty-two feet long, after almost incessant labor for five months. A second tabulation followed, grouping replies under more than sixty different headings. We admire the patience of the writer's wife, who did all this work, but we fail to discover any great value in the results. Absolutely no attention is given to the degree of certainty of the various conclusions; we are told that the males have the greatest number of memories for protracted or repeated occurrences, people, and clothing, and that they excel in topographical and logical memories, while females have better memories for novel occurrences and single impressions, for Christmas gifts and dolls, without a single figure to back up the important statement.

In the same way we are told that Indians find shorthand helpful to memory, and so on throughout the various subdivisions under the thirteen questions.

Nothing is said as to the class of people from whom the replies were collected, the ages of those questioned, or about the seriousness with which the answers were written.

C. B. BLISS.

*Schmeckversuche an einzelnen Papillen.* F. KIESOW. Philos. Stud., XIV, 4, 591-615.

This article gives an account of experiments, a continuation of work by Oehrwall, to discover whether or not the single taste papillæ reacted only to certain taste substances. Thirty-nine points on the tongues of two subjects were tested. Of these, four gave no reaction to salt, sugar, acid or quinine solutions; seven others gave characteristic tastes of each of the stimuli; one reacted only to sugar and another only to quinine. Of the remainder 19 + 5 (? = doubtful) reacted to sugar, 11 + 13 (?) to salt, 11 + 11 (?) to acid, and 6 + 8 (?) to quinine. Mechanical and electrical stimuli were used by the author, but the results are left for a later article.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.

## GENERAL.

*Ueber die Auffassung einfachster Raumformen.* RICHARD SEYFERT. *Philos. Stud.*, XIV., 4, 550-566.

This research was an attempt to discover some of the factors influencing the accuracy of reproduction (judgment) of simple geometrical figures. Various triangles were shown for a time under different conditions, and, after a few seconds, the subject attempted to reproduce the same.

The six following conditions were used: (1) Eyes fixed upon a point within the triangle; (2) eyes followed a point which described the sides of the (imaginary) figure; (3) the eyes were shut and the finger was moved over the sides of the triangle; (4) the triangle was looked at and the eyes were moved over its contour as in (2); (5) the eyes and finger were made to describe the sides of an imagined triangle; (6) the eyes followed and the finger described the form of a seen triangle.

Owing to the varied ability and training in drawing, this simple method (by drawing) of reproduction was not used. After the figure was shown or felt, the subject was given a card on which was drawn a base; on this he was instructed to mark with a pin the apex, and from this point sides were drawn to the extremities of the base and the angular errors were noted.

From the results of nine subjects the author concludes: (1) The decisive factor for accuracy of reproduction of simple forms is not the retinal image, but the sensation of eye-movements. The most exact reproduction of such forms occurs when the eye sees the figure as a whole and follows its outline. (2) Pure eye-movements without the image of the form, are next for exactness of the reproduction. With practiced subjects this kind of reproduction equals the first in exactness. (3) The perception with fixed eyes is very puzzling, and successful only for practiced individuals. With unpracticed subjects it is almost impossible to prevent the eyes following the outline of the object. (4) Simultaneous movements of the hand and eye ('und des Auges' not 'und des Armes', see pp. 558 and 560) as a rule lessen the accuracy of reproduction. Great practice of the muscles can increase the accuracy. (5) The least exact method is reproduction from pure hand and arm movements.

Horizontal and vertical errors in placing the apex of the figure would give similar results. These errors the author has not attempted to separate.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.



*Bemerkungen über Kinderzeichnungen.* KARL PAPPENHEIM.  
Zeitschrift für pädagogisch Psychologie, I., pp. 57-73.

This is a review of the different studies made of children's drawings chiefly in America. It covers methods of study of children who show special aptitude, the origin of types, the different stages of development, the relation of drawings to memory, observation and language. The use of drawing in the teaching of botany, geography and zoölogy is supported.

*Heredity and Environment. A Study in Adolescence.* EDGAR JAMES SWIFT. American Physical Education Review, 1898, pp. 8.

*Reflex Neuroses in Children.* EDGAR JAMES SWIFT. American Physical Education Review, 1899, pp. 8.

The first address describes a series of questions asked of Reform School boys, about the causes which had brought them into trouble. The results, though not decisive, in many cases offer good suggestions for further work.

The second address calls attention to the fact that defects of the eye, ear, or nose, are often causes of dullness in school children.

C. B. BLISS.

## VISION.

*Wahrnehmungen mit einem einzelnen Zapfen der Netzhaut.* G. F. SCHOUTE. Ztsch. f. Psych. u. Physiol. der Sinnesorgane, XIX., 251-263.

The author of this interesting paper shows, in opposition to Asher (Ztsch. f. Biol., XXXV., 400), that it is perfectly possible to throw upon the retina an image whose diameter is less than that of a single cone. This is important, because it removes any doubt that may have been felt as to the validity of the demonstration, by Hering and by König, that a minute point of white light is *not* seen to be now red, now green and now blue, as it falls now upon one and now upon another of the retinal cones. This demonstration gives an experimental death-blow to any three-fiber theory, and in consequence no such theory has of late years been upheld by any one. Holmgren's experiments of an opposite bearing have failed to win acceptance.

Schoute finds that he can distinguish no less than eight different sizes in small bright objects when their images are, even the largest of them, so small as to fall upon the top of a single cone. Such facts as this have hitherto been explained by supposing that, though no differ-

ence can be perceived in the images themselves, there is nevertheless sufficient light in their diffusion circles to enable the judgment to distinguish between their different sizes.

Schoute shows by ingenious experiments that this is not the source of the distinction, but that it rests simply upon the difference in the amount of luminosity. Within the range of these small dimensions, a given object cannot be distinguished from another which is both smaller and brighter. If a larger amount of light falls upon a given cone, we have no means of knowing whether it comes from a larger or from a brighter object, but because we are far more interested, in general, in the size of objects than in slight differences in their brightness, and hence make a far greater number of judgments of this nature than of the other, we here interpret an ambiguous difference in sensation as a difference of that character which stands for more to us. (In these small images we have also no means of distinguishing shape, and hence all such objects appear to us to be of the simplest shape or round.) The proposition is thus established that for images which fall upon a single cone, the judgment as to size is determined by the product of surface and intensity of light, as has been shown before, in fact, by Ricio (*Ann. d'Ottalmol.*, 1877). Asher's error was an error of method; he looked at a minute object with a microscopic arrangement of lenses, and found that for different degrees of diminution of its image, it always appeared to be of the same size; but this is merely what was to be expected, in the light of present results, for the quantity of light thrown upon the single cone was in each case the same. Schoute makes the curious observation that when the image of an object covers more than two cones, he has the distinct feeling of basing his judgment as to size upon the pure sensation of extension, that with equal certainty he feels that he is guided by difference in brightness alone when the image falls upon one cone only, and that when it is of just the size of two cones he finds his judgment wavering, so that he cannot say with certainty whether he is judging of size from brightness or from the extent of the image.

C. L. FRANKLIN.

BALTIMORE.

*Subjective Colors and the After-images: their Significance for the Theory of Attention.* MARGARET F. WASHBURN. *Mind*, N. S., 29, January, 1899.

Professor Washburn reports with admirable clearness the results of a series of experiments upon the possibility of influencing the succes-

sion of colors in after-images by the vivid *image* of a color. The most important outcome of the paper is summarized in the statement which follows:

The conditions under which the after-images were obtained were those suggested by Helmholtz in the *Physiologische Optik*. The subjects, of whom there were four, fixated, for twenty seconds, one point of an upper window frame; their eyes were then closed and covered, and they noted the sequence of colors of the after-images. The subjects were practiced until this order became invariable; they were then directed 'by an effort of will' to 'turn the image red all through its course.' Similar suggestions were made in regard to blue and to green. These suggestions were almost invariably effective, either by intensifying 'the traces of color already present in the field,' or by lengthening or anticipating the time of a suggested color, which normally occurred in the after-image series. Thus, a subject whose ordinary sequence of colors in the series of after-images was 'blue-positive, green-positive, red-negative, dark-blue-negative,' when asked to visualize red had the following series of color changes: 'first, a red image with dark lines, interrupted once by a momentary green image; the dark lines then became bright and the red negative image remained until the end of the series, traces of the blue appearing from time to time.'

The most evident inference from these results is the identity, for psychology, of percept and image—of sensations peripherally and centrally aroused. These experiments, therefore, though so distinct in subject-matter, strengthen the conclusions from Dr. Washburn's earlier study of the effect of visual images upon cutaneous localization.

The results are also considered in their relation to the doctrine of attention. 'The effort to call up subjectively a certain color meant,' at least for the three subjects of moderate visualizing power, 'simply an unusually intense effort to attend to that color.' But the result of this attention was an actual increase of the intensity and duration of a peripherally excited sense-experience, and it follows that the "function of attention is positive as well as negative, intensifying as well as inhibiting."

A criticism is added of Wundt's theory that the frontal lobes are an attention-center. Against this assumption it is urged that it accounts for nothing which can not be as well explained 'on the hypothesis that the organ of attention is the cortex as a whole'; but though the argument is well sustained it does not connect itself closely with the experimental results.



As a whole, the paper is an instructive illustration of the value of the experimental results which may be obtained without the aid of laboratory or of apparatus, by an investigator who is quick to apprehend a problem, accurate in defining it and ingenious in methods of working it out.

*Zur Kenntniss der nachlaufenden Bilder.* A. SAMOJLOFF. *Zeitschrift f. Psychol.*, XX., 2 and 3.

Samojloff experimented, at von Kries's suggestion, on the after-images, from morning light-stimuli, with especial reference to the points at issue between von Kries and Hess: the color of the after-image and its relation to stimulation of the center of clearest vision. The method and apparatus of the older experiments were completely set aside, in order to avoid the sources of possible error suggested by Hess. The color stimuli were given through openings of a revolving desk, which formed the front of a 'dark box' whose degree of illumination could be regulated. The results confirmed von Kries's conclusions; the after-images of the yellow stimulus were blue, and those of the blue were yellowish, that is, the after-images were negative, and not in accordance with Hess's results. Positive experiments on the stimulation of the center of vision also show results similar to those of the earlier Freiburg experiment: in the vivid, even though inexact, words of Samojloff "the after-image overleaps the central region around the fixation-point." It is shown that this is not a mere case of obliteration of an after-image through the brightness of the fixated point, for an equally intense light, illuminating the *periphery* of the retina does not annihilate the after-image.

The writer calls attention to the close correspondence of his results with von Kries's theory that the after-image depends upon the activity of the '*Dunkelapparat*' which is wanting in the center of vision; yet he does not claim that these experiments furnish 'rigid proof' of the entire lack of the '*Dunkelapparat*' in this part of the retina.

M. W. CALKINS.

WELLESLEY COLLEGE.

## PATHOLOGY AND NEUROLOGY.

*L'Instabilité Mentale. Essai sur les donnés de la psycho-pathologie.* G. L. DUPRAT. Paris, Alcan, 1898. 8vo. Pp. 310.

The motive of M. Duprat's book is not so much psychological as philosophical: his intention, in his own words, is less to write a book of science than to consider scientific conclusions and to examine the

first principles of the science with which he deals in order to give it, if possible, a philosophic foundation. In pursuance of this plan M. Duprat attempts to show the primacy which psychology has over physiology in the study of mental pathology. The more particular purpose of the book is to emphasize the importance of the concept 'mental instability' in psychical disease, and to relate all concrete mental maladies to this as species to genus. No mental process can normally occur unless there exist a principle directing the mental evolution, which by its permanence resists the natural instability of the mind. The more feeble the principle, the greater the distraction. Duprat's book, then, occupies itself with collecting the various medico-psychological observations upon the diverse forms of psychopathy, and with discovering in each of these forms a foundation of psychological instability. In pursuance of this purely philosophical plan the book is divided into three parts.

The first part, a general introduction to the rest, is concerned with the mental processes as a whole, normal and abnormal, and attempts to show that biology can go only part of the way in psychiatry, and that psychology must do the greater part of the work. It is impossible to deny the existence of biological disturbances underlying psychic ones, but there may at the same time be purely psychological causes of psychopathies. The biologic centers are also psychic centers.

The second part occupies itself with the consideration of the various psychopathic symptoms in detail, and attempts to find in each the fundamental fact of mental instability. This root-malady is classed according to its four aspects: instability of the intellect—incoherent thought; instability of the tendencies—the illogical rise of one from another; instability of the feelings—the rapid alternation from love to hate, etc.; and instability as action—aboulia, ataxia, etc. In the same part of his work, after considering the particular mental diseases specifically, M. Duprat considers them as a whole, under the title 'pathology of personality,' and the alternations of this general psychopathy according to sex, habit of life, and age. Here is included marked mental stability—the stubbornness of melancholia, for instance—which is shown itself to be rooted in the more fundamental disturbance of mental instability.

The third and last part of the book is occupied with the practical conclusions resulting from these conclusions.

The value of M. Duprat's book, as he himself admits, is purely philosophic, and can have interest only for those interested in attempts at logical classifications, and the inclusion of specific concepts under

one concept embracing them. This being the case, it is to be regretted that M. Duprat has been unable to define his class concept of mental instability in any definite way; so that after following all the concrete mental pathologies through M. Duprat's close-written pages, and learning that they are explained by one inclusive concept—mental instability—we are compelled to ask, what is mental instability?

D. P. BARNITZ.

HARVARD UNIVERSITY.

'*Zur Theorie der Nerventhätigkeit.*' Professor EWALD HERING ('Akademische Vortrag'). Leipzig, 1899. Pp. 31.

This little pamphlet of Professor Hering, while not the product of actual experimental research, has interest and value because it represents the opinion of a man who is, from his broad outlook, most competent to judge in a case so long and actively controverted as is this one. The writer in substance upholds the doctrine of the specific energies of the various parts of the neural organism, following therein especially J. Müller, but he goes further (as the rise of the neuron-theory necessitates), and strongly believes that not the cells of the neurons only, but also the prolongations from these have forms of nervous activity peculiar to themselves and to their respective uses in the organism. "The activity of the neuron and of its fibers," he says "may depend not alone, as some think, on the intensity but also on the quality of its sort of stimulus, whether this come from its own peripheral sense-organ or from a neighboring neuron." The physical basis of the difference in function is deemed to consist in the various forms of neural vibration which, with indefinite differences in the neural substance chemically, is emphasized as the 'inheritance' of the neuron and its projections.

GEORGE V. N. DEARBORN.

COLUMBIA UNIVERSITY.

## NEW BOOKS.

*Psychology and Life.* HUGO MÜNSTERBERG. Boston, Houghton, Mifflin & Co., 1899. Pp. xiv + 282.

*Criteriologie Générale, ou Théorie générale de la certitude.* D. MERCIER. Louvain, Inst. Super. de Philosophie. Pp. v + 371. 6 Fr.

*Discorsi su la Natura e sul Governo dei Popoli.* F. P. C. SIRAGUSA. Palermo, Virzi, 1899. Pp. 410. L. 5.



*The Messages of the Earlier Prophets.* F. K. SANDERS and C. F. KENT. Second ed., New York, Scribners, 1899. \$1.25, net.

*The Psychology of Reasoning.* A. BINET. Trans. from 2d French edition by A. G. WHYTE. Chicago, Open Court Co., 1899. Pp. 191.

It is well to have in English this new edition of Professor Binet's well-known book—one of the first publications of this prominent French psychologist. Its positions are too well known to require statement. The translation is very well done. J. M. B.

*Die Philosophie der Geschichte als Sociologie.* P. BARTH. Erster Teil, Leipzig, Reiland, 1897. Pp. xii + 396.

Mainly a historico-critical review of sociological theories. Extended notice of this important work is reserved until the appearance of the later parts. J. M. B.

*La Psicogenesi della Istinto e della morale secondo C. Darwin.* P. SCIASCIA. Palermo, Reber, 1899. Pp. xv + 178. L. 4.

*Talks to Teachers on Psychology and to Students on some of Life's Ideals.* WM. JAMES. New York, Holt, 1899. Pp. xi + 301.

*Marriages of the Deaf in America.* E. A. FOY. Washington, GIBSON for Nolta Bureau, 1898. Pp. vii + 527.

A valuable statistical study, with conclusions on the inheritance of deafness, etc., having important general bearings.

J. M. B.

*Personal Competition.* C. H. COOLEY. Vol. IV., No. 2 of Economic Studies, American Economic Association. New York, Macmillans, 1899. Pp. 173.

*Les Transformations du pouvoir.* G. TARDE. Paris, Alcan. 1899. Pp. x + 266.

*Wörterbuch der philosophischen Begriffe und Ausdrücke.* R. EISLER. Dritte Lieferung, *Empfindung to Geschichtsphilosophie.* Berlin, 1899. M. 2.

As this important *Wörterbuch* proceeds, both its excellences and its defects appear. It is made up mainly of citations under each head of definitions by various authors. It attempts no critical or definitive settling of meanings. It gives no equivalents in other languages. Its greatest defect is its extraordinary limitation in the matter of literary citation—limitation to German sources. Of English and American writers since Hamilton and Spencer, we have noticed in the psychological articles of the three first *Lieferungen*: one reference to James, one to Stout, one to Baldwin, and none to any other English or American writer except Bain; and this, after looking up several of the most

important psychological topics. The compiler seems limited, in his citations of both French and English authors, to works which have been translated into German. Wundt is the authority quoted under all the headings. When completed, the work, which is a perfect mine of citation from German writers, will be given full notice in the REVIEW.

J. M. B.

*Nervous and Mental Diseases.* H. CHURCH and F. PETERSON. Philadelphia, W. B. Saunders, 1899.

A remarkably able and valuable compendium. The Neurology is written by Dr. Church and the Psychology by Dr. Peterson. It is fully illustrated and the cuts of apparatus have great interest to the psychologist, to whom indeed the entire book should prove of very great value. We hope to print a detailed expert review.

J. M. B.

*The Metaphor: A Study in the Psychology of Rhetoric.* G. BUCK. Inland Press, Ann Arbor, Michigan, no date.

*Geschichte des Lebensmagnetismus und des Hypnotismus von den ältesten Zeiten bis auf die Gegenwart.* H. R. P. SCHROEDER. In 12 Lieferunden. Parts I.-V. Leipzig, Strauch, 1899. Parts M. 1 each.

*Das Hypnotische Hellseh-Experiment in Dienste der naturwissenschaftlichen Seelenforschung.* R. MÜLLER. I. Band, das Veränderungsgesetz; Band II., das normale Bewusstsein. Leipzig, Strauch, 1899. Pp. viii + 168, and 169-322. M. 5 and 4.

*Bewusstsein und Hirnlokalization.* W. v. BECHTEREW. Deutsch von R. WEINBERG. Leipzig, Georgi, 1898. Pp. 50. M. 1.50.

*Suggestion und ihre sociale Bedeutung.* W. v. BECHTEREW. Deutsch von R. WEINBERG. Vorwort von P. FLECHSIG. Leipzig, Georgi, 1899. Pp. iv + 84.

*Mathematical Contributions to the Theory of Evolution, VI. Genetic (Reproductive) Selection. Inheritance of Fertility in Man and of Fecundity in Thoroughbred Race Horses.* K. PEARSON, A. LEE, and L. BRAMLEY-MOORE. Philos. Trans. Roy. Society of London; London, Dolan & Co., 1899. 3s. 6d.

*Des Religions Comparées, au point de vue Sociologique.* R. DE LA GRASSERIE. Paris, Giard et Brière, 1899. Bib. sociologique intern., No. xvii. Pp. 396. 9 fr. or 7 fr.

*Interpretation Sociale et Morale des Principes du Développement Mental.* J. MARK BALDWIN. Paris, Giard et Brière, 1899. Bib. sociologique intern., No. xviii. Pp. vi + 580. 12 fr. or 10 fr.

*Naturalism and Agnosticism.* JAMES WARD. Gifford Lectures, Aberdeen, 1896-1898. London and New York, 1899. Pp. xviii + 302 and xiii + 294. \$4.

## NOTES.

DR. A. E. LOVEJOY has been appointed assistant professor of philosophy in Stanford University.

*From Comte to Benjamin Kidd; the Appeal to Biology or Evolution for Human Guidance* is the title of a book by Robert Mackintosh, to be published immediately by The Macmillan Company.

PROFESSOR A. H. KEENE, F.R.G.S., late Vice-President of the Anthropological Institute of London, has written a work on *Man, Past and Present*, which will be published in the United States by The Macmillan Company.

DR. D. S. MILLER, formerly of Bryn Mawr College, is to give courses (see the Harvard 'Announcement' in this issue of the REVIEW) in the Harvard Philosophical Department during the coming year, Professor James being away on his 'Sabbatical' vacation.

WE regret to record the death of Professor Ludwig Strümpel, of Leipzig. He had reached the age of 87 years.

AMONG psychologists and philosophers summering abroad we note President Patton and Professors Gardiner, Howison, Bliss.

PROFESSOR JASTROW is to return to his work in the University of Wisconsin in September.

ASSISTANT PROFESSOR F. KENNEDY has been made full Professor of Philosophy in the University of Colorado.

P. H. HORNE, a graduate of and instructor in the University of North Carolina, has been appointed instructor in the department of philosophy in Dartmouth College.

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ON page 288 of the May number of the REVIEW, line 2 should be 'asserts that there *is an* instinctive fear of a cat.' The title of the article should be 'The Instinctive Reactions of Young Chicks.' On page 286, in the last line, 'prooning' should be 'preening.'

EDWARD THORNDIKE.



# THE PSYCHOLOGICAL REVIEW.

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## A PLEA FOR SOUL-SUBSTANCE. (I.)

BY W. P. MONTAGUE.

*Instructor in Logic, University of California.*

### I.

#### *De Anima, an Anima Sit.*

Modern philosophy, both empirical and transcendental, has manifested a growing hostility toward all doctrines may be labelled 'scholastic.' Substance in general and soul-substance in particular are concepts that are peculiarly and essentially scholastic, and as such they have fallen into pretty general discredit with the thinkers of this century. Herbart and a few others have, indeed, favored the hypothesis of the 'something I know not what' as the basis and support of our mental life, but these defenders are few and their theories of the soul have not greatly influenced the psychology of to-day. Indeed, it is just the very question of 'how to get along without substance' about which all the tendencies of modern speculative philosophy may be said to center. Pre-Kantian and ancient thought accepts the conception of substance; modern thought rejects it; and philosophy, since Hume and Kant, can be understood as a series of efforts to explain phenomena without referring them to substance or substances. In the place of the indefinite something called substance Kant put the definite nothing, or *ding an sich*, leaving, as the only tangible subject matter of metaphysics, phenomena and the laws of phenomena, Content and Form. Given these two categories as the data of speculation, the question naturally arises as to which of the two is to be regarded as primary. Is it form or relation-stuff, or, on the other hand, is it content or sensation-stuff in terms of which experience is

to be described? The answers to this question are to be found in the two schools or tendencies of apriorism and empiricism. Both apriorist and empiricist are united in repudiating the notion of substance, and both join in attacking materialism and agnosticism, or, indeed, any doctrine which does not bow down to the all-sufficiency of the two categories of form and content.

This attitude of philosophy is thoroughly in keeping with the spirit of the age, recognizing, as it does, the importance of utility and economy alike in the conceptional as in all other spheres of action. For note: Spinoza found that he can get along with one substance instead of two; Kant reduces substance to shadow, while his successors, with a still greater ardor for conceptual economy, attempt to do without even the shadowy *ding an sich*. "Give us form and content, or even pure form alone, and we can deduce or *explain* everything," cry the post-Kantian idealists.

"Give us content and form, or content and the *fictions due to habit* (which latter are themselves mere facts or phenomena), and we can unify or *describe* everything," cry Hume and his disciples—each school endeavoring to economize by doing without some conception deemed necessary by a preceding school, and each justifying its omissions on the ground of the *inutility* of the discarded category. As a consequence of this wise frugality we find that the modern as distinguished from the ancient criterion for accepting or rejecting a new hypothesis consists wholly in the utility or non-utility of the proposed conception, and not in its inherent rationality or irrationality.

Since this is so, it is fitting that in attempting to reinstate the conception of a substantial soul we should begin by assuring ourselves that there exists a genuine need for some such conception. Are we able to explain mental phenomena without the hypothesis of soul? is this hypothesis of any *use* to philosophy or psychology? Now the various departments of philosophy have all shown their eagerness to answer in the negative. "The soul monad is not what morality and religion demand. It is not required by metaphysics or epistemology." But psychology, in particular, has outstripped the other philosophical sciences in the vigor and frankness with

which it denies and repudiates the existence of soul-substance. We have in fine the gladly acknowledged paradox of a 'psychology without a soul.' There are, it is true, no end of substitutes for the old substantial soul, 'formal unities,' 'concrete totals of experience,' 'unique centers of perception and activity,' besides all sorts of 'Egos' transcendental and otherwise; but no one of these has either the virtues or the vices of the mediæval soul-substance.

Since it is in psychology that the complete uselessness of the soul is supposed to have been most clearly demonstrated, it will be well for us to undertake our task from the psychological standpoint rather than from any other. We begin then by indicating the facts which would seem to us to necessitate the acceptance of the theory of soul-substance as a *sine qua non* of modern scientific psychology.

Psychology has for its subject matter states of consciousness as such, *i. e.*, thoughts, sensations, feelings, etc., considered as 'facts' and not as 'values'—mental content viewed apart from its normative worth. The same phenomena of consciousness which, when treated from the point of view of worth or conformity to ideals, make the subject matter of the *normative* sciences of Logic, Ethics and Æsthetics, when treated merely as facts form the subject matter of the *descriptive* science of Psychology. It is for this reason that psychology occupies a unique position among the sciences. It is, or at least it ought to be, a strictly descriptive science; at the same time all its data have a normative aspect. As a descriptive science it is bound to repudiate final causes and to recognize only efficient causes; and yet there are scarcely any mental sequences which can be understood apart from teleological—*i. e.*, normative or unpsychological considerations. Take, for example, the following mental sequence

$$\begin{array}{ccc} a & \beta & \gamma \\ \underbrace{\quad} & \underbrace{\quad} & \underbrace{\quad} \\ a=b, & b=c & \therefore a=c \end{array}$$

Considered teleologically the *causal law* of this sequence is obvious; but the same sequence considered psychologically is



by no means so easy to deal with. The Law of Identity or the *dictum de omni* can have no direct meaning for psychology, and so when we seek the cause of the inevitable succession of the mental state  $\gamma$  upon the preceding states  $\alpha$  and  $\beta$  we are in a quandary, and are apt to explain the sequence on some such grounds as the possession by the individual thinker of certain organic dispositions, certain brain conditions which respond with mechanical necessity to particular stimuli. In short, we are in duty bound to suggest any explanation whatever, no matter how complicated, so long as it be not the natural explanation of the teleological Law of Identity operating on a rational mind. It must once for all be understood that every fact, mental as well as physical, has an *efficient* cause; and it is the business of all descriptive sciences to seek out these efficient causes by the method of Induction. The flowers are what they are, not because of the delight which they give, but simply because of certain material conditions, to ascertain the nature of which is the business of the descriptive science called botany. The *final* cause is quite outside the world of facts, and never, except indirectly, is it of the least use in scientific explanation.

The mental world offers the spectacle of a seeming conformity to teleological norms; indeed so strongly is this evidenced that when, after a long train of reasoning, we, as psychologists, are forced to say that the various rational ideals by which our reasoning has been governed are absolutely and utterly ineffective, and that not one single act can be said to have its true cause in any rational consideration; when I say we are forced to acknowledge this, we seem to ourselves to commit an absurdity, the absurdity namely of endeavoring to make mental phenomena amenable to the canons of descriptive science. What indeed remains to psychology if final causes are banished? How few and of what a low order are those mental sequences in which we can get even the smallest glimpse of the mechanical or efficient causes which are to explain them? While on the other hand, it is equally true that if we do *not* give up final causes, we admit the impossibility of a *science* of psychology. To say that a final cause can in itself be a *vera causa* in producing any effect in the world of phenomena is, from the point

of view of the modern scientist, exactly the same as talking about noisy triangles or yellow lies. The two spheres of mechanism and finality of 'fact' and 'value,' of 'description' and 'appreciation,' of 'Madam How' and 'Lady Why,' are and ought to be separated with genuine Cartesian rigor.

Now grant all this, and we can see at once that the psychologist has upon his hands a first-rate mystery of the highest order—the great and ever-present mystery of

#### THE SEEMING EFFICACY OF FINAL CAUSES IN THE WORLD OF MENTAL FACTS.<sup>1</sup>

The existence of this mystery cannot be doubted, and the need of its solution is so pressing that until this need is satisfied the psychologist has no right to dignify his study by the name 'science.'

The methods of solving this mystery are five. In the first place, we may hold that it is the efficient causes which are fictitious, and that final causes rule the world and the details of the world that everything happens because of its fulfilling some rational end and for no other reason whatever. Teleology of this extreme type is, indeed, logically conceivable as a means of explaining the seeming communication of the two worlds; but inasmuch as this theory precludes not only descriptive psychology, but any descriptive science whatsoever—*i. e.*, any science which seeks for the how of a process rather than for its possible why—it may here be passed over.

The remaining four methods of grappling with our problem are the several doctrines of 'materialism,' 'occasionalism,' 'parallelism' and 'spiritualism.'

Just as the theory of absolute teleology mentioned above is possible only if science is abandoned altogether, so its counterpart materialism is incompatible with a belief in the *meaning* or *significance* of any phase of experience. Take, for example, the case of a sequence of psychical states culminating in a rational 'conclusion': just so soon as we deny that the conclusion

<sup>1</sup>This puzzling phenomenon of the apparent interaction of two totally incommensurate orders of experience is, of course, not confined to psychology, but it is nowhere else evidenced with such unambiguous clearness.

was reached because of any logical significance or teleological reference contained in the premises—and this denial we as materialists are bound to make—then indeed the meaning of the conclusion—*i. e.*, the conclusion itself—has simply vanished.

In  $a = b$ ,  $b = c$ : therefore  $a = c$ , to a really consistent materialist the 'therefore' which precedes the conclusion is out of place. It is not because  $a$  and  $c$  are both equal to  $b$  that they are equal to each other, but because the psychophysical nature of the individual who thinks the sequence happened to be so constructed that the mental states  $a = b$ ,  $b = c$ ,  $a = c$ , succeeded one another with mechanical *necessity*. But if the conclusion  $a = c$  is due to mechanical causes, and in no sense to the law of identity, then all meaning is gone from the syllogism. In short, materialism contains its own disproof in that a rational demonstration of the materialistic thesis would be inconsistent with a view which denies significance to final causes; for any rational demonstration depends for its validity on final causes—that is, on teleological rather than mechanical consideration.

Leaving these two extreme methods of accounting for the seeming efficacy of final causes, we come to the remaining three doctrines, which in their several ways endeavor to compromise the matter. Let us begin by considering the theory of Occasionalism.

The advocate of occasionalism grants the existence of both efficient and final causes, grants also the fundamental difference between them, and boldly asserts that notwithstanding the fact of incommensurability the two realms of mechanism and teleology, of matter and mind, do actually interpenetrate—by the aid of a miracle. Every time that the rational sequence of ideas is influenced by the material world, and every time that material events are made to conform to rational law, then is a miracle performed by God. Occasionalism has the merit of recognizing the three great truths which are the data of our problem:

1. The full significance of the separation of Finality from Mechanism.
2. The equally evident truth of the reality of both worlds.
3. The apparent influence which they occasionally exert upon each other.



Notwithstanding the credit due to occasionalism for its frank recognition of the difficulties of the problem, the theory itself is impossible as a serious doctrine, at least for contemporary thought. Not only have miracles gone out of fashion, but also the very notion of a miracle is entirely negative as a scientific explanation. The scientist rejects occasionalism and all kindred doctrines, not so much on account of their probable falsity, but rather on account of the certainty of their uselessness to science even if proved true. Occasionalism and a science of psychology are mutually exclusive, and to presuppose the miraculous—*i. e.*, inexplicable character of what is to be explained—is at best an unfruitful method of procedure. In view of all this we are justified in clearing the field of all hypotheses except those of Parallelism and Spiritualism.

The parallelist holds that mind and matter are two separate worlds parallel to one another, and that they never come in contact any more than do two plane parallel lines. As for the 'psychologist's mystery,'—*viz.*, the *apparent* contact of mind and matter—it is accounted for by an established harmony due either to a divine person or to natural evolution. This is the doctrine which in one form or another is the basis of whatever is best in modern psychology. Clearly formulated by Spinoza and Leibniz, it remained for Kant to establish it upon a sound epistemological basis.

The great scientific advantages of this theory of psychophysical parallelism are easily brought to light by comparing it with the three rival methods just treated. It resembles the absolute teleology of the first method in so far as it leaves to the world all its significance and meaning, but it differs from that method in not rejecting true scientific or mechanical explanation. And, again, it is quite as scientific as materialism without at the same time sacrificing all norms and ideals to a blind mechanical fate. While, thirdly, as compared with occasionalism, it is equally frank in recognizing the distinction between finality and mechanism, yet it substitutes for a series of miraculous acts on the part of the Deity a single miraculous construction of the universe, which is obviously a great gain in conceptual economy. Take, for example, the case already given: a syllogism contain-

ing universal truths is uttered by an individual man. The pure teleologist denies *in toto* the psychological or descriptive side of the process; while the materialist, if consistent, must deny that the conclusion was due to any rational or logical causation whatever. The occasionalist refers the mystery to a miraculous interference; but the *Parallelist* at once undertakes a dual investigation which has for its ends (1) a logical or normative explanation of the syllogism, and (2) a mechanical or descriptive account of the *how* of the process, *i. e.*, a statement of the psychophysical conditions existing in the organism of that particular individual which enabled the psychophysical event called the 'conclusion' to follow with mechanical necessity the psychophysical events called the 'premises.'

It needs, however, only the most casual glance to see the immeasurable superiority of parallelism as compared with the other methods. If parallelism is to yield to any rival hypothesis, certainly that hypothesis can only be the hypothesis of a soul-substance.

Let us now turn from the strong points of the parallelistic doctrine to some of the difficulties connected with the theory. In the first place, we are struck with the fact that parallelism, when taken seriously as the ultimate explanation of our problem, presents us with a universe which is extremely and disagreeably *artificial*. Parallelism in the theistic or Leibnizian form would have us believe that the Deity went to an infinite amount of apparently needless labor to get the atoms so arranged that they should be in exactly the right position in the brain of every future thinker or speaker to accompany mechanically his particular utterances and the complicated, because teleological, sequences of those utterances. It may, indeed, be urged against this objection that its cogency is purely emotional and not logical; and, furthermore, that it is no more than the objection to which any theory of preordination is exposed. Now it is true that the objection of artificiality is not a logical objection. It is, after all, a healthy common-sense bias in favor of simplicity, rather than a reasoned conviction of its truth, which leads us to look askance upon the possibility of the ultimate validity of any needlessly artificial hypothesis. And Leibniz's monadology, in

spite of, or perhaps because of, its wonderful ingenuity, is a doctrine which it is morally impossible to accept. We feel, indeed, that Leibniz himself, had he been the creator, might very well have overcome the difficulty of connecting mind and matter by copying from the clock-maker; but although we may believe that Leibniz, or, for that matter, any one of us, might have adopted this plan of creation, we know all the while that God did not and could not. And really to suppose that he did, is to take our own abstractions too seriously. Nor can it be urged that the preëstablished harmony is no more artificial than the common and reasonable notion of predestination, for the latter simply implies the divine authorship of a concrete world of souls, while the Leibnizian doctrine implies a divine fitting together of two human abstractions. But if we turn away from such peculiarly artificial anthropomorphism, we come at once to the other horn of the parallelists' dilemma, we have, namely, the task of giving a naturalistic in place of a supernaturalistic account of the origin of this wonderful harmony. Perhaps, as one psychologist has expressed it, we are justified for methodological purposes in regarding the atoms as *'having conspired together' to form combinations and sequences, which, although conducted entirely on the mechanical plan, shall yet fit in so nicely with the teleological world of mind as to appear to influence and be influenced by it.* There is perhaps no logical ground for rejecting this conceit. We cannot say that such a blind atomic conspiracy is infinitely improbable for the reason that the distinction between the probable and the improbable becomes meaningless when, all data being transcended, it is applied to the universe as a whole, Von Hartmann to the contrary notwithstanding. Nevertheless, despite the advantage of this theory as a working hypothesis, I must again fall back upon the ignoble refuge of common sense, and appeal to our emotional prejudice against such artificiality as is manifested in this account, as sufficient reason for rejecting it as an ultimate solution of the 'psychologist's mystery.'

Let us see, however, whether the modern doctrine of evolution can offer a less unsatisfactory method of explaining the genesis of a parallelistic world. Can it be shown that the



joining of mind and matter in the living organism is an aid in the struggle for existence? If so, we have a fairly reasonable theory, and a theory that is in harmony with the Darwinian spirit of the times. The seeming artificiality of the two previous types of parallelistic cosmogony is no longer a drawback to the theory itself, for we have the simple knowledge that parallelism is a 'survival of the fittest,' and that a psychophysical organism,—*i. e.*, a brain or nervous system—has outstripped both the purely physical and the purely psychical forms of existence. Now if this claim can be justified, parallelism has a very strong case. But can it be justified? Let us remember that mind and matter can never have influenced *one another*, for the very reason that they are parallel and incommensurate. There is a certain formation of matter, to wit, the brain, which happens to harmonize with thought—*i. e.*, to act as if it were affected by final causes; and, again, there is a certain kind of mind-stuff that acts as if it were influenced by physical causes. But if mind and matter are really parallel, if they never affect each other, how can the principle of natural selection aid us in accounting for the origin of their harmonizing in the living being? From the point of view of matter, consciousness must be regarded by the parallelist—as by Huxley—as an *epiphenomenon*, a phenomenon which is absolutely without effect in the material world. We are automata endowed with consciousness—*i. e.*, endowed with the power of looking on at the actions performed by our bodies. But if consciousness and matter *in general* are helpless to affect one another, it follows that *particular types* of consciousness and of matter (*e. g.*, a mind apparently conforming to mechanical laws or a brain apparently conforming to teleological laws) will be equally helpless to aid or to hinder the actions of their counterparts. If consciousness is a mere spectator, we may for a moment suppose it to be absent. What change would then result? No change at all. The world would proceed exactly as before; human bodies would walk and talk and go through all the multitude of apparently rational actions, and indeed, according to Huxley and the consistent parallelist, there would not be the slightest difference, from a factual standpoint, between our living world and a world

which was absolutely without life—a world which lacked the epiphenomenon of consciousness. To return then to the point at issue: we see that although a *brain* might very well be a survival of the fittest, the *joining of brain to consciousness* could never be such a survival without allowing—contrary to hypothesis—that consciousness is a true cause and not an epiphenomenon in the material world.

But supposing that we waive all the difficulties attending the naturalistic as well as the supernatural theories of the origin of parallelistic harmony; suppose, I say, that we waive all objections to the genetic side of the matter, and pass to a consideration of the logical consistency or possibility of this alleged harmony. We are told that mind and matter do not really influence one another, that they are parallel and that the causal law does not bind them together. Inductive science has furnished us with a grandly simple test, which is expressly meant to be applied to all such cases as this; we have only to use the three following rules in order to determine whether or not two things are causally united.

Let the two phenomena be *A* and *B*; then the rules may be expressed thus:

1. When *A* is absent *B* must be absent.
2. When *A* is present *B* must be present.
3. When *A* varies *B* must vary proportionately.

Now, if mind and matter should ever be found to fulfill these three conditions, would it not be a scientific duty to regard them not as parallel and sundered, but as reciprocally influenced by one another? As a matter of fact, we do find a class of cases which fulfill these three conditions, namely, all cases of individual life. And inasmuch as life, so far as we know, is never unindividualized, we are justified in saying that all life is an example, and the only example, of a causal relation between mind and matter—between the realms of teleology and mechanism. Whenever there is life then it holds true that

1. Absence or cessation of consciousness involves (or is invariably accompanied by) the absence of certain material conditions or brain processes; and conversely, when these brain processes are interrupted consciousness ceases.<sup>1</sup>

<sup>1</sup>About the precise nature and location of these processes little is known, but of their existence there is no question.

2. Presence of consciousness implies the presence in the brain of the mentioned processes, and conversely.

3. Qualitative and intensive changes in consciousness imply and are implied by corresponding changes in the brain, which are respectively qualitative (*i. e.*, structural) or quantitative (*i. e.*, functional).

Must we not then admit that all life is an example of causality between mind and matter, and that when the parallelist denies this causality he repudiates the canons of inductive science? If it is really true that in life these two great orders of events—the mechanical and the teleological—fulfill all the conditions of reciprocal causality, it becomes a scientific impossibility to regard them as parallel solely because they appear to us incommensurate.

But suppose it be objected that this criticism is based upon a naïve misapprehension of the parallelistic thesis, the whole force of which depends upon the truth of the (purely apparent) causality between mind and matter which we as critics of parallelism have been at such pains to establish. I answer that the distinction between actual causality and the *illusion* of causality exists only when the conditions for inferring the causality are *imperfectly* fulfilled. When two things act *to some extent* as though they were causally related, then we may with propriety hold that the causality can be either seeming or real; but when *all* the conditions for a valid inference of cause are fulfilled then we can no longer entertain the possibility of a causality merely seeming the result, let us say, of a parallelism between the two events. In short, although parallelism might very well counterfeit a causality only *partly* verified, it would be impossible, or at least infinitely improbable, that a perfectly verified causality was the result of a parallelism, however elaborate, at least in a world in which there was any approach to what we call the 'uniformity of nature.' The truth of this can be seen by a brief examination into what it is that gives cogency to the third canon of inference stated above. When two events are observed to be present and absent together at once, there is a certain probability that they are causally connected; when they are observed as mutually present and absent on ten occasions,



we have a probability of their being causally connected which is ten times as great as the first; and so when they are present and absent together on an infinite number of occasions there is an infinite probability—*i. e.*, a certainty—of a real causal connection between them. Now any continuous quantity can be regarded as containing an infinite number of discrete quantities; and so a continuous variable—say, a curve—contains an infinite number of separate variations: hence when two continuously varying processes of events agree with each other throughout the probability that they stand in a causal relation is infinite and equivalent to certainty; and the third rule of inference is simply the *continuous* or infinitely repeated verification of the conditions of the first and second rules.

If, then, the canons of inference hold good, parallelism must be rejected.<sup>1</sup> Mind and matter do really influence one another wherever there is life; indeed life may be roughly defined as the single known condition for the existence of an order of events which is neither purely teleological nor purely mechanical, nor yet a harmonious parallelism of both, but rather a perfect interpenetration of mechanism and finality, of solid fact and airy meaning.

The fact of correspondence could, on account of the 'equationlessness' or complete lawlessness of the curves, never be referred to a parallelism—*i. e.*, to a prearrangement on the part of Nature or God—but only to a continuously operative causality. It is this latter type of connection that unites the mechanical and teleological orders of experience.

Notwithstanding the various objections which have so often been urged against psychophysical parallelism, the theory has taken such firm root and in such high places that I venture one more attempt to show the difficulty inherent in the conception.

<sup>1</sup>Two phenomena or two sequences of phenomena which fulfilled only the first and second criteria of cause—which implied merely the presence and absence respectively of each other—might very well be parallel, and only *appear* to be causally related. They could be symbolized thus:  $\begin{matrix} A\text{---} \\ B\text{---} \end{matrix}$ . On the other hand, two sequences which in addition to the characters of the above pair exhibited the phenomenon of *concomitant variation*, could only be regarded as really causally connected; and they could be symbolized by two concomitant but infinitely variable or 'equationless' curves.

A mechanical world in so far as it is mechanical is a world of quantities; and a teleological world in so far as it is teleological is a world of qualities. The one is *continuous*, the other is *discrete*. Given any two pure quantities, and there is also given an infinite number of intermediate quantities; while, on the contrary, between any two pure qualities there is given no intermediate excepting the quite formal unity of the perceiving consciousness. Of course, we never get pure quantities in the physical world, nor pure qualities in the world of mind. The science of Mechanics cannot be reduced to geometry, because the discrete positions and masses of the moving bodies are just as important factors as the continuous distances through which they move. And analogously we can never reduce epistemology or logic to a completed system of mutually exclusive or absolutely discrete concepts or categories. It was this latter ideal which Hegel thought he had attained in his Logic; and the attempt to express all mental life as a dialectical product of the 'Idea and its Other,' of identity and difference is as grand and as impossible as the complementary attempt to express all material bodies as the product of a continuous space or continuous ether. Nevertheless, in spite of the failure to separate things into pure qualities and pure quantities, it remains true that qualities as such are related in only two ways, while quantities as such are related in an infinite number of ways. Any quality  $A$  has its formal opposite not- $A$ , while of the quantities two only stand to each other as opposites, viz., zero and infinity; and the only way in which a quantity can be 'turned into its opposite' (without introducing any qualitative determinant, such as difference in sign) is by combining it with zero or infinity. To make a change in a quantitative system which should be adequate or parallel to a change in a qualitative system would involve an infinite increase or decrease of the energy or quantitative value of the system.

Take now the typical case of a qualitative change. I make the successive judgments: 'Man is mortal,' 'Man is not mortal.' The conceptional universes of consequences which follow respectively from each of these assertions are mutually exclusive—are qualitatively different. Now, according to the parallelist, there were two material systems accompanying these two men-

tal systems, and when I changed from judgment number one to judgment number two, material system number one made a corresponding change and became material system number two. And as the two changes were parallel to one another, the measure of the change—*i. e.*, the ratio of the second state to the first—must be the same in both cases. Consequently the change in the material or quantitative universe must have been infinite, as otherwise the differences could not correspond with nor parallel one another. But surely it is preposterous to suppose that the physical world makes these tremendous jumps in quantity whenever any one divides a universe of discourse into *A* and not *A*. And yet if we leave it to itself, cut off from any causal connection with and yet exactly corresponding to the world of concepts, that is just what must happen. But, as a matter of fact, what does happen in the physical world when there is a change in the conceptional world? I say 'Man is mortal,' and a certain brain-state and consequently a certain modification of the entire material universe are present at the moment of my utterance. I now change the judgment to 'Man is not mortal,' and simultaneously the material universe changes also, but changes almost infinitesimally instead of infinitely. The addition of the word 'not' is accompanied by the tiniest and most insignificant of changes in the world of matter, although it completely reverses my conceptional universe. If, instead of the word '*not*,' I had put a nonsense syllable of three letters, the physical change would have been equally great, while the conceptional change would have been absolutely *nil*. Indeed, if we are to continue to hold to the parallelistic theory, we must once for all give up the idea that the energy of the effect varies quantitatively with the cause. The change from a brain state accompanying the rational affirmation of a judgment to the brain state accompanying its negation is primarily a qualitative change, and as such can only find its sufficient reason, its *vera causa* in the qualitative—*i. e.*, conceptional—change that occurs in the mind. In short brain-states, like all the other quantitative or continuously changing elements of the mechanical world, no matter how cunningly arranged by a Leibnizian god, would be of themselves infinitely lacking in the power to



keep up with the absolute or qualitative changes involved in reasoning.

Every one will grant that psychology has for its object matter a process that has a double aspect. Each psychical event is both a *fact* and a *meaning*. Must we not, however, go even further, and admit, on the strength of our criticism of parallelism, that psychical events do not have even these two aspects purely distinct? Neither the *fact*—*i. e.*, the actual presence of the feeling—on the one side, nor its *meaning* and significance on the other, are able to be understood apart from one another. There is no fact of consciousness which does not, even in its mere brute presence, imply some meaning; while, again, there is no meaning or judgment so universal and so thoroughly ideal that its utterance by an individual does not to some extent particularize or individualize it. In short, it is not only true that norms and facts influence one another, but each norm is itself to some degree tainted with fact; and conversely, each fact is *qua fact* to some degree dignified with an ideal significance. Mediateness and immediateness are, in spite of their opposed and incommensurate natures, matters of more and less.

But, one may well ask, What has all this to do with the conception of a substantial soul? Suppose all consciousness is a continuous sequence of events of such a nature that mechanical and teleological orders of existence persist both collectively and individually in exchanging salutations and in deferring to each other's laws; grant that this insufferable state of things exists, and that on account of it psychology is impossible for the simple reason that a psychological law could be nothing but a preposterous blending of physics and epistemology: does it therefore follow that we should add to the confusion occasioned from a mixture of two incommensurates by proceeding to introduce an unknowable *tertium quid*? Because matter and mind interfere with each other in consciousness, are we to invoke a substantial soul as an aid to comprehension? I answer, yes; it is logically necessary to call in a third thing, a thing, moreover, about which we know very little, precisely in order to explain this interpenetration of the material and the teleological which is the everyday mystery of consciousness. Our justification for this step is the following axiom:

*When two things, A and B, are related to each other, there is implied by that relation the existence of a third thing or medium, X, whose nature is 'individual' or 'simple' and different from either, though homogeneous and commensurate with both.*

When *A* and *B* are 'attributes' then *X* will be 'substance.' The paradox implied in the notion of substance is simply this—a substance (*X*) cannot be a phenomenon or attribute (*A*, *B*, *C*, ...); it cannot add to the qualitative content, to the 'whatness' of the object, and is, therefore, in one sense nothing; while on the other hand, inasmuch as the attributes or phenomena cannot exist or be understood in themselves, either singly or collectively, it follows that they must have their real being in a substance which underlies and connects them. To put the thing in the modern and expressive terminology of Mr. Bradley, every object is composed of a 'that' and an indefinite number of 'whats.' *What* the 'that' is we cannot easily say for the very reason that it is not, properly speaking, a 'what,' but a 'that'; it is the subject of which qualities can be predicated and which de facto is not itself an ordinary predicate. But it by no means follows; that the inherent difficulty of describing the 'that' justifies a denial of its existence. Each 'what' carries on its face its own inadequacy to stand alone or to explain its relations to its fellows; and the reality of the 'that'—the reality of substance—is not only given immediately in experience, but can be indirectly or mediately inferred by reflection upon the imperfection and unsubstantiality of the attributes.

Now mind by itself cannot explain matter nor matter explain mind; therefore the proved fact of their relation can only be understood by regarding them as attributes of a substance, a soul, whose nature if understood would explain their mysterious connection. As said above, the soul could be provisionally defined as "that which made final causes efficient in the material world; and conversely, as that which enabled efficient causes to produce teleological effects or meanings."

We have now, I think, set forth all the conditions of our problem and the negative or indirect reasons for solving that problem by the theory of soul-substances. From the outset we

have tacitly assumed that 'mind' and 'matter' were synonymous respectively with 'teleology' or 'finality' and 'mechanism' or 'efficiency'; the reasons for this assumption being the advisability of keeping clear of the issue between cosmological realism and idealism. All *esse* may be *percipi* or it may not; for our purposes the important thing was to distinguish between *percipi* which was mechanical and *percipi* which was *teleological*. Consequently the most ardent Berkeleyan would have no right to rebel against speaking of matter as real if the term matter were simply used as a generic term for all those events and sequences of events which conform to mechanical or factual as distinguished from teleological or normative laws.

After having pointed out the equal reality and mutual incommensurability of these two orders of experience, together with the indubitable fact of their *apparent* influence upon one another throughout the domain of life or consciousness, we next considered the five methods of solving this problem of a seeming relation between two incommensurates. These methods were:

1. Absolute teleology, which denies the existence of material or mechanical sequences.

2. Materialism, which denies the existence of mental or teleological sequences.

3. Occasionalism, which admits the reality of both mind and matter, but explains their interaction by a series of miracles.

4. Parallelism, a doctrine which, like occasionalism, admits matter and mind as realities, but explains their apparent interaction as an illusion produced by a naturally or supernaturally established harmony.

5. The theory of the soul—a theory which holds that mind and matter are the two real aspects or attributes of a single substance in virtue of which they can and actually do interpenetrate.

Thus far we have accomplished half of our appointed task: we have shown the inadequacy of the two attributes of mechanism and teleology to explain their own blending in the concrete sequences of consciousness; and we have, therefore, been driven to infer the existence of a *tertium quid*—a soul-substance—which so far has only been—*functionally* defined—*i. e.*, defined in terms of what it can do. It now remains to determine, so far



as possible, what the soul-substance is, and until this is done we have no right to distinguish the soul conceived as substance from the soul conceived as *ding an sich*. Indeed, the valid and positive notion of substance differs from the negative and useless notion of the thing-in-itself solely in virtue of its complete fulfillment of a set of requirements which are only partially fulfilled in the concept of a thing-in-itself. These requirements are three in number, and may be stated as follows:

1. A substance must, in order to be defined, possess an intelligible essence or character—a mode of its own—distinct from the attributes.

2. This 'substantial form,' or essence, must be related to all the attributes as genus to species.

3. And also to each attribute as species to genus.

The significance of these requirements may be best illustrated by observing what results if we omit any one of them. Suppose we omit the first requirement; we are left with the undefined that, the *ding an sich*. It is the genus of the attributes, because all qualities have the character of being presented or of existing; while, again, existence is itself a 'somewhat' different from any or all qualitative determinations. That which constitutes the difference between the hundred real and the hundred possible dollars is not to be denied, although it cannot be described. 'Existence' is then both genus and species; and to find the form of existence the 'whatness' of the 'thatness' would be all that was necessary to change the conception of the *ding an sich* into the conception of substance.

Suppose, however, that instead of omitting the first requirement we omit the second. We should then have in place of a generic substance simply an additional and merely specific attribute. If, for example, it is said that the soul is neither material nor ideal, we may know in advance that it can never explain as its logical derivatives the attributes of materiality and ideality. In excluding the attributes from its own ungeneric nature it excludes also the possibility of explaining the relation between them. It is the omission of this second requirement that marks the imperfection of such systems as that of Thales. Water is not the *sumum genus*, consequently it cannot be the absolute substance.

Finally, if we attempt to get along without the third requirement; if we say that the soul is nothing but mind and matter; that it has no new quality, no differentia of its own—we have pure phenomenism or positivism, a doctrine which holds that the substance of a thing is merely the sum of its attributes. In short, to omit this third requirement is to give up the conception of substance altogether.

These three requirements for a valid or adequate conception of substance are implicitly recognized in all metaphysics; but in St. Thomas's conception of God and in Hegel's conception of the Begriff,<sup>1</sup> the recognition is clear and explicit, so much so that one might almost say that the problem of the Absolute appears for these thinkers as neither more nor less than the problem of substance as above defined.

We are now in a position to understand the nature of the last half of our task. Once having justified our right to believe in a soul-substance as existing, we must further proceed to define our ding an sich, to fill in the blank which is as yet only determined functionally—as an unknown locus of known relations. In a later paper I hope to show that the moral consciousness affords a valid and unique instance of what is required for the adequate definition of substance; and that in moral sequences we find a type of causality that is at once mechanical and teleological, while yet differing from both exactly as the common *limit* of two separate series differs from those series.

In this paper I have attempted only a demonstration of the existence of a genuine need for a soul-substance from the point of view of descriptive psychology, and a vindication of the right to hypostasize such a conception as soon as it shall be properly defined.

<sup>1</sup> See particularly Hegel's chapter on *Kraft und Verstand*, in his *Phänomenologie des Geistes*.

## THE REACTION TIME OF THE EYE.

BY ASSOCIATE PROFESSOR RAYMOND DODGE.

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The reaction time of the eye has an unique significance for the physiologist and the psychologist, partly on account of the peculiar half unconscious character of the eye movements and the consequent general bearing upon the problems of reaction, but more particularly because every change of direction of the line of *regard* is fundamentally an eye reaction.

For the proof of this proposition I am compelled to refer the reader to the original discussion,<sup>1</sup> where will be found as well the proof of the fact which makes it important, viz.: In every change in the point of regard in a complex field of vision the eye distinguishes nothing during the actual movement. That this statement seems to be contradicted many times daily suffices to explain how the erroneous conceptions of the eye movements could have remained unchallenged so long and how so many false interpretations could have clustered around them.

Important as these facts are for the general theory of physiological optics, they have as well a very interesting bearing on the physiology of the eye in reading, whose problems gave the first cue to their discovery. On the one hand, they explain the regular alternation of pause and movement in the reading eye, to which attention has recently been called almost simultaneously on both sides of the Atlantic.<sup>2</sup> On the other hand, the experimental demands in the apparatus for the study of reading can only be satisfied when the eye reaction is understood and measured.

<sup>1</sup>Erdmann und Dodge, *Psychologische Untersuchungen über das Lesen*. Halle, Max Niemeyer, 1898. S. 68-76.

<sup>2</sup>Op. cit., Kapitel I. E. B. Huey, Preliminary Experiments in P. and P. of Reading, *Am. Jour. of Psy.*, July, 1898. P. 583.



The most favorable conditions for the exposure of letters, words, etc., will naturally approximate the conditions of the normal pauses of the eye in reading. Simplicity in the experimental conditions, however, demands that indiscriminate change in the point of regard should be eliminated.

In all previous experimental work where exclusion of the eye movements was essential, recourse was had to illumination of very short duration.

Helmholtz, Aubert and others used for illumination an electric spark, whose duration Helmholtz estimated at a fraction of a thousandth of a second. Under less exacting conditions it has been customary to use intervals varying from 1 to 10 thousandth of a second, assuming that the movement of the eye during such small intervals of time could be disregarded.

The fact above referred to, that the eye can perceive nothing in a complex field over which it sweeps during the movement itself, evidently prevents any compromising effects arising from a movement begun before or simultaneously with the exposure; and makes it possible to use an exposure whose duration is limited only by the time which would permit the eye to begin and carry out a new movement after the exposure had begun. Such a lengthening of the exposure as this would permit, not only reduces the mechanical difficulties of exposure apparatus, but has the more important advantage of adequate stimulation. Equally desirable in view of the interest that has suddenly sprung up in the psychology of reading is the adoption of a generally accepted interval of exposure, by whose use the results of different investigators may be made comparable.

It is in the interests of such a standard exposure that I have thought it best to present results of measurements in a short paper independent of their general theoretical bearing.

The strained conditions and consequent inevitable functional disturbances which all attachments to the eye must produce, render all results with such appliances open to serious criticism; while the general discomfort of such methods necessarily limits their applicability. These mechanical difficulties led to the discovery of a purely optical method which, in spite of the rather clumsy apparatus at our command in Halle, was

so satisfactory that I have used it, with some modifications of detail, in the following measurements.

In general the method is as follows: A stimulus  $f$ , capable of variation and accurate measurement in duration, is thrown on the blind spot of an eye at rest. Since any slight movement of the eyes will bring it into view, the natural movement, which follows some peripheral stimulation  $e$ , will bring it into view, provided its duration is sufficient. The necessary duration of the stimulus  $f$ , which will just permit the observer to see it after the cue for movement is given, is evidently the reaction time of the eye.

As the aim of the Halle measurements was solely a negative one, viz., to determine a maximum safe exposition time, we did not feel warranted at that time in constructing the special apparatus required for a more accurate measurement.

In designing a pendulum tachistoscope for some general work at the Wesleyan University Laboratory I incorporated the special features necessary for satisfactory measurements of the eye reaction. The essentials of the apparatus are as follows: A heavy second pendulum resting on broad knife edges and swinging through  $90^\circ$ , carries two large disks on its axis, one of which is temporarily fixed, while the other may be rotated and clamped at any point. The smaller disk has a radius of 12 inches; the larger, of 16.5 inches.

One inch from the periphery of the smaller circle is drawn a fine black arc, concentric with the disk, which, when viewed through a blackened tube and a horizontal slit in a black screen, placed immediately in front of the disks, appears as a short vertical line as long as the horizontal slit is wide. This serves as the primary point of regard, and would not change its appearance, even during the oscillations of the pendulum, if the arc were long enough. The arc is, however, so short that before the pendulum has completed  $\frac{1}{6}$  of its excursion, after release, the primary point of regard disappears; and at the same moment there appears a similar short line, which serves as the peripheral stimulation, made by a concentric arc  $\frac{1}{8}$  inch from the periphery of the smaller disk. The excursion of the eye as it looks from the disappearing primary point of regard to the

appearing peripheral stimulus, is a double sine of  $\frac{7}{8}$  inch in length, corresponding to an arc of about  $3^\circ$ , when the axis of rotation of the eye is 16 inches distant from the disks. Four inches from the primary point of regard in the larger and movable disk is cut a circular slit,  $\frac{1}{8}$  inch wide, concentric with the disk. Through this slit a strong light corresponding to  $f$  falls on the blind spot when the eye is at its primary position, and becomes visible only after the eye moves.

The reaction time of the eye is evidently measured by the necessary length of the arc corresponding to the stimulus  $f$ .

In the Wesleyan apparatus this is measured in .01" by direct reference to the vibrations of a tuning fork, registered on the periphery of the inner or fixed disk.

The whole apparatus has a delicate levelling adjustment, and the oscillations of the pendulum are consequently constant.

Since the release of the pendulum, although practically noiseless, is nevertheless usually perceptible as a slight jar, the result of the eye movement is not registered on the first oscillation of the pendulum, but always on the second. The observer only answers the question whether the bright stimulus was seen or not seen. The record is ignored in the calculation if the observer was conscious of false movements or imperfect fixation.

The results of the experiments are given in the following tables. Observer  $\mathcal{Q}$  is Mr. E. M. Quittmeyer, class of 1899, Wesleyan University, an honor student in philosophy.<sup>1</sup>

Observer  $D$  is the writer. A long series of preliminary experiments gave for each experimenter the probable upper and lower limits of variation from the true reaction mean.

The succeeding experiments were made in blocks of ten, in which ' $f$ ' was given an arbitrary duration. A sufficient number of such blocks of experiments was made to give  $D$  100 experiments each, when ' $f$ ' had the values 160, 170, 180 and 190  $\sigma$ , after the cue for movement had been given.  $\mathcal{Q}$  made 50

<sup>1</sup> Mr. Quittmeyer's services in the experiments reported are more than the faithful work of a careful observer. Many of the details of illumination are due to his suggestion, and if a number of circumstances had not conspired to prevent it, it was intended that he should make the report.



experiments each when ' $f$ ' had the values 150, 160, 170, 180 and 190 $\sigma$ .

The lower limits were determined by a larger number of trials, as they were of peculiar importance. No attempt was made to determine accurately the upper limits beyond which no negative answers were given. As is general in reaction studies, the maximum records have very little meaning, owing to the complex conditions which determine them.

In a very large number of experiments  $\mathcal{Q}$ 's lower limit for ' $f$ ' was 140 $\sigma$ .  $D$ 's lower limit for ' $f$ ' was 150 $\sigma$ .

$\mathcal{Q}$ .

$f =$	150	160	170	180	190
seen =	2	6	31	41	44
not seen =	48	44	19	9	6

The mean reaction time evidently cannot be reckoned as an arithmetical average of all the reactions, but must be given as that value which ' $f$ ' must have in order that just as many reactions should lie above as below it. This will be the case when

$seen = \frac{n}{2}$ . From the above data it is evident that the mean reaction of  $\mathcal{Q}$  lies between 160 and 170, and must have a value of 167.6 $\sigma$ .

$D$ .

$f =$	160	170	180	190
seen =	15	41	58	73
not seen =	85	59	42	27

The mean reaction time of  $D$  lies consequently between 170 and 180, and has a value of 175.8 $\sigma$ .

It is evident that the time interval thus measured does not correspond exactly with the reaction time as ordinarily understood. In addition to the peripheral and central processes, which theoretically make up the true reaction time, the eye reaction includes two other processes: first, the slight movement necessary to bring the light on to a sensitive part of the retina; and secondly, the duration of that stimulation necessary to produce a sensation.

Of these the second is well known to be only a fraction of a thousandth of a second, and may be disregarded.

The first is apparently more important; but, according to the known rapidity of the movement of the eye, must be much smaller than the mean variation, since the eye moves through  $5^{\circ}$  in  $10-15 \sigma$ ;<sup>1</sup> while the stimulus 'f' touches a sensitive part of the retina and appears as a bright band before the eye has moved  $1^{\circ}$  in  $30'$ . Moreover, this small lost movement is scarcely to be obviated by any mechanical means, since it represents an excursion of a point on the cornea of about .011 inch, an excursion which even the most delicate mechanical attachment could scarcely reduce.

If we attempted any correction for these constant errors, it would reduce the mean reaction time about  $6\sigma$ .

*Q* to  $162\sigma$ .

*D* to  $170\sigma$ .

The minimal reactions, however, signify more for the determination of experimental conditions than the mean reaction time. In view of the foregoing measurements, I feel justified in making the general recommendation that whenever practicable the exposures in the psychology of reading, as well as elsewhere, where a maximum constant exposure is desired, which is still too short for a change in the primary point of regard, be given a uniform duration of  $.1''$ .

Undoubtedly a slightly longer exposure might be used in most cases; but, in general, I believe it to be advisable to use a duration so small that it may remain constant, while the ease with which  $.1''$  can be measured and controlled is perhaps an added argument in its favor.

There is a difficulty in the above method that makes it useless for unpractised observers; this is the general inability to keep the eye fixed when expecting a peripheral stimulation. It, however, is the most accurate optical method when one has become master of the eye movement, since a very slight movement is enough to bring the bright light into view.

<sup>1</sup>Lamanski, Bestimmung der Winkelgeschwindigkeit der Blickbewegung. *Pflüger's Archiv f. d. g. P.*, II., p. 418-422. Dodge, Anhang zu psychologische Untersuchungen über das Lesen.

A much simpler method is recommended when the aim is merely to control the time of exposure, or to demonstrate to a class that within the given exposure no movement of the eye takes place. Under these conditions it is only necessary to expose two letters for the interval in question, far enough apart, so that when either one is fixated the other is not recognizable. If one is exposed at the primary point of regard, only a movement can make the other visible; and unless it is possible, after repeated trials, to see both, the interval of exposure must be too short for the movement in question. If the attempt were made to determine the mean reaction time by this method, the corrections for the time of movement of the eye would assume considerable importance, and would demand special measurements.



## A STUDY IN THE DYNAMICS OF PERSONAL RELIGION.

BY PROFESSOR GEORGE ALBERT COE.

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Up to 1891 the history of psychology contained no example of the systematic application of empirical methods of research to the religious phenomena in the midst of which we are living. Since that time, however, President Hall and several of his pupils, notably Professor Starbuck, have published significant contributions upon certain branches of this subject.<sup>1</sup> The chief result is the establishment of definite correlations between religious experience and adolescence. The conclusion most thoroughly worked out is that the period of greatest religious transformation for both males and females is, in general, the period of physical transformation from childhood to adult life. Another important generalization is that what is called conversion is only one of many forms in which a normal adolescent religious change clothes itself. From the case in which childhood religion grows mature without special agitation, to the cases in which conversion takes place amid volcanic outbursts of emotion, there is every grade and variety of disturbance, though with the same general outcome when adolescence is over.

These differences have never been satisfactorily accounted for, and indeed the question has hardly been raised except for the sake of hazarding a guess. "The explanation of sudden conversions," says Bain, "is no doubt to be sought in some overpowering impression upon the mind that supplies a new and energetic motive to the will, thereby initiating a new line of

<sup>1</sup>G. Stanley Hall: *The Moral and Religious Training of Children*, *Ped. Sem.*, I., 196ff.; E. D. Starbuck: *A Study of Conversion*, *Am. J. Psy.*, VIII., 268ff., and *Some Aspects of Religious Growth*, *Am. J. Psy.*, IX., 70ff.; A. H. Daniels: *The New Life*, *Am. J. Psy.*, VI., 61ff.; J. H. Leuba: *A Study in the Psychology of Religious Phenomena*, *Am. J. Psy.*, VII., 309ff. See also Luther Gulick: *Age, Sex and Conversion*, *Association Outlook*, Dec., 1897.

conduct. \* \* \* Such changes occasionally happen, but not without terrific struggles, which prove how hard it is to set up the volition of a day against the bent of years."<sup>1</sup> Here all sudden conversions are lumped together as though they were all of one type; all are declared to be accompanied by terrific struggles, and all are explained by a single circumstance.

Equally incomplete is the explanation of Nietzsche when he snarls at Christianity because, as he thinks, it is not in contact with reality. He declares that Christianity cultivates "an imaginary psychology (nothing but self-misunderstandings, interpretations of pleasant or unpleasant general feelings,—for example, the conditions of the *nervus sympathicus*,—with the help of the sign-language of religio-moral idiosyncrasy,—repentance, remorse of conscience, temptations by the devil, presence of God").<sup>2</sup> Doubtless this statement contains some truth; yet it is as inadequate to explain the broad variety of experiences occurring under Christian influences as it is to explain the whole sphere of perception, normal and abnormal together.

Here and there a more probable hint has appeared. Thus, Havelock Ellis makes the remark that a sudden explosion of suppressed hypnotic centers is 'the most important key to the psychology of conversion.'<sup>3</sup> Leuba, speaking of the conversion of John Wesley, throws out this hint: "An interesting remark can be made here concerning the influence of suggestion: it is as the change that God works in the heart is being described that the very same transformation takes place in Wesley."<sup>4</sup> The same writer also remarks that "the particular forms in which affective states dress themselves are functions of the intellectual atmosphere of the time."<sup>5</sup> This is undoubtedly a hopeful clue; but when the same writer goes on to affirm that joy "is never altogether wanting, and is always violent during the first hours or days that follow,"<sup>6</sup> he misses an essential fact. Starbuck was, I believe, the first writer to give adequate recognition, with

<sup>1</sup> *Emotions and Will*, 3d ed., N. Y., 1876, 453f.

<sup>2</sup> *Antichrist*, Works, N. Y., 1896, XI., 253.

<sup>3</sup> *Man and Woman*, 2d. ed., Lond., 1898, 292.

<sup>4</sup> *Psy. of Relig. Phenomena*, *Am. J. Psy.*, VII., 340.

<sup>5</sup> *Id.*, 357.

<sup>6</sup> *Id.*, 351.

empirical data, to the marvellous varieties that cluster about such terms as conversion. He advanced a step toward their explanation, also, when he showed that something more than a conscious exercise of either intellect or will was central in adolescent conversions.<sup>1</sup> He came still closer to the problem when he found imitation, example, etc., present as motives in 15 per cent. of his cases.<sup>2</sup> Nevertheless, a moment's reflection upon the capacity of the average person to tell the truth regarding his own motives will reveal some insecurity in these results and bring up the whole question of the best method of getting at the facts. Another clue emerged in Starbuck's admission that 'much depends upon temperament.'<sup>3</sup> Yet this clue has never been followed up. In fact, this same writer, commenting on some of his cases, confesses that some religious experiences 'seem to come in the most unaccountable ways.'<sup>4</sup> Now, I venture to believe that, if we could secure sufficiently full information as to the conditions, every one of these cases could be accounted for.

The present study, accordingly, is an attempt at a more complete analysis of individual cases than has heretofore been attempted. If we can lay bare the factors in a few cases that are fully accessible, the information thus acquired may afterward be of service in interpreting the broader differences of sects and religions. To forestall misunderstandings, it may be well to state at this point that the phrase 'the dynamics of personal religion' is not intended to convey, and cannot properly convey, any metaphysical meaning. The problem concerns the concomitance of certain groups of phenomena and nothing more. The question of divine influences in the mind of man and in history must stand in exactly the same position at the end of such a study as it does at the outset. Any one who prefers to do so is at liberty to interpret every result as a description of the mode of God's working in the world. Nothing in the study itself has any logical tendency to undermine this belief.

Our task consists in looking for coördinations between specific inner states and tendencies and specific external circum-

<sup>1</sup> *Am. J. Psy.*, VIII., 292.

<sup>2</sup> *Id.*, 281.

<sup>3</sup> *Am. J. Psy.*, IX., 110.

<sup>4</sup> *Id.*, 81.



stances. We are confronted at the outset with the problem of how to secure adequate data. In previous studies in the psychology of religion reliance has been placed upon the *questionnaire* method, which consists in securing from many persons written answers to printed questions regarding their experiences. This is doubtless a satisfactory method of securing certain facts; but our inquiry calls also for information which the writers of such papers ordinarily do not and cannot possess. Accordingly, my question list was so constructed and the answers so used as to make the latter not merely a record of certain facts, but also a reflection of the personality of the writer. These answers were also supplemented in various ways: First, personal interviews were had with a large proportion of the persons examined. The cross-questioning which these interviews made possible not only cleared up doubtful points in the papers, but also elicited many new and important facts. Second, a large proportion of the subjects were placed under careful scrutiny by myself and others, with a view to securing objective evidence as to temperament. These observations were guided by a carefully prepared scheme of temperamental manifestations. Third, interviews, based upon the same scheme, were had with friends and acquaintances of certain of the persons under examination. Finally, in order to get at the facts of suggestibility, hypnotic experiments were made upon all the important cases that were accessible. Fuller description of some of these methods of gathering data will appear later.

The number of persons examined was 74. Of these, 50 were males, and 24 females. Nearly all are college students who are healthy in both mind and body and have had the advantage of positive moral and religious training. Nearly all are just past, or are just passing out of, the adolescent period. The average age of the men was 24.7, and of the women (one case, 65 years of age being excluded), 22. Though this narrows the range of observation of temperament chiefly to the formative years, it brings these compensating advantages: the nearness of the chief religious experiences, the habit of introspective analysis specially characteristic of adolescence; and the naïve and spontaneous expression of personal facts. Again, a

large majority of the subjects were brought up under the influence of the Methodist Church, which lays great stress upon personal religious experiences. The opportunity to study the effects of suggestion was therefore excellent. In general, in spite of some limitations of the field of observation, the differences in both type of religious experience and type of mental organization were many and great. The accessibility of the material, moreover, and the opportunity to observe, ask questions and experiment repeatedly—these easily outweigh all the limitations. It is, indeed, not easy to see how a more satisfactory set of cases could be secured.

Let us now turn to the variations in religious experience from individual to individual. The chief one, and the one with which this study is occupied, is in the degree of abruptness of religious changes. One person reaches a higher plane of the religious life by a process of development scarcely ruffled by excitement; another attains the same state by passing through a mental cataclysm. Some elements of the explanation lie on the surface. For instance, the striking changes occur chiefly among denominations that definitely aim to secure them. Furthermore, these denominations have discovered many of the conditions favorable for producing such changes, such as a particular type or particular types of preaching and appeal; the use of music, particularly of certain kinds; intense social feeling fostered by meetings; the provision of external acts, signs or instruments—such as rising for prayers or to indicate decision, going forward, the altar, the mourners' bench—all of which evoke expression of the inner state and thereby intensify it; and, finally, the fitting of all the conditions together so as to produce a climax or a series of climaxes. What we need to determine next is the mental mechanism to which all this appeals, and also the reason why it fails of its result in many cases in which the conditions give hope of success. For it is a matter of everyday knowledge in revival churches that of two persons brought up in the same manner, and apparently meeting the same conditions, one may experience a brilliant conversion, while the other may experience no such states at all.

In order to secure definite ground for an hypothesis on this

point, the persons under examination were divided into two groups: those who had experienced a marked transformation, and those who had not. The fact that religious changes show all degrees of rapidity and of emotional intensity made it necessary to draw this line with great care. In every case, therefore, which the papers left in doubt, a personal interview was had. Striking transformation was defined to the subject as a profound change, which, though not necessarily instantaneous, seems to the subject of it to be distinctly different from a process of growth, however rapid. As soon as the subject grasped this definition, he was requested to classify himself, and his decision was accepted as final.

In the second place, a cross division was made on the basis of predisposition of the mind toward such experiences. Let us call this basis 'expectation of transformation.' A careful study was made of the home influences, the general church environment and the specific circumstances surrounding the religious awakening. Here, again, much had to be drawn out by personal interviews. A considerable number of the subjects had been taught that one who has been religious from childhood does not need a marked conversion. Others indicated that their thoughts were never turned strongly in the direction of conversion. All such were classed as not expecting a transformation.

Combining these two modes of division we secure two positive classes for minute study—those who expected a transformation and experienced one, and those who expected, but failed to experience. In the working out of this scheme a third division was found necessary in order to tabulate cases in which these two classes overlap; for a number of persons who experienced a marked transformation were unsatisfied and sought for something more without securing it, while others were satisfied, but sought for a still higher experience in vain.

To do justice to the case, it is necessary to note the caution that was exercised in making the classes. For example, in the class of those who expected but failed to experience there are included none who did not distinctly declare that they sought an experience without finding. Most, if not all, of them had subsequently learned how to be religious in spite of this disap-



pointment, yet the struggle in a large proportion of the cases had been acute.

From theology the suggestion may come that possibly these persons did not really surrender themselves to God. But an *a priori* assertion, or rather guess, like this ought to have little weight as against the following: All the evidence of the facts goes to show that those who were disappointed had put themselves in the same attitude of will as the others: furthermore, a large majority of the disappointed ones are now living positively religious lives in the evangelical sense of religious.

These two classes were next examined with respect to temperament. This was a laborious and perplexing undertaking, both on account of the unsatisfactory treatment of temperament by writers on psychology, and because of the complexity of the facts to be observed. It is easy for any psychologist to give a classification of temperaments that can be brilliantly illustrated from history, but it is quite another thing to devise a method for [grouping the persons one comes in contact with. At the present day two classifications are employed. The first, represented by Wundt<sup>1</sup> and many followers, is based upon the fact that one's mental processes may vary in both rapidity and strength. This basis yields four temperaments which correspond fairly well with the traditional fourfold division. The rapid-strong temperament corresponds to the choleric, the rapid-weak to the sanguine, the slow-strong to the melancholic, and the slow-weak to the phlegmatic. On the other hand, French writers for the most part adopt a qualitative basis—that is, classify according to the faculty or function that predominates. This is true of Ribot,<sup>2</sup> Queyrat,<sup>3</sup> Levy<sup>4</sup> and Fouillée.<sup>5</sup> Perez, however, retains liveliness and intensity as the basis.<sup>6</sup> This is not the place to discuss the general topic of temperament, nor to go into the merits and defects of these two plans of classifica-

<sup>1</sup> *Grundzüge der Phys. Psy.*, Leipzig, 1893, II., 519ff. See also Lotze: *Microcosmus*, Vol. II., Bk VI., Ch. II.; and Ladd: *Els. Phys. Psy.*, N. Y., 1897, 572ff.

<sup>2</sup> *Psy. of the Emotions*, London, 1897, 388ff.

<sup>3</sup> *Les Caractères*, Paris, 1896, 36ff.

<sup>4</sup> *Psy. du Caractère*, Paris, 1896, 182ff.

<sup>5</sup> *Tempérament et Caractère*, Paris, 1895, 20ff.

<sup>6</sup> *Le Caractère*, Paris, 1892.

tion. It is sufficient to remark that a practical scheme must provide at least a fairly definite mode of describing any and every person whose individuality is sufficiently marked to be noticeable at all.

Wundt's scheme was first employed, but it quickly proved itself inadequate to give a genuine characterization of many distinctly marked individualities. This was especially true when Wundt's classes were interpreted as if they were identical with the traditional four temperaments. The qualitative plan was next tried; but, while it supplemented the other, it proved inadequate taken by itself. In the interest of a workable scheme, therefore, it was found necessary to combine the two modes of division. The result was not a new classification of temperaments, but what we may call a scheme of the constituents of temperament. The mode of procedure now consisted, first, of judging whether sensibility, intellect or will was the most prominent faculty; next, of finding the second in prominence; then of estimating the place of each of the three faculties in respect to promptness and intensity. For each subject, in the end, there were three descriptive designations, as, for example, prompt-intense intellect, prompt-weak sensibility, prompt-weak will; and these three were arranged in the order of prominence.

The sources of evidence for temperament were the same as those employed by the writers just named, namely, permanent modes of action, of speech and of point of view; permanent interests; likes and dislikes; habitual social interactions, etc., whether observed and recorded by the subject himself or by other persons. The data were secured by the following methods: First, by inserting in the question list a number of questions concerning likes and dislikes, laughter and weeping, anger and its effects, habits of introspection, moods, promptness or its opposite in decisions, ideals, the effects of excitement, habits with respect to physical activity, etc. A particularly fruitful interrogation was the following: "If you were obliged to spend a whole day alone, felt at perfect liberty to follow your inclinations and had the means to do so, what would you do?" At no point in the questions was temperament or disposition mentioned.

The second method was by observation of the general tone of the papers. The question list, it may be remarked, was very lengthy. It included approximately 200 specifications, all planned with reference to the evoking of memories rather than the securing of categorical replies. Its length precludes its presentation here. The responses were correspondingly extended, and not the least remarkable thing about them was the amount of information they imparted between the lines. It was obvious that they were not merely a record of phenomena, but also a body of original phenomena. Sometimes what they purported to be as a record had to be offset by what they were as new facts. Thus, in response to the question, 'Do your friendships last?' nearly every writer gave an affirmative answer. Here it is probable that the ideal of the writers rather than their actual experience comes to expression. These answers have value, therefore, as evidence of the nature of the social instinct, but hardly as evidence of actually existing social relations. Occasionally the manner of responding to a question revealed more than did the content of the response. Intellectual interest stood out in one, strenuous seriousness or passionate earnestness in another, while the chattiness of a third revealed a type of impressionability strongly contrasted with both.

A third method was by objective observation and interviews, as already described. The scheme of questions underlying this part of the investigation was also extended. It included, among other topics, the following: The habitual state of the muscles, particularly the face, whether tense or relaxed; one's carriage and motions, whether quick, jerky, irregular, or more slow, free and pendulum-like; one's mode of speech and the quality of the voice; the expression of the eyes, and any other signs that show whether the subject is wide-awake to his surroundings; whether one is more given to the reception of impressions or to active effort to control surroundings; readiness to laugh and cry; specific manifestations of anger; characteristic moods; persistency; social self-assertiveness of various types; intellectual habits; religious habits.

The data obtained by all these methods were compared, and



thus the final judgment was based upon a really wide range of facts. Furthermore, in most cases, independent judgments were formed by different observers, and these judgments were finally checked off against one another. As soon as a definite and comprehensive mode of procedure was discovered, the facts began to fall into place with the sort of inevitableness that inspires confidence in one's method. The amount of agreement reached by observers independently of one another was another evidence of the trustworthiness of the method. If the lack of precision and of quantitative determinations should seem to impair the value of the results, two considerations might be offered in defence. The first, is that all the knowledge of temperament possessed by biographers and historians and by literary workers, and nearly all that possessed by psychologists themselves, has been gathered by methods analogous to this, though rarely, if ever, by methods so systematic and comprehensive. The other consideration is that this manner of learning men is one of the bases of the world's successful business. Indeed, a large part of the practical interests of life hang upon our ability so to observe temperamental manifestations as to be able to predict the general quality of one's reactions in different sets of circumstances. Of course, this is not a sphere in which claims to scientific infallibility become even plausible; nevertheless, the thorough and systematic analysis employed may fairly entitle the results to some degree of confidence.

The temperamental classification of the members of the three groups concerning whom adequate information was obtainable yields the results shown in the table on the next page.

The most marked contrast in this table concerns the relation of the two main groups to intellect and sensibility. Where expectation is satisfied, there sensibility is distinctly predominant; but where expectation is disappointed, there intellect is just as distinctly predominant. To appreciate the strength of this conclusion, it will be well to remind ourselves once more of the range of facts upon which it is based. In only three cases in Group I. and one case in Group II. was it necessary to rely solely upon the subject's paper. A second interesting result is that those whose expectation is satisfied belong almost exclusively

to the slow-intense and prompt-weak varieties, the temperaments approaching most nearly those traditionally known as the melancholic and the sanguine. On the other hand, those whose expectation is disappointed belong more largely to the prompt-intense variety, or the choleric temperament; though the distribution between the choleric, melancholic and sanguine is not

RELATION OF STRIKING TRANSFORMATION TO TEMPERAMENT.

	Sensibility Predom- inant.	Intellect Predom- inant.	Will Predom- inant.	Prompt- Intense.	Slow- Intense.	Prompt- Weak.	Slow- Weak.
GROUP I.—17 persons who expected a transformation and experienced it...	12	2	3	1	6	8	2
GROUP II.—12 who expected but did not experience...	2	9	1	7	3	2	
GROUP III.—5 others who belong to both the above classes....	2	2	1				

markedly uneven. Again, comparing the two main groups with respect to promptness and intensity, each by itself, we find that, on the whole, Group II. exceeds Group I. in both promptness and intensity. Finally, some slight confirmation of the representative character of these results is found in the heterogeneity of the cases in Group III. The full significance of these results concerning temperament, however, will not appear until we have examined the same subjects with respect to automatisms and suggestibility.

Careful inquiry was made, both in the question list and by personal cross-questioning, for evidence of mental and motor automatisms. The inquiry divided itself into these heads: striking dreams in connection with religious awakenings; hallucinations in connection with religious transformations; hallucinations occurring at other times; motor automatisms occurring at the time of religious transformation, and similar automatisms occurring at other times. The purpose of the inquiry did not make it necessary to render these various classes

rigorously precise. Accordingly, when it was difficult to decide whether a given phenomenon was to be classed as a dream or as a hallucination, I followed the impression of the subject. If he insisted that he was awake at the time, the experience was classed as a hallucination. Similarly, the group of motor automatisms contains some cases that fall near the boundary line. But, in general, it is believed that the list which follows is a full and substantially accurate census. It contains all the facts of these classes discovered in the entire investigation.

*Striking dreams in connection with religious awakening:*

Dreamed of being cast into hell. Suffered all the torments of the damned that he had ever heard about.

Dreamed of being cast out of heaven.

Dreamed of a heavenly procession which he could not join.

Dreamed of taking an examination of fitness to go to heaven.

*Hallucinations in connection with religious transformation:*

Streaks of light shone down.

A somewhat bright, diffused light just above the eyes; occurred twice.

Seemed to observe the joy in heaven.

Saw a vision of the broad way and of the narrow way, with many persons in the former and few in the latter.

*Motor automatisms at time of religious transformation:*

Uncontrollable laughter for fully five minutes.

A powerful thrill through the whole body.

Sudden clapping of hands before any change of feeling came.

Tobacco habit broken without effort or even seeking.

*Other hallucinations:*

Saw a light spring up from a tomb in a cemetery.

Used to hear his name spoken when he was about to commit some sin.

Had just retired after private devotion. Saw a dim, diffused light above the eyes.

Was touched by an absent friend.

Saw a dog that was not there.

Heard deceased grandfather's voice.

Heard mother's voice when she was far away.



Heard the voice of a friend.

Felt the presence of an absent friend. It seemed to be an objective fact and not a mere impression.

Heard music different from any he had ever listened to.

Heard angels sing.

In the midst of a public speech twice saw a scene he was describing.

Childhood fear of the dark has persisted. The feeling that a fiend is just behind and ready to spring upon him sometimes becomes so intense that self-control becomes impossible.

An inner voice which expresses approval at times of perplexity by saying, "Fear not, I am with you."

God tells her where things are that she is looking for. Also tells her things before they come to pass.

Voices and visions just before sleeping at night. Has often gone to the window or out of doors to see where the music came from.

Up to age of thirteen used every night to see figures in the room.

When praying had a vision of an absent friend who gave just the information that was desired.

Waked one night and saw a great luminous eye in the ceiling: thought it was God's eye.

*Other motor automatisms:*

Automatic laughter.

At times something very holy seems to be dictating his thoughts.

Has always felt himself under two influences: one good and one bad, and neither of them any part of himself.

Surprising and incomprehensible outburst of defiance to God at age of about ten or twelve years; shook fist at the sky and told God he hated him.

"The Holy Spirit often fills me so that I feel light, and it's no trouble to walk and not feel tired." (A lady well advanced in years.)

Talking, singing, whistling to one's self. This seems, at times, to become an automatic, sub-conscious performance. A parent affected in the same way sometimes lets out secrets by this means.

Let us now ask how these phenomena, exclusive of the dreams, are distributed among the different sets of cases. Of eighteen persons in Group I., eight have had either hallucinations or motor automatisms; of the five persons in Group III., four have had similar experiences. Hence of twenty-three persons who have had a striking religious transformation, twelve have also exhibited these automatic phenomena. But of the twelve persons in Group II., who sought a striking religious transformation in vain, only one has had either a hallucination or a motor automatism.

The total number of persons examined with respect to automatisms was seventy-four. Of these, nineteen had exhibited such phenomena; but twelve of these nineteen persons are found in Groups I. and III.—that is, one-sixth of the entire number of persons examined embrace two-thirds of the cases of automatisms. Putting these results in the form of percentages, we get the following:

General average of automatisms for 74 persons,  $25\frac{2}{3}$  per cent.

Average for those who have experienced a striking religious transformation, . . . . . 52 “

Average for those who sought such a transformation in vain, . . . . .  $8\frac{1}{3}$  “

In other words, the average for those who had a striking religious transformation is twice as high as the general average, and six times as high as the average for those who sought such a transformation in vain.

If the general average of automatisms seems rather excessive, the following explanatory circumstances should be borne in mind: First, motor automatisms are included along with hallucinations. Secondly, nearly all the persons examined were too young to have forgotten such experiences. Thirdly, the cross-questioning already described brought out a number of facts not elicited by the *questionnaire*, and not likely to be elicited by a census of hallucinations conducted by correspondence alone. Finally, it now becomes obvious that the high general average depends upon the *presence* of a relatively large number of persons who have experienced striking religious transformations.

The results are so unequivocal that interpretation is unnec-

essary. It may be worth while to add, however, that in two cases of motor automatism occurring at the time of religious transformation there was clear evidence of a congenital tendency to such performances. In both cases a parent had exhibited a similar automatism under similar religious conditions. In a third case it was possible to identify a phenomenon as probably automatic through a similar but more pronounced phenomenon in a parent. One case of hallucination was likewise clearly referable to congenital tendencies. Three of these four cases of congenital proclivity belong in Group I. Furthermore, to Groups I. and III. belong nearly, if not quite, all the persons who have experienced the healing of disease by faith, those who have received remarkable assurance of answered prayer in advance of the event, and those who reported other veridical premonitions. The conclusion is that the mechanism of striking religious transformations is the same as the mechanism of our automatic mental processes.

There remains for study the relative suggestibility of the three groups. At first thought, this seems to be a simple problem of more and less. But it is neither simple nor merely quantitative. Indeed, the qualitative varieties of suggestibility are quite as marked and quite as important as the 'suggestibility and non-suggestibility' which chiefly figure in the literature of suggestion. It must have struck many experimenters as a strange incident that, whereas persons of sound body and trained mind make excellent subjects, most of the literature represents suggestibility as identical with relative prominence of the lower centers. The fact seems to be that some persons are easily hypnotized, not because the higher rational centers are undeveloped, but precisely because the high development of these centers,—the habit of prompt concentration of voluntary attention,—makes it possible to follow the suggestions of the operator with precision. Moll remarks that the ability to direct one's thoughts in any particular direction is favorable to hypnosis, but that this ability is usually considered to be a sign of strength of will.<sup>1</sup> As the persons under examination in the present part of our study are, perhaps without exception, healthy, and as all have had con-

<sup>1</sup>Hypnotism, London, 1895, 40.



siderable mental training, it will be seen that ready response to suggestion cannot be regarded as an unambiguous sign. The experimentation was begun under the tentative hypothesis that auto-suggestion might possibly account in part for the failure of persons in Group II. to secure the desired experiences. The problem then became whether external suggestion was more prominent in Group I. and auto-suggestion in Group II.

The problem may be more precisely put by distinguishing between passive suggestibility and spontaneous auto-suggestion. The necessity of thus stating the distinction grows out of the ease of misunderstanding certain phenomena, particularly those commonly described as 'resisting the operator's suggestion.' Thus, if a subject struggles to open his eyes when I tell him that he cannot do so, this is no evidence of spontaneity. For the very assertion, in the early stages of hypnosis, that the eyes cannot open is a challenge to try; it is a double suggestion. This was exquisitely demonstrated upon one of my subjects. For some time I had tried in vain to close the eyes by making the usual passes and giving the usual suggestions of drowsiness, etc. At last the subject, who was apparently wide awake, declared that she could not close them and keep them closed. Catching at this hint, I suddenly remarked, "You cannot close them!" They immediately clapped shut with every appearance of doing it automatically. In another case in which the usual suggestions seemed to have little or no effect, the subject was instructed to keep his eyes closed voluntarily for a while; but his eyes opened very soon, and did so repeatedly. He finally declared that it seemed as if he *could not* keep them closed. In two other cases it was found that a previously formed conviction on the part of the subjects that they were suggestible had tended to make them appear more passive than they really were.

What was looked for, then, was evidence of spontaneity or originality, rather than mere readiness of response or its opposite. An illustration or two will make this clear. To one subject I declared that his outstretched arm was rigid and could not move. The arm immediately stiffened out, but began a series of incipient up-and-down motions. This was clearly a product of my own suggestion, as were also, perhaps, the sympathetic with-

ings of the body and contortions of the face. The cataleptic arm was the right one. Presently the left arm was raised and began to push down on the right one, evidently in an effort to lower it. Failing in the effort, the left arm itself now became cataleptic, and could not lower itself. Here the evidence of spontaneous auto-suggestion is unmistakable. Contrast this, now, with another case in which a suggestion was given that an arm was cataleptic. Certain incipient responses to the challenge were made as before; but they ceased in a few seconds, while the face and the rest of the body expressed little or no interest in what was going on.

Let us compare two other cases that are less striking, and yet unambiguous. In both, passes in front of the eyes and suggestions of heavy eyelids, etc., meet with very slow response, so slow that I finally close the lids with my fingers. If, now, I say "Your eyes are closed tight; you cannot open them," both subjects open their eyes. Similarly, they can unclasp their hands, and the like, whenever they are challenged to try. Thus far the two cases correspond point for point. But if, after closing the eyes, I leave the subjects alone, avoiding, as far as possible, the giving of further suggestions, a decided difference presently appears. One of the subjects sits with closed eyes for an indefinite length of time—that is, shows no initiative; but the other, as often as the experiment is repeated, spontaneously opens his eyes after a short interval.

Such experimentation resulted in separating the cases according to two fairly well-marked types. In respect to readiness of response to hypnotic suggestion the two types do not seriously differ. Under both types fall cases in which the response was almost immediate, and also cases in which it was very slow. But the behavior under suggestion was decidedly different. Let us call the two types the passive and the spontaneous. Under the former belong those who take no decided or original part in the experiment. Their response to external suggestion may not be very pronounced, but they initiate nothing after once they have begun to yield. Under the spontaneous type belong, on the other hand, the few who appear to be non-suggestible and those who, while responding to suggestion,

take a more or less original part by adding to the experiment or by waking themselves up.

Comparing Groups I., II. and III. with respect to this point, we find certain plain differentiations. To begin with, as might be expected, nearly all the persons who have experienced any of the mental or motor automatisms already described are 'passives.' Thirteen such persons were experimented upon, and, of these, ten clearly belonged to the passive type. This fact makes it appear that the two types here described are substantially parallel with those sifted out by certain experiments at Harvard University.<sup>1</sup>

A few cases were not accessible for purposes of experiment. The numbers experimented upon in the two groups were respectively 14 and 12. All the persons in Group III. were experimented upon. The results are as follows: In general, the line between Groups I. and II. coincides with that between the passive and the spontaneous types, though apparent exceptions exist, and though the interpretation of the facts is not equally clear in all cases. Of the 14 cases in Group I. (persons who expected a striking transformation and experienced it), 13 are of the passive type. Of the 12 persons in Group II. (expectation disappointed) 9 clearly belong to the spontaneous type, 1 is entirely passive and 2 are open to some doubt. Of the 5 persons in Group III. (striking experience, yet disappointed), 2 are passive and 3 spontaneous.

The nature of the evidence may be further illustrated and the conclusion still further strengthened by reference to the negative and doubtful cases. The one case in Group I. that is not clearly passive is the one first mentioned on a preceding page in illustration of the double character of many verbal suggestions. This case is probably a passive one, therefore; though not so counted in the above figures. Another member of this group seemed for some time to be an exception to the general rule. She had had three striking experiences, and yet was apparently not suggestible. One day, however, mention having been made in the class in psychology of pain induced in a tooth by imagining a dental operation, she soon felt a tooth-

<sup>1</sup> Cultivated Motor Automatism, by Gertrude Stein, *PSY. REV.*, V., 295ff.



ache. It became intense and lasted for three or four hours, the face meantime becoming sore and apparently swollen. This settled the question of passive suggestibility. Turning, now, to the negative and doubtful cases in Group II., we find that the one clearly negative case is one that stands on the border between Groups I. and II. This subject had more difficulty in classifying himself than any other one in either group. Again, of the two cases scheduled as doubtful, one is the only case in this entire group in which any form of mental or motor automatism was discovered. Nevertheless, the case remains ambiguous; for, though external suggestions are accepted with every sign of passivity, the subject has heretofore practised auto-suggestion, even to the extent of curing toothache and other minor pains thereby. His present passivity, therefore, may be partly or wholly due to training. By way of parenthesis it may be remarked that each subject was questioned as to whether he had ever been hypnotized or had ever witnessed hypnotic experiments, and his reactions were judged according to his replies.

The correlation between one's religious experience and one's type of suggestibility was sometimes found to be curiously complete. Here, for example, is a subject whose response to passes and suggestions of drowsiness is not prompt; yet when the response comes it simply plumps itself. The subject is now very passive. In response to a suggestion, an arm quickly becomes cataleptic; but, in the midst of the experiment, something having incidentally appealed to the subject's interest, he spontaneously opens his eyes and appears to be completely out of the hypnosis. This man was converted at the age of sixteen, with marked manifestations. His whole being was thrilled with joy, and he had what he regarded as the witness of the Spirit. But from seventeen to nineteen he endured terrible storm and stress, in which he sought in vain to recover his original status. He finally settled down to the conviction that we are children of God in our deeds and thoughts rather than in our particular moods and feelings.

A still more remarkable parallel is as follows: Response very prompt; lids clapped shut and trembled. At the suggestion that they could not open, they quickly opened. The re-

mark was then made that perhaps the lids would not close so promptly next time. The suggestion worked, for now it required many passes to shut the eyes. The arm refused to become cataleptic; but when I began to breathe deeply and slowly, as if asleep, the subject's head promptly began to fall forward; and it continued downward until it rested on the breast. The subject was now apparently in a deep sleep; but after awhile a spontaneous awakening occurred. He was re-hypnotized and told that he could not pronounce his name; a gentle struggle ensued and lasted for a considerable time, but the effort was not given up until the name was successfully pronounced. The characteristics here are initial passivity followed after a while by decided spontaneity. This exactly describes the subject's religious experiences also. On two different occasions, after earnestly seeking for a marked experience, he happened to notice some incidental thing in his environment that he took to be a divine token. Immediately he experienced great exaltation; his heart's desire seemed to be realized; but after a few days the emotion waned, and reaction setting in pronounced a severe verdict upon the whole performance.

In order to appreciate the weight of these results concerning the relation of suggestibility to religious transformations, it will be necessary to notice once more the principle upon which cases were classed in Group II. This group contains no case in which there was not a distinct effort to obtain an experience that never came. Now, of the 74 persons examined, there are many whose training and environment were equally adapted to induce expectation and seeking, but did not do so. It is therefore probable that spontaneous auto-suggestion prevented expectation in some as it prevented the fulfillment of expectation in others. Hence, the sphere in which it plays a decisive rôle is undoubtedly much larger than the numerical proportions seem to indicate.

Moreover, no statistical display can do justice to facts of this sort. For not only must the numbers express in some degree one's interpretation of facts, and not merely the bare facts themselves, but the qualities with which we are dealing are too profound and pervasive to be expressed in any simple formula.

The whole style of one's mental organization is involved. It is safe to say that any observer of human nature would perceive the propriety of setting off Groups I. and II. from each other. The personalities in each group taken by itself are relatively alike, while the two groups are clearly different from each other. Psychology merely renders this obvious difference more precise by saying that the difference is one of temperament and of a more or less spontaneous attitude toward environment.

It has been shown that three sets of factors favor the attainment of a striking religious transformation—the temperament factor, the factor of expectation, and the tendency to automatisms and passive suggestibility. Let us, in conclusion, note the effect of combining these three factors. Of 10 cases in which there is expectation of a marked transformation, together with predominance of sensibility and passive suggestibility, the number whose expectation was satisfied was 9; but of 11 cases of such expectation, together with predominance of intellect or of will, and with spontaneous auto-suggestion, not one was satisfied. These numbers include cases from Group III. as well as from Groups I. and II.

If our groups seem to contain rather few cases, it should be remembered that a problem of this kind requires relatively complete knowledge of a few cases rather than an item or two of knowledge regarding many cases. Our procedure must necessarily consist in a gradual narrowing down of the range of cases, together with increasing minuteness of scrutiny in each case. As a matter of fact, we have approached about as closely to the strict method of experiment as the subject permits. The factors are so definitely identified that prediction becomes safe wherever either of the two combinations just mentioned is found present. Given three factors, the fourth—the general character of one's religious experiences—can be predicted with a high degree of probability.

It is supposed by many that striking transformations in the affective life are reserved for those who have been great sinners. The idea seems to be that an abrupt transition from moral badness to moral goodness naturally carries great emotional disturbances with it. And doubtless such circumstances do tend



to intensify whatever happens. But it does not at all appear that these circumstances are the chief factors that determine the degree of affective transformation at conversion; for among the cases belonging to Groups I. and III. there is only a meagre sprinkling of persons who had ever been bad in any very positive sense. In fact, of the entire 23 persons, only 5<sup>or 6</sup> report having experienced any sorrow for specific sins, and even then the sin repented of was generally a bad temper or some similar infirmity. On the other hand, of 13 persons in Group II., all of whom sought a striking transformation in vain, 3 also report sorrow for specific sins.

In short, everything goes to show that the chief circumstances favorable to these striking experiences are expectation, abundance of feeling and passive suggestibility with its tendency to automatism. Shall we therefore conclude that conversion is practically an automatic performance? By no means. What has been proved is simply that when conversion or an equivalent change takes place in one's moral attitude toward life and destiny and God, it may clothe itself in certain emotional habiliments provided certain factors are present, but otherwise not.

“Would you cast the horoscope of a human life?” says Fouillée. “It is not to be read in the constellations of the sky, but in the actions and reactions of the interior astronomical system—do not study the conjunction of the stars, but those of the organs.”<sup>1</sup> Similarly, we may now add: Would you understand the emotional aspects of religious experiences? Do not ascribe them to the inscrutable ways of God, but to ascertainable differences in men's mental constitutions; do not theorize about divine grace, but study the hidden workings of the human mind!

<sup>1</sup> *Tempérament et Caractère*, Paris, 1895, 88.

## SHORTER CONTRIBUTIONS AND DISCUSSIONS.

### ATTRIBUTES OF SENSATION.<sup>1</sup>

I have the temerity to propose an attack upon the anomalous and, as I think, indefensible position of the so-called attributes of sensation: quality, intensity, extent and duration. Entrenched as it is behind traditional opinion, I hold that the entire conception of attributes of sensation is untenable, and this for two main reasons: first, because sensation is an elemental fact of consciousness and as such, by definition, irreducible: second, because each so-called attribute may be shown to be either itself an element of consciousness or a complex of such elements.

#### I.

Of these two, the more general argument should first be considered. By common consent of the psychologists who treat of consciousness from the analytic standpoint, sensations are unanalyzable elements. Thus Wundt<sup>2</sup> defines *Empfindungen* as “Zustände unseres Bewusstseins welche sich nicht in einfachere Bestandtheile zerlegen lassen”; Ladd<sup>3</sup> says definitely that ‘simple sensations’ are “processes of our sense-experience which we are unable in any way to regard as composite or as analyzable into still more nearly ultimate factors”; Külpe<sup>4</sup> coordinates sensations with affections as “letzen Elemente \* \* einer genauen Analyse”; James<sup>5</sup> observes that “sensation, so long as we take the analytic point of view, differs from perception only in the extreme simplicity of its object or content,” and Titchener calls sensations ‘elemental conscious processes’<sup>6</sup> and defines conscious elements as “mental processes which cannot be further analyzed, which are absolutely simple in nature and which consequently cannot be reduced even in part to other processes.”<sup>7</sup>

<sup>1</sup> Read at the New York meeting of the American Psychological Association, December, 1898.

<sup>2</sup> *Physiologische Psychologie*, 4te Aufl., I., 281.

<sup>3</sup> *Psychology Descriptive and Explanatory*, p. 92.

<sup>4</sup> *Grundriss der Psychologie*, §3, ¶1.

<sup>5</sup> *Principles of Psychology*, V. II., p. 1.

<sup>6</sup> *Outline of Psychology*, §7.

<sup>7</sup> *Op. cit.*, §4, p. 13.

Our next question concerns the nature of this analysis and its results. 'Element of consciousness' may mean the simplest concrete experience, the least complex content of actual consciousness; or it may mean the simplest distinguishable, though inseparable, ingredient of a given experience—a result of abstraction, an unanalyzable datum of consciousness. It can be shown that this second more rigid sense is that in which the word is used by those who treat psychology analytically. Thus Ladd is at pains to say that a "simple sensation is a convenient abstraction of psychological science<sup>1</sup> and definitely states that "such elements are never to be regarded as actually separable by analysis either from each other or from the state in which they are<sup>2</sup> said to exist. \* \* \* No psychologist \* \* \* thinks of maintaining the separate reality of the factors of mental life." Wundt's assertion is as unequivocal:<sup>3</sup> "Isolirt ist uns die einfache Empfindung niemals gegeben, sondern sie ist die Resultat einer Abstraction." Similarly, Külpe says distinctly:<sup>4</sup> "Die seelischen Elementarphänomene [sind] stets in irgend welcher Verschmelzung oder Verknüpfung mit anderen wahrnehmbar. \* \* \* Ein wirkliches Erleben nur einer einzigen Empfindung kommt nicht vor." And with equal decision, Titchener<sup>5</sup> asserts that "the particular sensation, regarded apart from other sensations is the product of scientific analysis, an abstraction of actual mental experience."

By common admission, therefore, the sensation, so far as it is analytically treated as an element of consciousness, is not a concrete experience at all, but a result of abstraction, useful for purposes of close observation and of scientific classification. It is certainly, then, an apparent contradiction to speak of the element as having attributes, in the ordinary meaning of attribute, which is just 'quality' or 'characteristic.' The element is precisely that which cannot be further reduced, characterized or qualified. Therefore, only the complex phenomenon has attributes, and these turn out to be precisely the elements of which it is composed.

This objection to the ordinary doctrine of attributes has been generally overlooked. Külpe, to be sure, says briefly:<sup>6</sup> "Spite of the qualitative simplicity of the sensation, different attributes disclose themselves," and Schumann<sup>7</sup> in a recent contribution, 'Zur Psychologie der Zeitanschauung,' observes that it is not securely set-

<sup>1</sup> *Op. cit.*, p. 92.

<sup>2</sup> *Ib.*, p. 89.

<sup>3</sup> *Op. cit.*, I., 281.

<sup>7</sup> *Zeitsch. f. Phys. und Psychol. der Sinnesorgane*, XVII., 1, p. 112.

<sup>4</sup> *Op. cit.*, §3, ¶5.

<sup>5</sup> *Op. cit.*, §43.

<sup>6</sup> *Op. cit.*, §4, ¶1.



tled (*sicher festgestellt*) "how we come to distinguish the attributes of intensity, quality and temporal duration in the inseparable unity (*untrennbarer Einheit*) of an auditory sensation;" but these are cases in which the contradiction is calmly faced and accepted. Titchener proposes the following solution of the problem: "Although the sensation," he says,<sup>1</sup> "is an element of mind—*i. e.*, a process which cannot be split up into simpler processes—yet it has various aspects or attributes—presents different sides, so to speak—each of which may be separately examined by the psychologist." But the attribute, thus defined, cannot be distinguished from the element by the fact that it is presented to a psychologist and examined by him, for the same holds as true of the sensation itself as of any attribute. And the question at issue is precisely this: If the process really cannot be split up into simpler processes, how does it happen to have more than one 'side' or 'aspect'?

In the essay from whose earlier pages we have already quoted, Schumann seems to suggest<sup>2</sup> another explanation of the difficulty. He observes that the sensation, spite of its oneness (*trotz ihrer durchaus einheitlichen Natur*), can call up distinct judgments of intensity, quality, extent and duration. But in insisting upon the irreducibility of the sensation and in finding the diversity of the attributes in the judgments about sensation, he is as untrue to introspection as to traditional theory, for a sensation deprived of all its attributes will itself vanish. As Külpe has it, "Die Empfindung ist nichts ausser ihren Eigenschaften. Es bleibt kein Rest \* \* \*." One may, of course, make judgments about quality, intensity and extent; but all judgments are based, in their last analysis, on immediate consciousness, and the attributes, in order to be judged about at all, must first be immediately experienced.<sup>3</sup>

The only detailed justification, which I know, of the theory of attributes, is contained in a discriminating paper, from which I quote at length, by Dr. Ellen Talbot, on 'The Doctrine of Conscious Elements.'<sup>4</sup> "We have said," Miss Talbot remarks, "that when we have resolved our mental facts into facts which are themselves irresolvable, our process of analysis is finished. This is true; yet it would not be correct to say that there is no further occasion for analysis. There is need of a second process for the purpose of determining the

<sup>1</sup> *Op. cit.*, §8, p. 29.

<sup>2</sup> *Op. cit.*, p. 131.

<sup>3</sup> *Op. cit.*, §4, ¶1.

<sup>4</sup> *Philosophical Review*, IV., p. 162.

properties of our elements \* \* \* its various attributes, such as intensity and quality. But this does not shake our faith in the validity of our general criterion of ultimates, for this second analysis is in no sense a continuation of the first process. \* \* \* In the first analysis, we passed successfully from one process to another, finding in each new stage the explanation of the more complex one which preceded it. When we have at length reached a process which we cannot explain by any other *process*, our regress is finished, our element is discovered. Whatever analysis may now be possible will be entirely distinct from the first and will in no way affect its claim to be distinct."

The argument is ingenious, but misleading. Even if one grant Miss Talbot's contention that sensations, on the one hand, and qualities or intensities, on the other, are reached by different processes of analysis, it still remains true that the results of that second analysis may justly claim the title of 'element' rather than that of 'attribute.' But the entire hypothesis of a second analysis shows itself, on closer scrutiny, to be baseless. It is probably derived from the false analogy with an atom or with a chemical element, which, while physically and chemically unanalyzable, is obviously characterized by psychic attributes, such as weight and form, and color or odor. But just as a chemical element is not further decomposable and reducible to chemical attributes, so it is logically impossible that a psychic element should lend itself to further psychological analysis.

Introspection bears out this *a priori* conclusion. The analysis whose results are admitted to be elements of consciousness—that is, the discrimination within a complex percept of distinct sensations and affections—does not differ noticeably from the analytic study of its hues, intensities and forms, which, according to Miss Talbot, is a second sort of analysis. But if this 'second analysis' into attributes is indeed a mere continuation of the first, into sensations, then these sensations can no longer claim to be unanalyzable elements of consciousness. The only escape from this position would be by a return to the rejected theory that 'element' means, not an undistinguishable abstraction, but the simplest fact of real experience. In this case, however, as has been suggested, analysis has already gone too far, for even the combination of quality, intensity and extent which makes up a sensation, on the ordinary view, is an artificial abstract and not the simplest of concrete mental experiences. If elements are to be defined, on this principle, as the simplest factors of actual experience, then they can include nothing more remote from reality than ideas or images and emotions.

In truth, one cannot have it both ways: either the sensation has attributes, but then it is a complex, no element and has lost its excuse for psychological being; or the sensation is an irreducible and unanalyzable element, but then its simplicity is absolute, not to be trifled with, and not to be explained away by reference to any second process of analysis into elements, which yet are not elements, but only 'attributes,' 'aspects' or something equally vague and meaningless.

## II.

The conclusion that an element of consciousness cannot possess attributes leaves untouched the question of the nature of the so-called attributes. For, however misleading the colors under which they sail, quality, intensity, extent and duration are nevertheless genuine factors of our experience. If the traditional classification is rejected, some other must be suggested in its place.

At the outset, duration must be sharply distinguished from its fellow-'attributes.' When it is said that sensations have quality, intensity and—in some cases—extent, the meaning is, that to have the sensation at all one must be immediately conscious of quality, intensity and sometimes of extent. But we are not by any means always conscious of the duration of a given sensation; on the other hand, we are notoriously oblivious of the passage of time in much of our sense experience. And yet always, whether we are conscious of it or not, the sensation has duration—that is, "it lasts a certain time."<sup>1</sup> Duration is not, therefore, an attribute, like the rest, by virtue of being a constituent of sensation, but is, as it were a reflective attribute, what Schumann calls a '*Beurtheilung der Dauer.*'<sup>2</sup> Moreover, duration, even in this sense, is not a purely psychic attribute, but belongs to physical as well as to conscious facts, and is in truth the characteristic of all serial phenomena. Of course, duration, besides being later predicated of an event of consciousness, may itself also be immediately experienced; and indeed such direct acquaintance is the basis of the later prediction. But duration is, in this case, distinctly a complex experience. Höffding<sup>3</sup> has analyzed it very acutely into the factors of 'change' and of 'connection.' Certainly it lacks the simplicity of the attribute.

<sup>1</sup>Titchener, *op. cit.*, § 8, p. 30.

<sup>2</sup>*Op. cit.*, p. 131. Schumann, curiously enough, treats all the attributes as judgments called forth by the 'einheitliche Empfindungen,' ignoring the immediateness of our experience of them.

<sup>3</sup>Eng. Tr. Outlines of Psychology, pp. 184-186.



Even the most ardent advocate of the traditional theory should, therefore, reject 'duration' as an attribute of conscious elements, for either it is an unpsychological attribute of phenomena in general, reflectively 'added' to the sensation, or else it is a complex psychic content. 'Quality,' 'intensity' and 'extent' must be differently treated. They are 'attributes' by virtue of being psychic contents, and if we refuse them the name we must fit them into some other appropriate corner of our psychological scheme.

The case of quality may be most readily considered, for already the universal habit of classifying sensations according to quality<sup>1</sup> and the admission by most psychologists that quality is the most important element have correctly suggested that quality is itself sensation. Titchener goes further. Besides reproducing Külpe's description<sup>2</sup> of quality as the 'core or kernel of sensation'<sup>3</sup> to which the other attributes are referred as the duration, intensity and extent of a quality; and, not content with calling quality the 'most important and fundamental'<sup>3</sup> and the 'absolute'<sup>4</sup> attribute, Titchener says definitely:<sup>3</sup> "It is quality which makes sensation an elemental conscious process." More than this, in his paragraph on the 'total number of elementary sensations,'<sup>5</sup> he states distinctly that each of these 40,000 qualities is a conscious element, distinct from all the rest and altogether simple and unanalyzable. This reduction of quality to sensation-element accords with the plain results of introspection. Such 'qualities' as 'this blue,' 'this pitch,' 'this warmness,' are surely distinguishable factors of consciousness, though they are, of course, inseparable from certain intensities and—in the case, at least, of the color and the warmness—from certain extents. But if distinguishable, since they are also irreducible, they are by definition elements of consciousness.

Nothing forbids a similar treatment of intensities as sensation elements; but such a theory lacks even the virtual sanction of the authorities, and must, therefore, be more carefully considered. It appeals, in the first instance, to ordinary self-observation. Does not introspection clearly reveal that complex, sensational experiences differ in intensity as truly as they differ in quality? A very soft sound of a given pitch is as distinctly 'different,' though differently different, from a loud tone of the same pitch, as two tones of the same intensity but of discordant pitch. In the same way highly salted food differs unequivocally from that which is only slightly salted; 'brightness' as well as

<sup>1</sup> Ladd, *Elements of Psychol.*, 356; Baldwin, *Senses and Intellect*, 85.

<sup>2</sup> *Op. cit.*, § 4, 2.

<sup>4</sup> *Ib.*, p. 77.

<sup>3</sup> *Op. cit.*, p. 31.

<sup>5</sup> *Op. cit.*, § 22, p. 67.

'blueness' is a direct, distinguishable element in a visual sense experience; and the loudness as well as the high C quality is a noticeable constituent of the auditory content. Of course, the blueness is more definitely named, more practically important and perhaps more affectively toned, yet it is not more undeniably present, as a distinguishable part of the experience.

To those who already virtually admit that 'quality' is itself sensation, an additional introspective argument will be found in the relation of intensity to quality in the visual series of greys. For here, as is generally admitted, intensity and quality coincide; a grey of lessened intensity is a grey of a different shade. This seems to show so close a relation between the two that the one may surely be treated as sensational element, if the other is.

Three objections to this doctrine must be seriously considered. The first lays stress upon the relative nature of intensity. "We estimate intensity," Titchener says,<sup>1</sup> "always by comparison with other intensities. Our use of terms indicates this. 'Blue' means something fixed and absolute, but 'large' is altogether relative and comparative." This distinction, which must certainly be admitted, does not, however, invalidate the claim of intensity to be regarded as an element of consciousness. It is a fact that we have few names for intensities, partly because there are so many of them shading almost imperceptibly into each other, and partly because only the greater differences of intensity are of practical concern to us through their connection with our emotional experience or through their harmful effect on our bodies. But if the possession of a name were the essential distinction of the 'quality' from the 'intensity,' then odors for the most part could no longer be classed as qualities, since they notably belong to the group of the unnamed. The relativity of intensities, though admitted in this sense, does not, therefore, debar them from coördination with the qualities, among the conscious elements.

It may be urged, in the second place, that intensity is too general a characteristic to be classed as sense element; that variations in degree are common to colors, sounds, odors—indeed, to all sensations; and that so common an attribute cannot itself be a sensational element of consciousness. Now, granting the assertion that intensity is a peculiarly 'general' sort of conscious content, this means only that one and the same sort of intensity accompanies all sorts of conscious ele-

<sup>1</sup> *Op. cit.*, § 26, pp. 77-78. This argument and those which follow are offered by their author as proofs of the relative importance of quality, compared with the other 'attributes.'

ments, and this hypothesis is not inconsistent with the teaching that intensity is a psychic element. This possibility need not, however, be discussed, for introspection does not bear out the observation on which it rests. Color intensities are not the same sorts of intensity as sound-intensities. Parallel with the difference between color and pitch, there is a difference between brightness<sup>1</sup> and loudness. So there is a difference between hardness—a pressure intensity—and the marked degree of a given sweetness. Intensities are really, therefore, as individual as qualities.

The last and most weighty argument remains. To quote again from Titchener, no distinct "bodily process in a definite bodily end-organ is connected with a sensation-intensity, since one and the same kind of bodily process may \* \* \* be more or less well-marked (intensity of sensation) in different instances." Now to those who believe, with the writer, that observed distinctness is the ultimate criterion of psychological analysis, and that the discovery of assignable physiological differences may strengthen and supplement, but never contradict, the result of psychological analysis, this argument cannot be final, even if one admit what it implies, that there are no characteristic physiologic accompaniments of intensity. Such admission, however, is unnecessary; nor need we take refuge, to save our theory, in the unverified hypothesis, that contents which differ in intensity are conditioned by the excitation of different cortical layers. In truth, the physiological correlate of intensity is as readily assigned as the physical stimulus: amplitude of atmosphere or of ether-wave. Just as differences in the locality of nervous excitation correspond with differences in sense-quality, so differences in the degree of physiological excitation may correspond with differences in psychical intensity. Such distinctions of physiological intensity cannot, it is true, be connected with definite conscious states after the manner in which 'sense centers'—that is, quality centers—have been localized, but undeniably they exist and may be regarded as the physiological correlates of psychical intensities.

Thus the objections to the sensation-character of intensity lose their force, either because they involve unessential criteria of sensation or through contradiction of the results of introspection. Intensities, therefore, like qualities—loudnesses and brightnesses, like hues and pitches—take their places among the distinguishable elements of consciousness.

By almost precisely parallel arguments it might be shown that ex-

<sup>1</sup>In the sense 'color-intensity'; not in Titchener's sense 'grey.'



tensity, if regarded from a nativistic point of view, is itself an element of consciousness, whereas, to the empiricist, it is a complex of sensational elements, chiefly motor. In either case there is nothing gained by naming it 'attribute' of sensation. For if abstract irreducibleness and distinctness be seriously maintained as the sole criteria of the psychic element, analytic psychology has no place and no use for the 'attribute' of sensation.

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### IS THE MEMORY OF ABSOLUTE PITCH CAPABLE OF DEVELOPMENT BY TRAINING ?

The experiments the results of which I am going to report here were made in Berlin during the time from March to October, 1895, jointly by Dr. Victor Heyfelder and myself. I did not publish them earlier, because I expected to make a complete investigation into the memory of absolute pitch. After having given up this intention I shall describe those experiments separately.

The theoretically important question is: whether human beings are to be divided into two classes, one of them enjoying a memory of absolute pitch, the other wanting it, or whether there is but a gradual difference in memory of absolute pitch, some people needing more, some less practice to obtain an equal facility.

Should the former be true, we would have to assume that the first class possesses a physiological property, the lack of which prevents the others from acquiring that mental faculty. But it would be very difficult to say what kind of physiological property it might be.

In favor of the latter is the fact that everyone has a certain amount, however small, of memory of absolute pitch, being able to recognize and discriminate, *e. g.*, the sounds of a violin and a bassviol merely through the pitch.

Kries<sup>1</sup> indeed will not grant that this already may be called a memory of absolute pitch. But I do not see any reason for refusing this name in any case where the individual is *unable* to determine the pitch with an average error *less* than a certain interval, *viz.*, a third. That there is no such reason is proved by our experiments, which show that individuals with not more memory of absolute pitch than above described by *systematical* and *sufficiently lasting* practice may be trained to meet the conditions of Kries. It may be mentioned

<sup>1</sup> *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Vol. 3, p. 257-279.

that the *possibility* of such training by Kries and many others has been *denied*.

We used for our experiments tuning forks as well as a piano. In both cases we named the pitches not by their musical names, but by their vibration rates, a table of which we had lying before us. We began with few pitches and from time to time added some new ones, as is to be seen in the tables. Each tone was repeated as often as wished.

On the piano we began with 10 pitches at intervals of a sixth. When the number of different pitches reached 20, the intervals were major thirds; when 39, whole tones.

TONE PRODUCED BY TUNING FORKS; MARCH TO MAY, 1895.

		HEYFELDER.				MEYER.			
Correct Judgments %.		83	78	70	56	75	71	67	59
Number of Judgments.		136	365	457	91	137	364	460	92
Vibration Rates.	100	$\frac{7}{10}$	$\frac{24}{79}$	$\frac{21}{86}$	$\frac{4}{32}$	$\frac{5}{11}$	$\frac{17}{41}$	$\frac{17}{55}$	$\frac{3}{12}$
	122	$\frac{15}{30}$	$\frac{27}{88}$	$\frac{17}{72}$	$\frac{2}{25}$	$\frac{11}{22}$	$\frac{13}{41}$	$\frac{15}{49}$	$\frac{1}{20}$
	150	0	0	$\frac{29}{73}$	$\frac{2}{25}$	0	0	$\frac{23}{44}$	$\frac{4}{20}$
	188	$\frac{14}{27}$	$\frac{36}{86}$	$\frac{26}{71}$	$\frac{1}{25}$	$\frac{11}{38}$	$\frac{21}{50}$	$\frac{16}{49}$	$\frac{4}{20}$
	220	0	0	$\frac{26}{70}$	$\frac{1}{25}$	0	0	$\frac{25}{49}$	$\frac{5}{20}$
	300	$\frac{15}{30}$	$\frac{24}{79}$	$\frac{19}{74}$	$\frac{1}{25}$	$\frac{12}{31}$	$\frac{25}{50}$	$\frac{27}{71}$	$\frac{5}{20}$
	400	0	$\frac{29}{73}$	$\frac{24}{61}$	$\frac{1}{25}$	0	$\frac{12}{30}$	$\frac{21}{47}$	$\frac{4}{20}$
	480	$\frac{15}{30}$	$\frac{30}{77}$	$\frac{23}{69}$	$\frac{1}{25}$	$\frac{11}{35}$	$\frac{23}{50}$	$\frac{23}{68}$	$\frac{5}{20}$
	680	0	$\frac{34}{82}$	$\frac{30}{76}$	$\frac{1}{25}$	0	$\frac{16}{47}$	$\frac{33}{77}$	$\frac{2}{20}$
	800	0	0	0	$\frac{1}{25}$	0	0	0	$\frac{1}{20}$
	960	$\frac{18}{36}$	$\frac{29}{73}$	$\frac{14}{35}$	$\frac{2}{25}$	$\frac{14}{31}$	$\frac{22}{50}$	$\frac{20}{66}$	$\frac{3}{20}$
	1200	0	0	$\frac{21}{52}$	$\frac{1}{25}$	0	0	$\frac{26}{55}$	$\frac{3}{20}$
	1600	$\frac{11}{22}$	$\frac{17}{44}$	$\frac{14}{35}$	$\frac{1}{25}$	$\frac{12}{31}$	$\frac{23}{50}$	$\frac{26}{77}$	$\frac{4}{20}$
	2400	$\frac{7}{14}$	$\frac{11}{28}$	$\frac{12}{31}$	$\frac{1}{25}$	$\frac{11}{22}$	$\frac{23}{46}$	$\frac{13}{26}$	$\frac{4}{20}$
	3200	0	0	0	$\frac{1}{25}$	0	0	0	$\frac{1}{20}$
4000	$\frac{1}{2}$	$\frac{19}{38}$	$\frac{18}{36}$	$\frac{1}{25}$	$\frac{14}{28}$	$\frac{21}{42}$	$\frac{17}{34}$	$\frac{1}{20}$	

The fractions give the relation of right to wrong cases.

TONE PRODUCED ON THE PIANO; JUNE TO OCTOBER, 1895.

Number of different pitches.	HEYFELDER.		MEYER.	
	Number of judgments.	Correct judgments %.	Number of judgments.	Correct judgments %.
10	69	81	69	86
12	46	72	46	85
14	46	70	46	65
16	69	75	69	61
18	92	74	92	66
20	368	73	368	59
39	736	64	736	60

Even when we had the choice of 39 pitches, more than one-half of our judgments were correct, and errors surpassing the neighboring pitch on either side were quite rare.

We did not continue those experiments further, because the value of the acquired facility did not seem to us to correspond to the expense of time. Now, after several years have passed we have lost the greater part of what we had acquired, by the want of continued practice.

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## PSYCHOLOGICAL LITERATURE.

*Instinct and Reason, an Essay concerning the Relation of Instinct to Reason, with some special Study of the Nature of Religion.*

HENRY RUTGERS MARSHALL. New York and London, Macmillans. Pp. vii + 574. \$3.50.

This work presents a most ingenious and interesting hypothesis, the fruit of fifteen years of special study and reflection, as to the relations and relative biological values of these two groups of mental processes. The work possesses a thorough-going quality, born of patience and sincerity, found only in works produced in a similar manner. Its first object was to present the conception of religion which it contains; but to this end it was necessary to treat of both instinct and reason, and the discussion of religion assumes a subsidiary place.

The method of the work is 'objective.' Like the investigations which resulted in the law of evolution, the method here subordinates the inner to the outer, the psychic to the organic, and construes all mental processes, even religion, in biological terms. The work accordingly does not concern itself with questions of origin. The law of evolution does not touch the question of origins, and is not a law of progress except for those whose desires and impulses lead them so to construe it. The empirical relations of instinct and reason, together with their biological significance and value, are here in question; and these problems are to be studied by construing wide objective groups of human and animal activities in the light of the biological doctrine of evolution. We are studying throughout the work what may or must be conceived, rather than what is—a task which, in all scientific procedure, goes before the work of verifying and establishing, and shades imperceptibly into it. The work presents something more than an unusually suggestive working hypothesis.

The book is divided into five parts treating of introductory conceptions, instinct, impulse, reason and certain relations between instinct and reason, respectively. First, as to method. While our point of view is similar to that which an utter stranger to our planet might be conceived to assume in order to arrive at some intelligible account of human and animal conduct—a purely objective point of view; nevertheless we are not far from the mental series at any time. The doc-

trines that mind acts on body, that body acts on mind, that both are aspects of one fact (epiphenomenon theory), and that the two series of events are simply parallel, are passed in review and criticized. Psychic states constitute part of the conditions of processes in the motor centers of the cortex, and we are forced to one or other of two conclusions—either the psychic effect alters the sum-total of physical energy in the brain (which cannot be accepted), or there is something psychic in all causation. Our brain processes, moreover, constitute a group of mental states within that larger group called objects of the outside world, and it is between this small group within a group of mental phenomena and the remainder of the content of consciousness that the causal-interaction-theory asserts a causal relation. But this strange hypothesis is not necessary: another—that of parallelism—is equally tenable and serves our purpose well. This theory assumes a psychic somewhat, which the author calls ‘mentality,’ coincident with each neural activity within us. Where neural structures organize into a system, and neural activities become continuous, mentality likewise organizes into a system and becomes self-conscious. The psychic phenomena of double consciousness, hypnotism, amnesia, and hysteria are coincident with disintegrated neural systems. The Ego of psychology is ‘an unanalyzable whole, and part of consciousness’: “the ego and the field of inattention, therefore, would seem to be one and the same thing, the differences in the application of the terms being determined by differences in the point of view.” Neural systems organized under one preëminent system constitute the brain, and under certain conditions may be functionally separated from their connection with the preëminent system. The destruction of ‘association fibers;’ the bearing of neuro-psychological rhythm, as developed differently in different systems and at different times, upon the phenomena of normal and disunited consciousness; and the differences between this view and the old ‘mind-stuff theory,’ are discussed in some detail. The discussion of parallelism closes with a few brief but interesting metaphysical suggestions in the form of questions. The entire chapter, although of course not absolutely new, is vigorous and courageous.

The last discussion of Part I. takes up general definitions of instinct, habit and reason. “Instincts are forces \* \* \* which appear in us because we are organisms; \* \* \* which are more or less thoroughly coördinated” (p. 68). They have been acquired by the race “because in the long run they have been, as they in general still are, valuable to life under the conditions which normally arouse them.” (p. 70): Habits may be called pseudo-instincts which have been

learned, not inherited, by the individual. Reason, on the other hand, is "that which leads us to adapt ourselves to new conditions, to guide and change the actions which are determined by instinct, and is what we may call the verdant factor in psychic life" (p. 70). Reason covers intelligence—"we act intelligently when we would override and vary the actions to which we are led by our organic instinct" (p. 80).

Part. II., Concerning Instinct. Instinctive actions are not dependent in any degree upon our appreciation of the advantages they bring us; and we are here not concerned with the question as to how they originated. In any cell-aggregate two influences will always be operative in the determination of conduct: first, "the elemental variant influence which would lead any cell to act for itself alone," and second, "the modifying influence from the aggregate of which the stimulated cell is an element." In the higher forms of organic life, those activities which are determined by the influence of the aggregate are instinctive, while those which are determined by the elemental variant influence are reason (p. 109 ff.). All congenital series of actions determined by the constitution of the organism, and subserving definite biological ends, must be classed together as instincts (p. 87); and the presence of some biological end subserved by the instinctive activity is the all-important thing. Determination by organization, definiteness of reaction, should not for a moment be made a differentia of instinct (as by Professor Morgan) (p. 90). Fixity of reaction is only an ideal seldom reached, but the biological end is fixed, and this is the objective mark of instinct. The subjective mark of instinct is the absence of any influence from the conception of the biological end. Not the particular act, but the *trend* of many is the truly instinctive thing; the former varies, the latter is constant. Impulse is a plain state due to the inhibition of instinct-actions, to the failure to carry out distinct images of motor activities. Impulse should always have a subjective significance in psychology.

The term instinct applies, also, to the activities of parts of organisms, where these are in unison with the activities of the entire organism and occur in response to regularly recurring stimuli, as, *e. g.*, the activities of the heart, lungs, etc. All the instinctive activities are automatic, and their psychic concomitants merely form part of the unanalyzable psychic mass called the Ego. Fundamental in organisms are the instinct-actions toward advantageous stimuli, and away from disadvantageous stimuli. Instincts are classified into three groups: those tending to the preservation of the individual, those tending to the pres-



ervation of the race (sexual instincts), and those tending to the preservation of social groups to be found among many species of animals.

First, those tending to preserve the individual. Owing to the uniform dependence of organisms on their environment, certain instinct actions are universal, and receive definite names. The corresponding instinct-feelings are emotions such as joy, sorrow, dread and relief (p. 113). Corresponding to instinct-movements toward objects and other organisms, love appears subjectively; corresponding to movements away, anger and fear; to the instinctive functional adjustments of the sense organs to objects, surprise. Another group of instinct-actions is Professor Baldwin's 'self-exhibiting reactions'; but this group is so rare, irregular and weak that the corresponding instinct-feelings fail to be realized. Marshall does not favor the 'back-stroke' theory of the emotions; he assimilates emotional expression and emotion to the categories of 'instinct-action' and 'instinct-feeling.' Differences of muscular reactions in expression do not make the differences in emotional states which the back-stroke theory would lead us to expect. The emotion is the psychic coincident of the total reaction of the neural system concerned at the moment of emotional expression. In general, all individualistic instincts must be subordinated to those which relate to the persistence of the species to which the organism belongs, just as the reactions of the elementary cells for their own benefit get subordinated to reactions for the good of the organism as a whole.

Instincts relating to the preservation of the species are the second group considered: these are the instincts pertaining to reproduction. Here come up for consideration such topics as sexual pursuit, self-exhibiting reactions that attract, mating, the protection of mother and young, and instincts of the 'deferred type.' The forms in which these groups appear in the higher organic life of man are discussed at some length. Individual variant instincts may become rational ends, as when a student or professional man suppresses the reproductive instincts in the effort to secure personal ease, or freedom from the cares of ordinary family life. "Evidently, we see here very clearly the relation of intelligence, of the reasoning process, to elemental variance" (p. 138).

The third group consists of instincts relating to the persistence of social groups. Here the different forms of coöperative conduct, such as attacks made in combination by ants, wolves and men, herding for facility in finding food, herding for defence and offence. In man we see forgetfulness of self, family, etc., in times of war; monogamy; personal loss suffered rather than commit murder; hunger rather than

theft; benevolence and art instincts tending to the advantage of the race rather than to that of the individual. In all this the individual's advantage is either indirect or entirely absent. Here, intelligence and reason interfere only in the interests of unpatriotic tendencies, violation of the marriage relations, etc. Ethical impulses are instinct feelings which have no individualistic significance. The inhibition of impulses to kill enemies and to commit adultery is due to instinct—violators are simply atavistic. Sympathy and pity, philanthropy and art are here discussed. But in this higher sphere, Nature's problem becomes complex; the same sets of circumstances can seldom recur; consequently only certain trends of action persist; but thought of the trend of instinctive action destroys the force of the impulse. Individualistic instincts reassert themselves in killing, licentiousness and theft, and in all of this the effects of reasoning are most marked. But there is a possible serious hesitancy, a sincere doubt, solved by rational argument.

Apart from these groups of instincts, those which have to do with the relations of other instincts, such as imitation and play, are mentioned. Imitation belongs to a complex instinctive type, and is not identifiable with the 'circular process' which Professor Baldwin would have us call imitation.

Throughout this discussion, the assumption has been determinant that the race instincts develop out of and upon the individualistic instincts, and that the social instincts develop out of and upon the other two groups. The subordination of the first group to the second and of both to the third is a necessity of the race and a universal fact. Hence the conception of a hierarchy of instinct-efficiencies established and preserved by Nature by the method of natural selection.

But how is this hierarchy to be established and preserved in the individual? Granting that those in whom it does not appear tend to disappear from society, it is more conceivable that all should disappear than that such a hierarchy should spring up by chance. Can the social organism be said to exert the necessary control over the individual? Chapter VII. is devoted to the task of showing that society, although organic, is analogous merely to those low forms of organic life which grow by accretion of like elements and which exert but little influence, as organisms, upon the individual elements. It is a matter of indifference whether we compare society to psychological or to physiological organisms: the two correspond, or rather the one is dependent upon the other (p. 183)—a statement which does not seem perfectly consistent with the picture given on page 34 of the relation

of one series to the other. Why are societies analogous to low rather than to high forms of organic life? The reasons given in the work do not seem at all adequate. For example, among others, the point is made that in the higher organisms the life of the parts (the heart, *e. g.*) depends upon the life of the whole, while in society individuals live on whether the social organization lives or not. Now it seems as though the judicial or legislative functionaries of society would be more analogous to the heart than is the individual citizen. It is a question whether the absolute separation from each other of the individuals composing society would not involve their death *as social units* just as the separation of the cells composing an organism involves their death *as cells*. It seems like bad logic when the author reasons from this fancied analogy to the conclusion that there is little likelihood that the race will ever attain to high social organization; and again, when he reasons that if there were such a thing as a social consciousness, the individual could no more know it than a sensation can appreciate our higher life of reflection. This chapter's significance for the argument is its rejection of the thought that social suggestion and control preserve the hierarchy of instinctive efficiencies which the theory demands.

In the next chapter, the eighth, the tendency to variation in social aggregates is represented as excessive. Under the special stress of unusually strong stimulation, and wherever the restraints due to social instincts are removed or weakened, the individual tends to act as an individual. Reasoned processes are the latest and highest developments of this variant principle; but ratiocination is not an important determinant in the struggle for existence (p. 204). Racial and social instincts can be accounted for on the hypothesis that the result on the whole is better individual adaptation for existence in an environment. But we cannot help recalling attention to this point, for natural selection does not seem self-consistent here. What was, to start with, a struggle of the individual with his *individual* environment seems to be unconsciously understood, when the argument demands it, as a struggle of the individual with the environment *of the social group*. The discussion, not being concerned with origins, does not tell us why the individual stops struggling with that part of his environment constituted by the remainder of his social group, or, in other words, how he comes to identify himself with the social group to such an extent as to fail to discriminate between the two environments. Professor Huxley maintained that an egoistic struggle for existence could never become so intense or far-sighted as to develop into an altruistic sacrifice



of self: the survival of the fittest is a law of individual survival, and we do not know that any one has shown how it can become a law of the survival of social groups. In the sphere of psychological, ethical and social evolution, the law of natural selection explains very little. The discussion, however, establishes the need of some controlling instinct to preserve the hierarchy of instinct-efficiencies, and the point which I have above disputed is not absolutely essential to the argument.

This controlling or governing instinct is to be religion; and chapter nine discusses the question, Is religion instinctive? Actions expressive of religion are organic, and subserve biological ends, and these are the marks of instinct. It is not necessary that they should be definite and regular: the higher instincts are rarely so; and we should consider the fact that religious activities are practically universal in the race, although it is not necessary to the argument that they should be entirely so. The function of religious expressions is, to restrain the tendency to over-variation from typical forms of reaction and to emphasize the order of impulse efficiency developed above. To understand this, we should consider instinct-actions, and neglect both origins and beliefs. Seclusion, fasting and self-torture are three typical forms of religious expression. They are not in themselves advantageous to the individual nor to the race, rather the opposite, and yet they have persisted. They have persisted because they tend to produce a quiet of soul and a reduction of physical vigor which favor the hearing of the 'voice' within which is the voice of our racial and social instincts. The fact that religious illuminations seem to come from without is due to their hallucinatory character. Exhaustion from hunger, fatigue and self-torture make the zealot peculiarly susceptible to hallucinations. By these activities the individual instincts are suspended and the social and racial instincts are permitted to make themselves felt. There may have been, must have been a time when the racial (sexual) instincts needed this religious support in order to the perpetuity of the race and its proper development, and hence the various forms of phallic worship which have appeared in the past. Submission to the Power that guides the universe is involved in all three forms of religious activity.

Prayer persists because, in the silent seclusion of the closet, with the attention fixed upon some concrete or ideal object of wide import, the tendency to individualistic reaction is repressed and the suggestions from man's deeper nature are emphasized. Sacrifice has a like value. Celibacy and pilgrimages either produce the same effects

upon the soul or favor those forms of religious expression which do produce them. The efforts of those who hear the 'voice' to enforce their admonitions on others take the forms of teaching, temples and mysterious ceremonials. Purifications and lustrations, initiations into religious brotherhoods, stimulations to the æsthetic sense, such as processions, pageants, songs and temples, are all discussed in this connection. The analysis of these religious phenomena is very suggestive; but it is undertaken in order to show that religious exercises subserve biological ends, and many will feel, doubtless, that the analyses appear plausible only *after* we have assumed that religious exercises do subserve biological ends. Our problem is that of conceiving, rather than that of demonstrating, relations, and it is a fact that conversion and other religious phenomena seem to be empirically connected with puberty and the development of the social and racial instincts; but if the discussion of the function of religion were intended as an argument to show that religious exercises subserve biological ends, it could scarcely stand before the charge of reasoning in a circle. And yet, unless we take this discussion as an argument (as we cannot do), the author has not shown that religion is an instinct. Perhaps many, again, will feel that the organic character of religious activities was not clearly enough established to warrant the conclusion based upon the point.

Part III., Concerning Impulse. Impulse is the subjective aspect of the objective inhibition of an instinct-action. The analysis of craving and desire in the light of this definition follows (p. 348). Every man represents a hierarchy of impulses corresponding to the order of subordination of the instincts, and this gives his ethical standard for the moment (pp. 358-362). Wherever the efficiencies of opposing impulses are equal, my 'egohood' decides, and I will which I shall follow. (The ego is identical with the field of inattention.) We never act contrary to the ethical standard of the moment, but this varies from moment to moment and from man to man. For each man at each moment there is an individual standard of the moment; but a relatively stable individual standard arises in moments of reflection and restraint from immediate action. This forms the basis of mature ethical judgment, a third standard; but this one also changes with the environment and with habit (p. 372). Social influences, however, give rise to the conception of the ethical standard of the most highly moral man of whom we can conceive, and this standard, though variable, is relatively stable.

This standard is not, however, the basis of conscience. "Consci-

ence is the protest of a persistent instinct against its inhibition by a less persistent, but, for the moment, more powerful force" (p. 388). Conscience is itself not an instinct, but a relation between instincts (p. 408). Besides the ethical conscience we have also patriotic, æsthetic and various pseudo-consciences. True conscience tells us of instinct, while a pseudo-conscience tells merely of organized habit (p. 395). Conscience, the sense of duty, remains always the same, but we find a new development of conscience in connection with the development of the religious instinct (p. 397). "The existence within us of a sense of duty as it is experienced in its fullest form, is conclusive evidence at the same time of the existence within us of the religious instinct" (p. 398).

Part IV., Concerning Reason. By this process we are to understand "the capacity found in animals, and in ourselves as animals, to act apparently in opposition to, or, at least, without reference to, instinct" (p. 414). The distinction is made between 'reasoned' or 'intelligent actions' and 'reasoned' or 'intelligent feelings.' The pursuance of future ends and the choice of means for their attainment are the marks of reason. Reason is marked by choice (an objective *result*), and choice is the evidence of will on the psychic side both in ourselves and in the lower animals. Reason in germ or in complex form must be a process as wide as psychic life. Reason and will are indissolubly connected, all rational processes ending in will, and all volition being at the moment of volition rational (p. 424).

All reasoned action, again, must be 'referred back' to instinct-action—it is action according to an older, simpler and more highly organized instinct, relating only to the stimulated element of the organism when some more recent, more complex and less highly organized instinct relating to the organism as a whole or to the social organism would have asserted itself, had not immediate and decisive action been made necessary by the nature of the stimulus. "All reasoned actions must also be referred back to and appear as modes of that simplest of all phenomena of activity, the reaction of a single cell to the stimuli from its environment" (p. 438). The distinction between instinct and reason is really not fundamental; it is the distinction between a typical reaction and a variant reaction. Reason represents the influence in organic life which breaks down our complex inherited tendencies.

Consequently, our inherited impulses are a safer guide to right conduct than reason, as a general rule. That instinct is of higher import than reason, is the burden of nearly one hundred pages at the close of the work. But the ethical impulses are not instincts accord-



ing to the author's definition—they deal with relations between instinct actions—and religious expressions are instincts which have to do with the preservation of a proper order of instinct-efficiencies. Hence arise the last problems of the discussion, viz., the relations of morality to religion on the one hand, and of both to reason on the other. As to the first problem, morality is practically ineffectual except it be religious, although, in our theorizing, ethical conceptions are the logical basis of religious opinions, so that the latter grow in adequacy with the growth of moral experience and thought. Hence the importance of the utmost conscientiousness in life, if we would not fail of the best attainment possible for us. It is possible for an intensely religious person to be immoral, and hence such anomalies as the prayer of the thief for success in his present attempts to filch his neighbor, etc.

As to the relation of religion to reason, the accumulated wisdom of the ages is, on the face of it, probably of greater worth than the thought of any single individual in any particular community at any particular time. We ought, in the interest of progress, to listen to reason, to reason freely and fearlessly, indeed, and to take risks; but we should never forget that in doing so we *do* take risks. In searching for a rule of conduct we have the following as a final word: "Act to restrain the impulses that demand immediate reaction, in order that the impulse order determined by the existence of impulses of less strength, but of wider significance, may have full weight in the guidance of your life. In other words—*Be Religious.*"

The theory thus presented with as little comment as possible speaks for itself, but we desire to ask a few questions. First, as to the objective method adopted. Does it not make it impossible to use some facts and distinctions which are essential to the discussion? At some places the author himself has departed a little from the rule of perfect objectivity to consider subjective marks of instinct, etc. One asks himself, for example, for the subjective difference between instinct and reason. If we regard the two marks of organization and subservience to biological ends purely in the objective, why may we not show that reason itself is instinctive? Construing these marks objectively simply, one feels that it would be easier to show that reason is instinctive than that religion is. Indeed, the rational process does creep into the tents of instinct very frequently, and it seems to be merely the necessity of a 'variant factor' in the theory that prevents reason from stalking boldly into the main street of the opposite camp.

Again, this hierarchy of instincts and impulses, which seems so definite in its subordinations and coördinations when considered as an

objective phenomenon' what is it from a subjective point of view? Some will feel that the author's picture is as much removed from mental experience as are, *e. g.*, the rules of the syllogism in formal logic—I mean, of course, the mental experience of the race as well as that of the individual. To illustrate my meaning, take the order of appearance of the different ranks of instincts and impulses, as depicted by the discussion. Is it true that the racial instincts appear after the individual and before the social, or that the social appear as far behind the individual as the theory demands? It seems more than questionable to some. Moreover, the discussion assumes that there is always in the individual a strong tendency to revert to the individualistic type of reaction: the biological end of religion is to counteract this tendency. But it is an open question whether the individual is thus individualistic at bottom, as a matter of psychological fact. The genetic distinction seems to some to be between the more and the less rational. Professor Baldwin and others have watched children with these various theories in mind, and have been surprised to find what may be called social reactions (using the terms in Mr. Marshall's sense) as early as individualistic reactions, after excepting those which are purely organic and which occur before the child can be said to be any more psychic than a plant. There seem to be many reasons for believing that this is true of the race as well.

Again, how many of the author's social reactions are instinctive, and how many are due to suggestion and imitation? Shall we include the latter social reactions in the class of instincts? Mr. Marshall seems to say so. But do they not belong to the category of variations, leading to the modification of old ways of reacting to stimuli? Are we to broaden our notion of instinct so as to include the organized reactions of the social group to its environment, as well as those of the individual to his? If so, then Professor Baldwin may grant that imitation in this sense is a complex instinct (as Marshall maintains) without abandoning his own position that it is not an instinct. It comes to be a question as to the meaning of instinct, and perhaps some will feel that Mr. Marshall makes the word far too wide.

Lastly, as to Reason, the 'variant factor in psychic life.' Does not every instance of reacting to old stimuli in new ways, of adaptation or accommodation, belong to the category of reason as here understood? Reason covers intelligence, and the marks of both are selection and biological aim (as objectively discoverable in the results of the act). Mr. Marshall says that reason must be coextensive with psychic life. But what of those primitive acts evidencing selection and aim, which

give rise to so many other instincts—*e. g.*, the expanding and contracting movements which many assume to be the organic correlatives of pleasure and pain? These are classed by our discussion as instincts (p. 109ff.). They must belong to both categories, as the author uses them. This same difficulty appears in the analysis of reason later in the work. Reason is the variant factor in psychic life; but in discussing the subject on page 448 we read that variation is sometimes ‘produced immediately as the result of a very forcible stimulus,’ and then it does not ‘involve any previous effects upon consciousness at all’—*i. e.*, it is not instinctive. Lower down, on the same page, we read that “all variation is determined finally by instinctive reaction, divergences being due to differences of width and complexity of the organic systems involved.” On pages 79 and 80 we read that activities determined by the influence of the aggregate are instinctive, while those due to elemental variation are ‘reasoned’ in the higher forms of life at least; but page 439, “the distinction between instinct and reason is indeed not fundamental.” On page 449 the basis of the emphasis of the partial impulses connected with variation is the *stimuli* which determine the impulses, “and here we find ourselves dealing with the essential processes of reasoning.” The difficulty here is not merely a verbal one. Reason tends always in the author’s discussion to disappear in instinct; but if reason is instinct, and if variation is reason, then how is evolution possible? I do not see how we can determine the relation of instinct to reason without considering the question of origins, which the author everywhere rules out of the discussion.

We wish Mr. Marshall had discussed the ‘circular reaction theory’ of the biologists and of Professor Baldwin and others. In this circular process which they understand to be the law or essential method of organic and of psychic life, there are both aim and selection, both organization and biological end; but it seems to fall into both of Mr. Marshall’s categories. If this process is defined as one which repeats its own stimulus, and if this is the typical form of all psycho-physical reaction, then socially coöperative conduct ought to develop in organic life side by side with individualistic conduct. In other words, if the circular process is a true conception, I do not see how Mr. Marshall’s view of the relations between the various instincts can be maintained.

The author regards reason, will and choice as coextensive with psychic life; but is not this a needless broadening of the meanings of terms?

I may have failed to grasp this complex and interesting theory in



its details; but, if not, it would seem clear that the method of the work is too 'objective' (it is deductive), and that the terms of the discussion are used too loosely.

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*La Psicologia Contemporanea.* GUIDO VILLA. Turin, Fratelli Bocca, 1899. In 8vo. Pp. xvi+660.

The object of this volume, as explained by the author himself, is to give a clear account of the present condition of psychological studies in those countries where the scientific study of the mind is most in favor, as in Germany, in England, in the United States and in France (preface). The author apologizes for not having said much of his own country. He seems to believe that, thus far, the contributions of his countrymen to the advancement of scientific psychology have been scanty and insignificant. Signor Villa remarks that most of the Italian psychologists are, properly speaking, philosophers; that the dominant tendency in psychology is still that of the 'inner sense' which goes, with certain authors, as far as a purely spiritualistic conception of the mind. It is only a short time since works were being published on physiological psychology, like those of Sergi, De Sarlo, Buccola, Faggi. But, he concludes, the new psychologists, by going to the opposite extreme of the spiritualistic philosophers, seem to be occurred into such an intensity of materialistic beliefs as to lose the equilibrium of feeling required for dealing scientifically with the facts of mental life (pp. 78-79). This, with the exception of a few scattered references to books published in Italy on various psychological topics, is about all he says concerning the Italian psychologists, in a volume of more than 660 pages.

For such a shameful lack of enthusiasm over the work of the Italians in the field of psychology, Signor Villa has been very severely reprimanded by Professor Sergi. (*Il Pensiero Nuovo*, Vol. IV., 1899.) Sergi claims that Italy can stand comparison with any other country in regard to psychological work. As a proof, he recalls the fact that a laboratory of experimental psychology was founded at Reggio Emilia by Tamburini as far back as 1880, that psychological laboratories were soon after organized in the Universities of Naples, Rome and Turin, in connection, respectively, with the chairs of psychiatry, anthropology and physiology; that since 1879 the first work on physiological psychology was published in Italy by Sergi himself, the same work being later on translated into French by Ribot and Es-

pinas and published in Alcan series. Sergi further recalls the names of the most prominent contributors to the advancement of experimental and comparative psychology in Italy: Vignoli, Lombroso, Morselli, Buccola, Mantegazza, Mosso, Luciani, Tanzi, De Sanctis, Patrizi, De Sarlo, Ferri, Ferrero, Sighele.

We are willing to concede that Signor Villa has somewhat exaggerated the insignificance of the Italian contributions to scientific psychology, probably for the purpose of pleasing certain old caryatides who control the distribution of university chairs. We will also admit that it cannot be asserted, as Villa does, that the tendency still prevailing in Italy in the study of the mind is that of the 'inner sense.' This is only true in reference to a few men who teach psychology as a part of philosophy in some of the universities. These fossils, however, cannot be made to represent the whole movement of studies and research in psychology which is growing, outside of the philosophical faculties, chiefly in connection with the chairs of nervous diseases and physiology. But we cannot share Sergi's indignation at Villa's alleged lack of patriotism. There should be no 'chauvinisme' in a discussion of this kind. Nobody can deny that scientific psychology has found, even in Italy, a number of competent students. But we cannot seriously claim, as does Sergi, that Italy stands second to no other country in the line of psychological work. Psychology has not even gained in Italy a recognition as an independent 'natural science' in the university curriculum. It is still a branch of philosophy. Consequently, there cannot be specialists devoting their life to psychology alone. Occasional psychologists, however intelligent they may be, are recruited among physiologists, neurologists, anthropologists. Thus, psychological research appears to be a sort of by-work. There is absolutely no sign in Italy of a movement of psychological studies and researches comparable to that flourishing in Germany or in the United States. Partial attempts, scattered and isolated efforts, in a word, something which is undoubtedly growing, but is still very immature; this is the real condition of psychological studies in Italy to-day. The fact recalled by Sergi, that a psychological laboratory was founded in Italy in 1880, is not a conclusive argument. The important thing is not to have so-called laboratories, as a novelty imported from abroad through a sort of scientific snobbishness, but to have students who do nothing but psychological work and who make discoveries of new facts. What are, then, the original contributions made by Italian psychologists which can be said to mark a step onward in the building up of scientific psychology? Is there any Italian work

to be compared to those of Wundt, of James or of Ribot? Of course, I am speaking of purely psychological works. Golgi, Luciani, Mosso, Morselli—men of whom Italy is justly proud—cannot be considered as psychologists as long as physiology and psychiatry are to be looked upon as sciences totally distinct from psychology.

Signor Villa's book is a work of popularization. If advanced students and trained specialists have nothing to learn from Villa's résumés, beginners will find it a useful guide to the study of psychology. In the conditions now prevailing in Italy a book like this may undoubtedly help to arouse interest in psychological research, to extend the circle of psychological students, to bring the last results of experimental work abroad within the knowledge of a wider range of persons. Such a work had never been attempted in Italy. We are far from saying that Villa has succeeded in his difficult task. He is undoubtedly a conscientious worker, but sadly lacks the talent of distributing his subject-matter in the most convenient and suggestive form. The book is, therefore, full of unnecessary repetitions, it is heavy and cumbersome, its reading is tiring through prolixity and overabundance of 'historical stuff.' But, in spite of that, Villa's patient and truly meritorious effort deserves the warmest encouragement, and we cannot help declaring that Sergi has been utterly unjust in accusing Villa of ignoring the laboratory and of being a 'dilettante.' That Villa is fairly well informed of the results of experimental work and of the physiological facts bearing on psychological research is abundantly proved by his chapter on 'Psychological Methods,' and by the clear account which he gives of the recent discoveries in nervous histology by Golgi, Ramon y Cajal and others, in the chapter on 'Mind and Body,' one of the best in the whole book.

The following are the general headings of the chapters: Introduction; I., Historical Development of Psychology; II., Conception of Psychology; III., Mind and Body; IV., The Methods of Psychology; V., The Psychical Functions; VI., Consciousness; VII., Psychological Laws; Conclusion.

Signor Villa is thoroughly acquainted with German psychological literature, especially with Wundt's works. He gives, on the whole, one of the best résumés of Wundt's doctrines as unfolded in the three standard psychological works of the great German master and in all his monographs published in the 'Philosophische Studien.' Especially worthy of notice is the résumé of Wundt's theory of Will (pp. 432-438); all the more so, as Wundt's theory of will is one of the most important elements of his interpretation of the facts of mental



life and cannot be found in a coherent and unique statement; but it is to be reconstructed from nearly all his works, and chiefly from the 'System der Philosophie,' the 'Grundzüge der Physiolog. Psychol.,' and the 'Grundriss der Psych.' Villa is also well acquainted with French and English literature. But he is a determined follower of Wundt's doctrines. Perhaps his admiration for that powerful intellect carries him too far beyond the limits of 'rationabile obsequium.' Wundt's doctrines are for him the alpha and the omega of psychology. He tenaciously clings to the presupposition of 'psychophysical parallelism,' but fails to understand that a provisional empirical assumption, justified only by the adoption of the 'natural science' standpoint in psychology, cannot be transformed into an imperative dogma without overstepping the boundaries of science and running into metaphysics. He says (p. 413) that there is an *absolute* difference between the physiological phenomenon and the psychical process. But, by emphasizing the hiatus between the causal series, by vigorously asserting the irreducible difference between the elementary facts of both series, he helps to accentuate what has always been the weakest point in Wundt's system—*i. e.*, the impossibility of conceiving a 'parallelism' where experience shows 'dependence' of one series (the psychological) upon the other (the physiological). Parallelism presupposes the independence of the two orders of fact. But what becomes of the psychical process if the nervous system disappears? The truth is that the assumption of a psychophysical parallelism, alleged to be a merely empirical statement of facts to be provisionally and uncritically accepted as the starting-point of scientific psychology, has resulted, in the end, in a desperate attempt to preserve, in a new and insidious form, the postulates of spiritualism. When disfigured through dogmatism and forced into the turbid region of metaphysics, the principle of psychophysical parallelism must necessarily end in a puzzling enigma. If, as Villa declares, the origin of the mental fact coincides with the origin of life on earth, so that the two series of facts—the mental and the vital—reveal their alleged parallelism throughout the whole animal series (p. 656); if we can explain the biological phenomenon as a result of highly complex chemical processes, which in turn may be traced back to the general laws of physics (p. 658); if, on the other hand, we cannot explain the elementary psychical fact as the result of the same physico-chemical agencies producing life (*ibid.*); then the origin of the mental fact remains unexplained as something which springs up *ex nihilo* while life, its concomitant, has definite antecedents. We cannot escape the 'impasse'

without prolonging the psychical series beyond the limen of life into the inorganic world, just as we prolong the biological series down to the lower plane of chemical processes. We must, in other words, attribute to the inorganic world some sort of unconscious mentality. Thus, when pressed too far, the principle of psychophysical parallelism leads directly to some new form of animism, hence to pure mysticism.

But this is not the place for discussing, in an episodal way, such momentous problems. We will simply add that Villa does not seem to be acquainted with recent American literature as well as with the German. Of Professor Baldwin's works, he seems to know only the 'Handbook' thoroughly. The brilliant studies on 'Child-Psychology' seem to be known to Villa only by the title (pp. 84, 89). He is ignorant of Professor Baldwin's most recent work 'Social and Ethical Interpretations in Mental Development,' thus missing one of the most important contributions of American thought to the advancement of both psychology and sociology, a work which, together with 'Les Lois de l'Imitation,' by Tarde, marks a critical moment in the growth of social science. I have also noticed lack of exactness in biographical information regarding prominent American psychologists. For instance, he gives Professor Münsterberg (pp. 86, 125) as lecturing at Freiburg, and Professor Baldwin (p. 83) at Toronto. The ignorance of Tarde's and Baldwin's works accounts for the extreme vagueness and confusion we have noticed throughout the book in regard to the conception of social psychology and to the relationship of social psychology to sociology.

In conclusion, we will say that Signor Villa's book, taken all in all, is a conscientious work which, despite the author's most decided infatuations for certain deceptive Wundtian formulas, might become a very useful guide to beginners, if the author, in a new edition, would use the scissors freely in order to suppress all the unnecessary repetitions which make the book so voluminous in its present arrangement. A carefully prepared index would very greatly increase the usefulness of this work.

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*The Elements of Sociology.* FRANKLIN HENRY GIDDINGS. The Macmillan Co., New York and London, 1898.

This volume "is not an abridgment of the author's 'Principles of Sociology,' but is a new book." To the psychologist the most sig-

nificant feature in the work as compared with the 'Principles' is the increased prominence given to social psychology. The 'Principles' devoted a large space to discussion of the scope and method of Sociology. The present work makes a great advance toward organizing all the manifold phenomena with which Sociology has attempted to deal, and viewing them as manifestations of a single principle, which, it is needless to say, is that of 'consciousness of kind.' Whether sociologists are likely to take kindly to this tendency to make their science essentially a Social Psychology is for them to say, but it cannot fail to interest the psychologist.

As in the 'Principles' so in the 'Elements,' the author does not seem to recognize any more recent psychology than that of Mr. Spencer and the associationists. "The unit of investigation in the study of consciousness is sensation, which is the simplest of all mental facts." But the most serious objection to the author's central principle seems to me to be that it is a clear case of the psychologist's fallacy. It not merely makes the whole motor force of human societies pre-eminently cognitive rather than impulsive in character, but it assumes that the like-mindedness by which people become co-workers is the product in large measure, at least, of their recognition that they are alike. To put it in a form which is more extreme than Prof. Giddings' statement, but which after all is quite in the spirit of his general thesis: people have common interests because they discover they are like-minded, instead of discovering that they are like-minded because they have common interests. The primacy of the intellectual or of the impulsive aspect of consciousness is the matter at issue, and the biological evidence seems to point increasingly to the latter alternative. Some of the particular illustrations of the power of 'consciousness of kind' strike one as remarkably devious paths for explaining simple facts. Thus, for example: when two strangers meet unexpectedly "there is either a shock of unpleasant feeling or a certain thrill of pleasurable feeling." "Now the feeling of shock surprise, anger disgust, which may happen to be the experience in the case is beyond doubt due to a very complicated impression of unlikeness which the stranger makes." Even the psychologist who will have naught of Darwin or James in his theory of emotions would be loath to trace all disagreeable reactions to a perception of unlikeness. Indeed Prof. Giddings goes on in the same paragraph to suggest the simpler explanation without any apparent consciousness that this is the case. "The man's appearance as seen with the eye may be repellent or threatening, his voice may grate repellently on the ear." A threatening appearance, a grating voice may



be just our own most prominent characteristic; they will not be any the more pleasurable in a stranger on this account. We dislike the threatening appearance because of the anticipated pain, or because its past associations, individual or hereditary,—not because of its unlikeness. And as regards children, it is the familiarity of objects or persons, or their likeness to those objects or persons with which he is familiar, not their likeness to himself, which occasions pleasure. The child who has never seen a negro may be at first afraid; but the child nursed by the negro ‘mammy’ has no such experience. It is because the negro is unlike the other persons of its acquaintance, is unfamiliar, that the child in the former case feels fear. It is the common *interests* of the primitive family which make the kinsman dear and the stranger an object of suspicion.

Another case in which a devious instead of a simple method of explanation is followed, appears in the account of belief. This is defined as “the confident expectation that what we desire will come true; that what we find extremely interesting in accounts of the past were true.” “This confidence we feel because in a majority of instances the things we have desired and striven for have been realized.” This seems to me an attempt to explain a fact of social psychology by an individualistic hypothesis. Belief as signifying the acceptance for practical purposes of any idea or theory or presumed fact must in the large proportion of cases be based, not on immediate personal experience, but on information or authority of others. The whole possibility of the child’s profiting at all by the past experience of the race or by the larger knowledge of parent and teacher depends on belief in what he is taught. Natural selection as well as social selection would soon eliminate those members of a race who believed nothing except what they had themselves experienced. The antecedents of belief, if not the belief itself, are to be found in any social group of animals, the members of which depend upon each other for news of food or warning of danger.

The ultimate psychological law, according to the author, which explains the fact of consciousness of kind and so of all other social facts, is that “consciousness endeavors to attain painless clearness, or positive pleasure, with a minimum of difficulty.” As one reflects on the work and manifold activities of the world, on the development of civilization by the long and unrelenting struggle, on the ever-widening range of interests that emerge, one is tempted to say that the formula is both too abstract and too simple to be of use for actual explanation. Consciousness cannot be adequately defined in terms either intellectual

(‘clearness’) or affective (pleasure), and the poet was a good psychologist when he wrote ‘More life and fuller,’ as the basal law of human striving.

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*Talks to Teachers on Psychology, and to Students on Some of Life's Ideals.* WILLIAM JAMES. New York, Henry Holt & Co. 1899. Pp. vi + 301.

This book consists of lectures on psychology given, in 1892, at the request of the Harvard Corporation, to the Cambridge teachers, and of three addresses at women's colleges. The latter are not pedagogical, but they are so congruous in subject and mode of treatment that they are properly included in the volume.

The ‘Talks to Teachers’ start from the biological conception of man as an organism adapted to react on its environment. This conception is not offered as a complete statement of the facts. The author is explicit on this point at the outset: “No one believes more strongly than I do that what our senses know as ‘this world’ is only one portion of our mind's total environment and object” (p. 25). In the last lecture Professor James's well-known position in regard to free will is stated: “a belief in free will and purely spiritual causation is still open to us. \* \* I myself hold with the free-willist” (p. 191). The ‘ultra-simple point of view’ is adopted for the sake of the unity and simplicity which it imparts to the exposition. It has the advantage of preserving the continuity between human and animal psychology, and of coördinating the brain life and the mental life as having one fundamental kind of purpose. Whatever higher functions and products the mind may be capable of are necessarily conditioned upon useful adaptations, so that these may be considered the more essential, or at least the more primordial.

But however proper it may be to abstract, as all sciences do, from the totality of phenomena for the purpose of clearer understanding, this procedure is attended with peculiar danger in psychology. To take the senses, a few instinctive impulses, association and the ideomotor function of will, and treat these as the whole of mind, is misleading. “I cannot but think that to apperceive your pupil as a little sensitive, impulsive, associative and reactive organism, partly fated and” (the qualification should be observed) “partly free, will lead to a better understanding of all his ways. Understand him, then, as such a subtle little piece of machinery” (p. 196). “Such is the little

interested and impulsive psychophysical organism whose springs of action the teacher must divine" (p. 62). The frequent characterization of the subject of psychology as an 'organism' and a 'machine,' the emphasis put upon reaction on the environment as the essential thing about it, the use of physiological instead of psychical terms of description—this tends to concentrate attention upon mechanical elements and aspects. The definition of education leaves out of account ideal ends—truth as intrinsically excellent, one's perfection as a rational being, etc.—and insists only on serviceable behavior. "Education cannot be better described than by calling it *the organization of acquired habits of conduct and tendencies to behavior*" (p. 29). In the enumeration of native instincts and tendencies the biological standpoint is kept in view; fear, curiosity, imitation, emulation, ambition, ownership, constructiveness, are adduced—the existence of disinterested impulses is recognized only in the bare mention of love. The expository advantages of the point of view adopted are counterbalanced by an inevitable obscuring of the free activity of mind, and by the ruling out of consideration, for the greater part, of its higher manifestations—intellectual, æsthetic and ethical. This may not be a fair criticism in view of the care taken by the author to prevent misunderstanding; but it is pertinent to ask whether partial points of view, gotten by abstracting from the complete facts, are desirable in psychology; whether, for example, it is expedient to exclude, as Professor James would do, all metaphysical prepossessions and implications. If the facts do not involve these, there can be, of course, no dispute; but those who believe that conscious experience is ontological in essence may well consider whether it is best to ignore this in the discussion of psychological problems.

These lectures illustrate the advantages of entrusting the 'popularization' of science to the hands of a master. The extravagant claims, the incautious generalizations, the profuse use of technical language, with which we are so familiar in works of a certain class, are here refreshingly absent. It is something to be thankful for that instruction and counsel so wholesome and timely as that contained in the opening remarks should gain so wide a hearing. "In my humble opinion there *is* no 'new psychology' worthy of the name. There is nothing but the old psychology which began in Locke's time, plus a little physiology of the brain and sense and theory of evolution, and a few refinements of introspective detail. \* \* \* I say moreover that you make a great, a very great mistake if you think that psychology, being the science of the mind's laws, is something from which you can



deduce definite programmes and schemes and methods of instruction for immediate schoolroom use. Psychology is a science, and teaching is an art; and sciences never generate arts directly out of themselves' (p. 7). "Least of all need you, merely *as teachers*, deem it part of your duty to become contributors to psychological science, or to make psychological observations in a methodical or responsible manner. I fear that some of the enthusiasts for child-study have thrown a certain burden upon you in this way. \* \* \* For Heaven's sake, let the rank and file of teachers be passive readers, if they so prefer, and feel free not to contribute to the accumulation" (pp. 12, 13). If the last quoted remark should deter any too zealous investigator from entering one field, in particular, in which some truly fearful results have been achieved—that of pathological psychology—the service will be by no means small.

It is needless to say that the same qualities of thought and style which appear in the author's other works—not the least of them being a happy incapacity for dulness—are abundantly manifest in these 'talks.' One is inclined to envy Professor James the friends who tell him so many delightful anecdotes, exactly fitted to illustrate his points. A number of stories are given such as would make one's fortune at a dinner-party. If Falstaff were a sufficiently dignified personage, he might be quoted in explanation: "I am not only witty in myself, but the cause that wit is in other men." There is doubtless a delicate concession to the lady auditors in this change of gender: "Anecdotes and reminiscences will abound in all her talks, and the shuttle of interest will shoot backward and forward; another teacher has no such inventive fertility, and his lesson will always be a dead and heavy thing" (p. 96).

The lectures on habit, attention, memory, will, contain all that is most concrete and practical in the corresponding chapters of the *Principles of Psychology*. The use of needlessly mysterious and pretentious words for expressing simple meanings is effectively commented on in the lecture on apperception. The following helpful pedagogic suggestions—a few out of many—may be noted: the transitoriness of instincts (p. 61), elementary defects not fatal (p. 135), too few heads of classification (p. 163), the bulky will (p. 181), two types of inhibition (p. 193).

The 'talks to students,' which constitute the second part of the volume, have these titles: 'The Gospel of Relaxation,' 'On a Certain Blindness in Human Beings,' 'What Makes a Life Significant?' The first is an interesting and persuasive protest against mental and

moral over-tension; the other two insist on the importance of a sympathetic appreciation of the points of view and the ideals of others, showing that only thus is the common life of humanity redeemed from apparent insignificance and discerned in its potentialities of dignity and heroism. An application of the line of thought in the second and third addresses is suggested in the Preface, which may not be entirely agreeable to readers of 'imperialistic' proclivities.

This volume deserves the attention, not only of teachers, but of parents, and of all persons interested in psychology and in education.

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*Conduct and the Weather—An Inductive Study of the Mental Effects of Definite Meteorological Conditions.* Monograph Supplement to THE PSYCHOLOGICAL REVIEW, No. x. EDWIN GRANT DEXTER. Pp. viii+105.

This study is an attempt to throw some light upon the question of the weather in its relation to human activities. The method is for the most part inductive and consists of a comparison of the occurrence of certain data of conduct, under definite meteorological conditions, with the normal prevalence of those conditions.

The study was made for the cities of New York, and Denver, Colo.

The data considered were taken from the various public records of those cities and consist of misdemeanors in the public schools and penitentiaries, arrests for assault and battery (males and females considered separately), arrests for insanity, the death-rate, suicide, clerical errors in banks and strength-tests in the gymnasium of Columbia University. A period of more than ten years is covered and something over 400,000 data considered.

As a basis for the study, the mean temperature, barometer and humidity, the total movement of the wind, the character of the day and the precipitation, as recorded by the officers of the United States Weather Bureau, for each day of the period covered, are used. The occurrence of bad deportment in the schools and penitentiary, of assault and of the other classes of data are then referred to these meteorological conditions, and the exact weather upon which they are most prevalent determined. These relations are shown by means of tables and more than 150 curves.

Some interesting things are shown. Among them, that upon cloudy and rainy days there are less bad marks given in the schools—with the smaller attendance allowed for—less arrests for assault, and even less



suicides than upon clear, dry days. The death-rate, however, increases a little for wet weather. Upon perfectly calm days the latter is high, while all the other occurrences mentioned are below the average. Extremely high winds, too, seem to have a soothing effect, for excesses in conduct are comparatively few at such times. Moderately brisk winds have the worst effect. For high humidities, all the data, except the death-rate, were low. This seems rather strange, for muggy, sticky days are of such a character. A very marked effect is shown by the barometer. With the exception of death, all the occurrences were low for high readings, and high for the reverse condition of the mercury column. Some conclusions are drawn with respect to the relations of the barometer to periods just preceding storms. The effects of different degrees of heat are shown to be the greatest of all, temperature of from 75° to 85° being accompanied by nearly 50% more assaults and other evidences of bad conduct than the normal. Temperatures above 85° show a marked falling off, as if, under such excessive heat, little energy was left for bad behavior. A large part of the paper is devoted to the study of school children. In it are discussed the answers to a questionnaire sent out to nearly 200 teachers in various parts of the country, asking their opinions as to the effects of the weather upon their pupils. These answers are compared with the exact effects shown by the study of the records of deportment in the school registers. The teachers were almost unanimous in their opinion that the weather has its influence not only upon the deportment, but upon the character of the class work of their charges.

A study of school attendance is included in their work, and some conclusions drawn as to the influence of the weather upon the health of the pupils.

The general conclusions arrived at in the paper are that those weather states which are physically energizing and exhilarating are accompanied by an unusual number of excesses in deportment and the minimum of deaths and mental inexactnesses, while the opposite meteorological conditions show the reverse effects.

THE AUTHOR.

*The Emotion of Joy.* GEORGE V. N. DEARBORN. Monograph supplement to THE PSYCHOLOGICAL REVIEW, No. IX., New York, Macmillans. April, 1899. Pp. ii+70.

Starting with evolution, psychophysical parallelism, and the kinesthetic theory of emotion as necessary and basal presuppositions, this monograph discusses the emotion of joy with somewhat of that



detail which every emotion in turn demands. On the one hand, joy is considered in its bodily aspects as dependent on universal biologic inheritance from simpler organic forms, while on the other hand its psychical characteristics are pointed out, and especially those fundamental social relations which constitute so important a part of all human emotional phenomena. The adequate study of an emotion is shown to implicate well-nigh every aspect of biology, in the narrow sense of the term. The research was introspective as well as experimental.

Three years of practical work in the psychological laboratories of Harvard and of Columbia is described in the reports of five distinct though related series of experiments. Three of these investigate and fully confirm the ancient but, for science, hitherto rather vague supposition that pleasant mental states are correlated in the body with contraction of muscles classed as extensor and unpleasant states with flexor activity, the three series of experiments relating respectively to the reactions of the hand and the head, the forearm, and the leg. The hand, because the most mobile portion of the body at our disposal, showed most emphatically the psychophysical tendency in question. The forearm was studied only as to its reaction to pleasantness and unpleasantness in voluntary extensions and flexions through an arc of about forty degrees, while in case of the hand, head and leg only the involuntary and unconscious movements were observed. The stimuli employed were odors, colored light and sound, and these were judged as pleasant, indifferent or unpleasant in seven degrees, 'one' being the most relatively pleasant and 'seven' the most unpleasant. These experiments together number about three thousand, and, considering the practical difficulties of any laboratory research into affective conditions, strongly verify the chief presupposition which from general considerations seemed *a priori* to be so probable.

The much discussed nature of the smile and laugh is hereby explained, early embryonic conditions showing that the muscles active in these phenomena are properly extensor muscles, thus at once explaining the hitherto mysterious fact of laughter and confirming the underlying theory.

The fourth set of experiments deals with the bodily correlation in general of joyous emotion in a novel and more or less productive way, while the fifth series is a research, *quantitative* as well as qualitative, into the vascular and respiratory somatic concomitants of joy.

Emotion, the most complex of human psychological phenomena, is defined as "a temporal portion of excited sentient experience

wherein the subjectivity and the psychophysical attention to the object, real or ideal, are heightened with or without a tone of pleasantness or of unpleasantness, and wherein the feeling and the bodily position or movement are, or tend to be, characteristic and correlative." Emotion properly so called may be found pure, then, only in the 'lower' or simpler orders of life, for in man the vast complexities of his, always social, personality render any such affective period beyond the physiologist's power of description. In the simpler orders of animal life, down to its vanishing-point in the *amœba*, the pleasantness-extension and unpleasantness-flexion principle is more complete; in man, however, with all the complicating and often conflicting tendencies there obtaining, its manifestations may still be regularly observed, with constant exceptions here explained, as the persistence of basal biologic law necessitates.

The regular occurrence of habitual inhibitions, due to complex conditions of civilized social development, supplies the apparent deficiency in the kinæsthetic theory of the emotions of man. Any emotion, being biologically in animals, savages and naïve infants a more or less constant series of phenomena, is theoretically at least susceptible of future scientific determination more or less exact; while the emotional processes of civilized human selves are so complicated by social interaction as to be no longer properly emotions in the biologic sense, but rather concrete expressions of the affective social consciousness at present indefinite and involved.

Analysis discriminates five components of a period of emotion—namely, psychophysical excitement; various feelings and their concomitant bodily movements and strains; heightened consciousness of the emotion's object as in relation with the subject-agent; often a pleasant or an unpleasant tone of consciousness; and at times increased self-reference.

An emotion is an affair invariably of both a mind and a body, practically the whole of the latter of which it regularly implicates: it is universally dynamogenic.

Contraction of the extensor muscles is more pleasant in itself than contraction of the flexors, and this fact, together with the general tendency to flexion which a (naturally unpleasant) sudden shock produces, perhaps determined, phylogenetically, the empirical opposed mode of affective bodily function.

A bibliography of about one hundred and twenty-five volumes bearing on the subject and its relations may be found at the conclusion of the monograph.

THE AUTHOR.



## OPTICAL ILLUSIONS.

*Raumästhetik und geometrisch-optische Täuschungen.* THEODOR LIPPS. Zeitsch. f. Psych. u. Phys., XVIII., p. 405.

*Ueber die Natur der geometrisch-optischen Täuschungen.* ST. WITASEK. Zeitsch. f. Psych. u. Phys., XIX., p. 81.

*Eine einfache physiologische Erklärung für verschiedene geometrisch-optische Täuschungen.* E. EINTHOVEN. Pflüger's Archiv, LXXI., p. 1.

*Ueber geometrisch-optische Täuschung.* W. VON ZEHENDER. Zeitsch. f. Psych. und Phys., XX., p. 65.

The article by Lipps is written in reply to the criticisms made by Heymans in his review of Lipps' book on optical illusions. The special discussions of the particular figures may be omitted in this brief review. They are in part new, in part repetitions of the earlier applications of the principles of weight, bounding activity and the other æsthetical factors of which Lipps has made use in all his writings.

On the more general question of the nature of illusions, Lipps again defends at length the position that illusions are false judgments, not false percepts. They arise through comparison, and it is during the act of comparison that the idea based on the percept—not the percept itself—is so modified by the addition of the æsthetical ideas that it is changed from its original form to the illusory form.

The article of Witasek takes up the problem with which Lipps deals, and attempts to show on theoretical and on empirical grounds, that the illusion in the Zöllner figure, and presumably those in the other figures, cannot be due to false judgments, but must be, in some way, due to modifications in sensation processes.

The paper opens with a comprehensive review of the recent work on geometrical illusions and an attempt to arrange all the various theories in an appropriate scheme of classification. All processes containing illusions are complete only when they close with a judgment. The judgment is based, however, on percepts, and any illusion may be due either to the percepts on which the judgment is based, or to the method of dealing subjectively with the percept during the act of judging. According as the one phase or the other of the complete process is selected as responsible for the illusion the theories may be classed as judgment-theories or perception-theories. But perception is the result of combining sensations. It is possible, therefore, to subdivide perception-theories into these which attribute the illusion to the



sensation factors and those which attribute the illusion to the synthetic process. The nature of the synthetic process is, however, always predetermined by the sensation factors entering into the percept, and so the sub-class which attributes the illusion to the synthetic processes is of small importance.

As between judgment-theories and perception-theories, the writer decides on the following theoretical grounds in favor of the latter. Judgments may be acts of comparison or acts of classification. But a judgment which discovers a difference between two percepts must have found the difference in some actual disagreement present in the percepts themselves, otherwise no ground for judging a difference would be present. An illusion always involves such a judgment of difference; we must then, according to the above, look for the ground of this judged difference in the percept rather than in the process of judgment. In the second place, a false classification cannot be regarded as the explanation of illusions, for it is not a question here of right naming or right grouping under remembered categories: but rather it is a question of the continuity of a perceived category (as, for example, whether a line is continuously straight or not), or it is a question of the correspondence between two cases of the same general category.

The empirical evidence with which the writer confirms his theoretical discussions is derived from two groups of experiments on the Zöllner figure. In the first group the parallels and the transverse obliques were drawn on two separate cards and united binocularly into a single figure. At the beginning of experimentation the observer was disturbed by binocular rivalry, but after practice the writer tells us that he was able to overcome this enough to observe the figures for considerable intervals without rivalry. The illusion was at first lost entirely, but as rivalry was gradually overcome it reappeared and steadily increased in intensity. At last, when rivalry disappeared entirely, the illusion was clearly noticeable but somewhat less intense than when the two parts of the figure are observed in the ordinary way. This decrease in intensity was subjected to quantitative determinations, and proved to be on a general average about 75 per cent. The writer argues: the Zöllner figure percept formed by binocular fusion in the manner described is just the same for judgment as one formed in the ordinary way. The decrease in intensity of the illusion which was discovered was, therefore, not explicable on any judgment-theory. The decrease must be attributed to the change in the conditions of perception. Similar results leading to the same conclusion were secured on

other figures, though there is no detailed report of the other experiments.

The second group of experiments deals with the presence of unnoticed illusory influences. When a single line is crossed by transverse obliques the illusory effect may be present, though it is too small to be easily judged. The method of the experiment was as follows: a mercury mirror was so arranged that cards could be placed on edge on the horizontal surface of the mirror. The cards were thus held perpendicular to the mirror. Along the horizontal edge of the cards, just at the surface of the mirror, were drawn horizontal lines. From these horizontals were drawn perpendicular verticals. The verticals were so reflected in the mirror that they and their reflections seemed to form continuous straight lines. When the perpendiculars were inclined toward the horizontals, or when they seemed so inclined, the line and its reflection no longer seemed continuous, but seemed to form an obtuse angle at the surface of the mirror. There are two ways, then, of judging whether the angles between the horizontal and verticals are right angles: one is by inspection of the angle itself, the other is by judgment of the straightness of the line formed by the real line and its reflection. If transverse obliques, such as those used in the Zöllner figures, are drawn across the vertical, it will be found that at certain angles of obliquity a really vertical line will be so slightly affected by the transverse lines that inspection of the angle does not reveal any noticeable diminution of the right angle, while observation of the mirror line will show that the apparent continuity of vertical and reflection is destroyed. The writer argues that the illusory effect must have been present in the inspected right angle, but it was too slight to be noticed under those relatively unfavorable conditions of judgment. There may, therefore, be a perceptual illusion even when there is no illusion of judgment.

The binocular experiments are quite as difficult to criticise as they were to perform. The fact that rivalry was overcome by practice is a result of importance in itself, and certainly calls for some further investigation. Other investigators have been unable to overcome rivalry by practice. The character of the result obtained under such conditions will always be questionable. And it certainly does not follow, as against Lipps, for example, that such binocular images are equal in value to the ordinary Zöllner figures. The attention must be seriously distracted by the strain of overcoming rivalry, and the æsthetical effect will naturally be reduced proportionately. Or, in terms of Filehne's hypothesis, one might say that the conditions here presented are fur-



ther removed than ever from ordinary perspective drawings, and the effect of tridimensional associations is accordingly much weakened.

The mirror experiments are ingenious in method and tend to establish a fact of importance. That the reflected image does not enter as a disturbing factor is not clearly made out by the writer.

The main thesis of the paper, as opposed to Lipps' contention that illusions are due to judgment, opens up an issue on which it seems impossible to reach any generally acceptable opinion. Conscious and unconscious judgments, associations of all degrees, synthetic perceptual processes, all pass so easily into each other that it is impossible to draw a hard and fast line and say the illusion is here or there. If a group of sensation factors is such as to invite the addition of this or that association factor, and if after the association factor has been added, the subject finds his judgment biased, then there is undoubtedly a sense in which the illusion belongs in every stage of the process. Until agreement can be reached on the more fundamental psychological questions of the relations of sensations to percepts and of percepts to judgments, there will always be disagreements in this special field. The more concrete question of what the association factor is—putting aside now the question of where it is added—is an exceedingly complex one. Recent discussions have all tended to the general impression that such factors may be of great variety even in a single illusion. The writers who depend on movement, those depending on æsthetical motives, those who call in perspective, and finally those who give less generally applicable explanations of particular illusions, are not necessarily in opposition to each other, though the criticisms with which these writers usually introduce their work indicate a general lack of agreement.

Finally, as to the source or motive of the association or other cause of the illusion, every new writer points out some new possibility. It is at this point that we may introduce the last two articles of our list. The paper by Einthoven offers in explanation of illusions of the Müller-Lyer and Poggendorff types, a theory which is allied to the irradiation explanation of the latter figure given by Helmholtz (p. 708, 4th edit.). Einthoven's hypothesis is as follows: Most of the points of a figure cast their images on the periphery rather than on the center of the retina. These peripheral images are made up of diffusion circles, and in judging of lengths and directions the observer is guided by the greatest amount of overlapping of the diffusion circles. Thus in the Poggendorff figure the diffusion circles lead the observer to locate the point of contact of the intercepting parallels and the intercepted oblique within the acute angle.



The explanation does not aim to apply to all illusions, but only to the types mentioned. The freedom with which it deals with indirect vision, which is at best an obscure factor, and the apparently direct vision involved in all illusions, furnish the criticisms of this theory. Figures seen in indirect vision, when attended to at all, are usually interpreted in terms of what is seen when the image falls on the fovea, not *vice versa*.

The fourth paper is a deplorable example of misdirected effort. It illustrates in a very striking way the danger of making hypotheses on the basis of some one's else results. How it could ever have escaped the attention of author and editors that the facts are exactly reversed is hard to understand. Such, however, is the case, as will be seen from the following: The main thesis of the paper is, that the Poggendorff illusion can be explained by certain facts long ago discovered by Volkman. Volkman took two lines: one fixed, the other capable of rotation around its center. He allowed the observer to set them so that they seemed parallel. The result was (and it is quoted in italics by von Zehender) that "*Die Diameter [that is, the lines referred to] welche parallel erscheinen divergiren ohne Ausnahme nach oben.*"<sup>1</sup> That is, lines which seem parallel do in reality diverge at the top. Lines which are in reality parallel will therefore seem to converge. In spite of this well-known fact, von Zehender lays at the foundation of his hypothesis the following statement, in accordance with which all his figures are drawn, and on the validity of which the value of his theory of course depends: "*Die beiden Linien A und B in vorstehender Figur 2 seien die wirklichen Parallellinien, durch deren Zwischenraum die Continuität des Schrägstriches ( $\alpha^\circ\beta^\circ$ ) unterbrochen wird. Nach den Ergebnissen der Volkman'schen Versuche erscheinen diese beiden Parallellinien nach oben schwach divergent.*"<sup>2</sup> The explanation which follows is based on this statement and requires no comments.

In the second part of the paper the writer attacks a somewhat different problem on the basis of certain facts first reported by Opper. The problem is the estimation of the sizes of acute angles. If a vertical and a horizontal line are so drawn that they intersect at right angles in the middle of a visual field, thus dividing the field into rectangular quadrants, and if then the subject is asked to bisect the four right angles thus formed, it will be found that the lines of bisection will always be placed too near the horizontal lines. That is, an angle which has one horizontal edge and is in reality small, will seem equal

<sup>1</sup>P. 70.<sup>2</sup>P. 71.

to an angle with one edge vertical which is in reality larger. The small angle is evidently taken for larger than it really is, while the larger angle is correspondingly underestimated. Here is von Zehender's statement: "*\* \* \* dass (Spitze) Winkel, die sich mit einem ihrer Schenkel der verticalen Richtung anschliessen, irrthümlich leicht für grösser gehalten werden als sie sind, während ebensolche Winkel, die sich mit einem ihrer Schenkel der horizontalen Richtung anschliessen, ebenso leicht für kleiner gehalten werden als sie in Wirklichkeit sind.*"<sup>1</sup>

If the conclusion were to be seriously considered in spite of its wrong statement, it might be objected that two acute angles which are parts of a right angle are hardly suitable examples on which to test the attributes of acute angles in general. But the further consideration of the paper may be omitted.

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*Psychology and the Individual Teacher.* CHARLES H. JUDD.  
Journal of Pedagogy, XII., 136-148. 1899.

The present article is a defence of the value of the study of experimental psychology to the teacher. The general standpoint is found in the relation of the teacher to the child.

Education is analyzed to be in its broadest sense the 'acquiring, arranging and applying experiences.' For such activities the teacher is not necessary. The child by itself will learn something, however little, and instruction becomes necessary only that the individual may economize time and energy in the acquirement of experience. The teacher has, accordingly, a twofold function in dealing 'with the content of human experience' and 'with the child who is to be put into possession of this content.' Material and method are alike indispensable to the teacher. With the first psychology claims to have nothing to do, and it is only from the point of view of method that the science can pretend to be of advantage to the teacher. The author assigns to the teacher the function of training the child to look at various experiences in the same manner as does the adult, and he shows that it is the plan and the duty of psychology to indicate how the higher level may be best and most easily reached. Experimental psychology shows the teacher how to analyze material for the better presentation to the growing mind, and it makes him familiar 'by analogy with the relation of children's mental lives and their external

<sup>1</sup>P. 99.

conditions.' *Rules* for the teacher to obey should not be given by psychology, but *principles* should be established, which may be applied under varying conditions. This last factor, variation, brings out most clearly the value to the individual teacher of the study of psychology. If principles are understood, and some facts underlying the principles are known, new facts will be assimilated and arranged with the old, and methods will be changed accordingly. With fixed rules, however, new conditions find the teacher unprepared by training, and method becomes forced and stilted.

Finally, what the teachers need "is a broad, general course in psychology to bring them back into a vital sympathetic relation with the practical investigation of the child's mental condition. Such training places the individual teacher above the theory."

SHEPHERD IVORY FRANZ.

*University of Iowa Studies in Psychology.* Edited by G. T. W. PATRICK. Vol. II., pp. 163. 1899. Iowa City, Ia. \$1.00.

The present volume includes (1) short studies by Professor Seashore on the Müller-Lyer illusion, a material-weight illusion, localization of sound, acuteness of hearing, pitch discrimination and motor ability; (2) an account of experiments upon the analysis of the perceptions of taste; (3) a discussion of some phenomena of the secondary personality, and (4) the description of two new pieces of apparatus.

1. The first series 'have been selected,' we are told, 'with reference to the need of data, their interrelations, and the adaptation of methods and apparatus.' Some of the experiments are standard ones, 'some have been developed by other investigators and are here developed a step farther, and some are new.'

(a) Various forms of the Müller-Lyer illusion were used to note the effect of the illusion under varying circumstances. The limiting lines were circles, coins, squares and angles. It was found that the force of the illusion decreased with the size of the coin, and when, instead of coins, circles were used the illusion was lessened. Complexity of outline increases the force of the illusion, and "it also appears that the fainter the outline is the more the eye strives to follow it." The introduction of a base line lessened the illusion, and the limiting lines greatly affect the amount of the illusion—circles, 13 %; squares, 1 %— "A vertical distance is overestimated when compared with a horizontal distance." Practice has no effect in increasing or diminishing the illusion if the subject remains in ignorance of its presence,



but there is a decrease in variability. Women are more susceptible to the illusion and are more variable than men. Two hundred children that were tested showed double the effect noted on adults, but no difference was noted for the two sexes. There seemed to be no regular decrease with growth and no general relation with mental ability was found.

(*b*) The material-weight illusion. Each of three cylindrical blocks of wood, iron and cork, of the same size and of uniform weight, was compared with standard sets of blocks and the estimation of weight was noted. Eight determinations were made by each subject with each block. In general the cork and the wood blocks were overestimated and the iron block underestimated. The illusion is about 18 % of the actual weight, and is about the same for women as for men. The essential condition of the illusion is that the preliminary estimation of the weight of the different blocks shall be wrong—*i. e.*, the subject has the autosuggestion that the cork and the wood blocks are light and the iron one heavy; but when lifted the cork and the wood blocks are felt heavier than was supposed, while the iron block is much lighter than was judged. The illusion persisted even when its nature was known, but not so strongly. There seems to be no variation with age or sex.

An interesting suggestion is made that it may be possible to increase the muscular ability by means of the illusion. If the subject *thinks* he is lifting less than what he is actually lifting, would he be as greatly fatigued after lifting this weight one hundred times as he would be if it felt heavier? And, in like manner, may not the maximum effort be increased by means of this illusion? A few experiments show that the maximum effort was affected by the size-weight illusion. "Nearly all who have tried it can lift more in the barrel (a flour-barrel) than in the half-peck measure."

(*c*) Localization of sound in the median plane. A 100 v.d. tuning-fork connected with an induction coil gave sounds in three different places relative to the observer—above, right and left. Strong and weak sounds were given, and sometimes two sounds together. Estimations were made of the distance in feet, and the direction in degrees in the vertical and horizontal planes. There was a tendency to locate the sound produced overhead as 'upward and forward.' Of the fused sounds (right and left together), 25% were thought to be in front of the vertical plane, 73% back of it and 3% in it. 72% of the sounds were located above the horizontal plane, 12% below and 16% in it. The grouping of the subjects into three classes according to the

differences in acuteness of hearing between the two ears showed a marked tendency to locate a median sound toward the side of the stronger ear. When the sounds were in the median plane and their probable location unknown, there was found little ability to locate them properly. The ability was not improved when the probable location was known.

(*d*) Hearing-ability and discriminative sensibility for pitch. In these tests great individual differences were noted. The average hearing ability of the men and women was found to be about equal. The women, however, had much better discriminative ability for pitch. No marked relation between keenness of hearing and accuracy of pitch discrimination was noted. The keenness of hearing of children seems to increase with age, and likewise the pitch discrimination. Some of the differences, however, may be due to lack of understanding on the part of the younger scholars, not to mention the error of drawing conclusions from such a small number of children that were tested. No relation was found to exist between pitch discrimination and mental ability, the distribution of cases seeming to be a chance arrangement. It is concluded that "this is the strongest evidence in favor of the theory that the discriminative sensibility for pitch depends principally upon the natural structure of the end-organ and is subject only to small variation with education." It seems to the reviewer that a more extended series of observations must be made, and the results confirmed ere this conclusion can be safely accepted.

(*e*) Motor ability, reaction time, rhythm and time sense. Fifty-six subjects were tested, and no differences were found between men and women either in rapidity of movement or in the variation. Reaction to sound gave the shortest and least variable times, reactions to touch were next in length and reactions to light took the longest time. Discrimination time—*i. e.*, the whole time less the simple reaction time—was found to be about  $75\sigma$  and the choice time about  $90\sigma$ . The variation in each of these series was about equal. A free rhythm was kept quite constant for 90 seconds by all observers. The pressure with which the rhythm was made constantly increased. The rhythm established seemed to be somewhat determined by the respiratory and circulatory processes. In a regulated rhythm, the subject making taps in conjunction with a mechanical stimulus, there was found a marked tendency to accelerate the movement, and the men seemed to be slightly more accurate than the women. Estimations of empty time intervals of  $\frac{1}{4}$ ,  $\frac{1}{2}$ , 1, 2, 5, 10, 20 and 40 seconds by the method of average error showed an overestimation for the shorter intervals and



(with two exceptions) an average underestimation for the longer times. School children made almost correct estimations of 5 seconds, but underestimations of the 10- and 20-second intervals. No sexual differences were noted in this test.

2. In this article Professor Patrick gives an interesting and valuable account of taste experiments with an anosmic subject. Popularly the taste of any substance is thought to be conditioned largely by taste sensations. It is known, however, that smell, touch and sight play large parts in our taste perceptions. The analyses of the influence of each of these factors have been few and incomplete, and the present study will be gladly welcomed. The several theories regarding the qualities of tastes are noted. The theory that there are four primary tastes, which by combinations and fusions produce an indefinite number of other tastes, was tested with the anosmic and three normal women as observers. Mixtures of salt, sweet, bitter (quinine) and sour (tartaric acid) solutions were used to discover whether such mixtures gave new qualitative tastes or permitted the simple constituent tastes to be perceived. The latter condition was found to be true. All the observers were able to analyze the mixtures with a considerable degree of accuracy. After considering other investigations the author concludes "that the hypothesis which seems at present most in accord with known facts is that there are only four taste sensations (possibly only two); that these remain distinct in consciousness, not subject to fusion or mixture with each other, and that the manifold taste perceptions of daily experience are made up of these four taste sensations, with their grades of intensity, and sensations of smell, touch, temperature, sight, and muscle sensations." Of touch and smell, the more important is probably touch, while sight plays a more important part than has commonly been supposed. In any analysis of tastes various difficulties confront the investigator and the only factor easily eliminated is sight. With normal subjects smell cannot be entirely eliminated even by closing the nostrils, while it is almost impossible to exclude touch sensations. In complete anosmia results may be obtained which are uninfluenced by smell sensations, since these are wholly wanting. The observer used was a woman peculiarly suited by education for such experiments. Blindfolded she was unable to get any reaction or sensation from over twenty-five substances which ranged through the nine classes of smells enumerated by Zwaardemaker. Those substances which could be determined were found to give taste sensations in the back of the throat or to produce touch sensations in the mucous membranes. The observer's taste sensations were then tested with



numerous familiar chemical and household substances, and the results were compared with those from several normal persons. The anosmic was found to be less active in judgment of salt, quinine and acid solutions. She had finer discrimination for passive touch. It was found that the substances that could not be recognized by any of the subjects depended entirely upon visual sensations for their supposed qualities, those recognized by the normal observers evidently depended upon the smell qualities, those recognized by all depended upon taste, touch and muscular sensations, while those recognized only by the anosmic depended upon differences in texture (*i. e.*, in touch sensations). "On the whole the experiments confirm the hypothesis made in this article, and while not diminishing the importance which has been given to sensations of smell in the 'tastes' of common experience, they indicate that touch and muscle sensations play an unexpectedly important part." The article brings out clearly some of the unsolved problems of the relations of the less intellectual senses and it will undoubtedly draw the attention of many to this almost virgin field.

3. Professor Patrick's second article is already known to readers of the REVIEW, from which it is reprinted (Vol. V., No. 6). It needs no further mention.

4. Two new pieces of apparatus are described by Dr. Seashore. The spark chronoscope is a pendulum chronoscope with arrangements for taking records by the graphic method while the pendulum is in motion. The following excellences are claimed for the new instrument: "Accuracy, adaptation for a variety of connections, soundless action, direct reading, ease and permanence of adjustment, and quickness and convenience of manipulation."

The audiometer is an instrument to produce variations in sounds and to measure the keenness of hearing. The new feature of the instrument is the use of varying sized secondary coils of an induction apparatus for sending currents to telephone receivers. The larger the secondary coil—*i. e.*, the greater the number of wire turns—the more intense will be the sound produced. The intensities vary from 1 to 1079. Simplicity, convenience, accuracy, constancy and size are noted as some of the merits of the apparatus.

The whole volume is interesting and instructive. The sole criticism the reviewer would make is that the first series of articles are 'minor studies.' None of the problems are treated exhaustively and, as Dr. Seashore rightly suggests, "all the time and energy might well have been devoted to one of the problems or a part of one." It is hoped that the researches here begun will be completed in future issues of the Studies.

SHEPHERD IVORY FRANZ.

*Magic, Stage Illusions and Scientific Diversions, including Trick Photography.* Compiled and edited by ALBERT F. HOPKINS, with an Introduction by HENRY RIDGELY EVANS. New York, Munn & Co., 1897. Pp. xii + 556, 400 illustrations.

*Spirit Slate Writing and kindred Phenomena.* WILLIAM E. ROBINSON. New York, Munn & Co., 1898. Pp. v + 146, 66 illustrations.

The contents of the work on 'Magic' are indicated by the five books into which it is divided, which are as follows: (1) Conjurers' tricks and stage illusions; (2) Ancient magic; (3) Science and the theater; (4) Automatic and curious toys, and (5) Photographic diversions. There is also a useful bibliography. 'Slate Writing' describes most of the phenomena of the ordinary spiritualistic séance.

The books are primarily intended for the general public and are well suited for instruction and amusement. The boy who learns these tricks at fourteen has a pleasant and useful employment, and is less likely to be a spiritualist or Christian scientist, and more likely to be a serious student of physics and psychology ten years later.

But the books also deserve notice in a psychological journal and a place in the psychological library. Conjurers' tricks and illusions offer a rich and almost unworked mine for the study of the psychology of perception. Suggestibility and the psychology of the crowd are important factors in the success of such exhibitions, and from this point of view they offer opportunity for research. The books should certainly be read by those interested in 'psychical research.' The numerous and varied methods by which ghosts can be made to appear and slate writing can be produced should lead every one to doubt his senses and his ingenuity on the occasion of their production. Lastly, many of the devices used for the production of illusions, etc., are extremely ingenious, and could to advantage be copied in the laboratory. The methods of chrono-photography, though scarcely deserving to be classed under 'magic,' are of special interest, as these should be used by the psychologist for the study of both perception and movement.

J. MCK. C.

*Kant und Helmholtz: Populärwissenschaftliche Studie.* LUDWIG GOLDSCHMIDT. Leipzig, Leopold Voss. 1898. Pp. 135.

It is only in Germany that such a work as this could possibly be considered popular. Indeed, even in that country its popular character

must consist chiefly in the usual quotations from the 'Dichterphilosoph' Goethe, for not only is the thought difficult, but the style, at least in parts, is excessively stilted and artificial.

The author is evidently, above all, a mathematician, and the work throughout is written from a mathematical standpoint. It is divided into three parts. The first describes some of the more general relations between the two thinkers; the second is devoted to an exposition of Kant, particularly of doctrines relating to mathematics; the third, which is the largest and by all means the most significant, deals with the conflict between the doctrines of space represented by each investigator. This chapter contains an interesting discussion of the modern non-Euclidian geometry. Toward the end the author becomes critic as well as expositor and endeavors to defend the Kantian doctrine of the *a priori* character of space against the attacks of Helmholtz.

The chief value of the works consists in the exposition. Its difficulty lies in its manner, for the style is not clear and the author has made the mistake, unusual in a German work, of failing to subdivide his material. The latter fault is particularly trying, especially in a book in which constant re-orientation is an absolute necessity. The author is very evidently a master in his field.

F. KENNEDY.

UNIVERSITY OF COLORADO.

*A Basis for Theory of Color-Vision.* WILLIAM PATTEN, PH.D.  
American Naturalist, Vol. XXXII., No. 383, Nov., 1898. Pp. 832-857.

Professor Patten, of Dartmouth, in this paper, prepared for the Morphological Society meeting of December, 1896, takes a step in the direction which is apt to lead to a theory of color-vision more satisfactory than those, based chiefly on the phenomena of the process, which we now have. From histological research made more than a dozen years ago (see 'Mitt. aus Neapel,' 1896) and since, he has reason to believe that the eye in its essential structure and action is somewhat similar to the ear. He investigated the visual organs of the lower orders, chiefly the mollusca and arthropods, and as a result maintains that "the rods and cones, or the parts corresponding to them in the lower animals, are not homogeneous, but fibrillated, and that in a number of invertebrates the fibrils are arranged according to their length in accurately graded series, and in such a position that they always stand at right angles to the rays of light that fall on them. The ether waves thus vibrate across a series of fibrils of different



lengths." The structural unit of the eye, then, appears to be a fibril from one to four micro-millimeters in length, many hundreds of which may be present in each rod or ganglion-cell. It seems highly probable to Dr. Patten that this structure is that also of all eyes, however difficult it may be to prove the fact in the higher animals and man.

This structural hypothesis is applied interestingly to the various conditions of vision, chromatic and achromatic, and it appears to suit very well. Ten illustrations are scattered about the text.

It is interesting to note that the main thesis of this paper have been recently corroborated in general terms by the elaborate researches of Professor Apáthy into the fibrillation of the neurons as well as by the work of several histologists upon the finer structure of the sensory end-organs and of the neural fabric in general. The probability that the retinal elements are fibrillated in a manner proportional to the empirical complexity of their function is rendered highly presumable by this extended work here rather too briefly reported.

GEORGE V. DEARBORN.

COLUMBIA UNIVERSITY.

*The Ethical System of Adam Smith.* ETHEL MUIR. Cornell University. 1898. Pp. 67.

After a brief statement of the antecedent 'moral sense' philosophers, the writer considers the ethical system of Adam Smith in two chapters; Sympathy; The Nature of Conscience. In the Conclusion this division is adhered to and reinforced by a discussion of 'the function of reason and sense in *The Theory of Moral Sentiments.*'

With Adam Smith, approval and sympathy are coextensive. He endeavors to meet Hutcheson's objection that we sometimes do approve without sympathizing, by positing a 'conditional sympathy.' That is, in all cases where we approve without sympathizing, we know we should sympathize if we attended sufficiently to the impression. The approval is grounded in this 'conditional sympathy.' Dr. Muir accepts this view and goes so far as to say that it is not the exception (as Adam Smith considered it) but the rule: we approve not upon sympathy, but upon 'conditional sympathy.' If the characteristic quality of sympathy be considered, the recognition of myself in another, then in the ensuing sense of ownership in that other, I cannot avoid a certain emotional warmth. Why this emotional quality accompanying the sense of ownership, should be called sympathy only when it has risen to a certain degree it is difficult to see. It appears that this distinction between sympathy and 'conditional sym-

pathy' is a remnant of the ancient logical method in psychological procedure, and is in the nature of things uncalled for. On the other hand, if we invariably sympathize when we approve, it cannot be said that we necessarily approve when we sympathize. Often the sympathetic expression is so shot through with feelings of attachment, tenderness, etc., that the individual never attains to the ethical moment, when he either approves or disapproves. His sympathy is the spontaneous expression of a primitive form of himself. Sympathy that is also approval, has freed itself from these trammels and stands as a moral attitude. It is the expression of the ethical self.

Dr. Muir considers the relation of Reason to Sympathy, the great problem of *The Theory*; and believes the great underlying principle of the system is reason. Unfortunately we are not told whether 'the judging faculty' or 'the higher reason' is intended. Here is indeed a great obstacle to a purely expositional and critical study of any but the most modern moralists. Even up to Adam Smith's time, psychology had not yet ripened into a propædæutic to ethics; and terms were largely used either in the loose popular sense or left altogether undefined. In the relation of Reason to Sense, the writer sharpens the position of Adam Smith by opposing it to that of Kant. "Smith regards reason as supreme, and sympathy as occupying a subordinate position (p. 64) \* \* \*. But reason is dependent upon sympathy for assistance in the formation of its judgments and its rules. For, without sympathy man would be unable to enter into any relations where morality would be possible or where there could be any necessity for the moral judgments of reason" (p. 67). Adam Smith is said by Dr. Muir to identify conscience with reason; again, "The supreme judge of conduct is the self" (p. 56). It would probably be a correct reading of Adam Smith's theory to say that to him the highest good is self-approbation. This the writer implies, but nowhere very clearly expresses.

J. W. L. JONES.

PRINCETON UNIVERSITY.

*The Applicability of Weber's Law to Smell.* ELEANOR ACHESON McCULLOCH GAMBLE. *American Journal of Psychology*, X., 1-62.

Miss Gamble's main problem is indicated by the title of her dissertation, and the evidence she offers is the result of her own painstaking experiments in this difficult and somewhat unattractive field. Zwaardemaker's olfactometer was employed (in which, as is well-

known, the intensity of the odor is regulated by slipping out beyond the end of a glass inhaling-tube more or less of a surrounding cylinder containing the redolent material), and there were used some twenty-six odorous substances both liquid and solid, gathered from the four quarters of the globe. In the main, the procedure was to give an inhalation of the standard intensity, then a stimulation clearly stronger or weaker, whereupon the subject moved the sliding cylinder until the sensation was just noticeably different from the standard. If the experiments were from the first designed to test Weber's law, it is surprising that, of so many performed, so few were carried out without changing either the subject or the substance or the nostril or the method. The net number of experiments in which all these conditions remained constant, with a change of the standard intensity alone, is very small; and even among these no system is apparent in the variations of the standard.

Out of it all, however, there is the indication that for two standard intensities, the difference-threshold often makes some approach to what Weber's law would require, although striking departures are likewise apparent. For the two standards the threshold was, on an average, something over one-third for the lower, and over one-fourth for the higher. In other words, the value came closer to a *relative* constancy of the threshold than to an absolute constancy, and consequently (the author argues) it may be said to be evidence in favor of the law. This would of course be better reasoning if we were sure that the threshold must show either one or the other form of constancy, and that by excluding the one alternative we could force on the other: but whether these alternatives exhaust the field is one of the matters to be proved.

As regards the value of the difference-threshold for the different odors, regardless of intensity, it was found to lie in the neighborhood of one-third in a large proportion of cases, and to be fairly constant for the different odors (as against Zwaardemaker), except for some few substances when the apparatus is supposed to have been at fault.

The author is aware that the adjusting of the tube by the subject was a possible source of error, in that the judgment may have been influenced by the feeling of the distance the hand was moving, as well as by the mere variation of the odor. In fact the error from this source she believes to have been one of the main causes for the falling off of the threshold for the higher intensities. It seems to me that this might also account for some of the constancy found for the different substances. Certainly in view of so grave a source of error the reasons offered for not having the experimenter make the changes—that



it would have excluded some of the substances used, or would have made the procedure more laborious—seem quite inadequate. Fewer experiments concentrated on definitely arranged standard intensities, with fewer subjects and fewer substances, could well have given clearer results as far as the main problem was concerned.

The good historical introduction and the, at times perhaps, too minute account of the preparation of materials, with all the difficulties involved, should be read by any one who proposes work in this line.

G. M. STRATTON.

UNIVERSITY OF CALIFORNIA.

*Psychology for Teachers.* By C. LLOYD MORGAN. New York, Charles Scribner's Sons. Pp. xi + 240.

In writing this book the author evidently intended, not so much to present a system of psychology, as to point out the more important traits the teacher must take account of to produce a rounded mental result. It is, of course, a nice matter in such a case to determine just how much psychology, and of what sort, can best be employed; and some will undoubtedly feel that Mr. Morgan might well have apportioned his space in a different way. In fact, with the exception of the last two chapters, of which more will be said later, the best things in the book are not given the systematic chapter discussions, but are tucked away in scattered paragraphs.

The author's psychology has the strong points of the great English tradition—an emphasis on association and language and personal experience and the constant testing of conceptions thereby; and yet with no slighting of the motor and emotional and volitional functions of mind. But teachers will look in vain for their old friend 'apperception,' nor will they find a single reference to a nerve-cell or a 'higher-center.' The book may thus seem a trifle tame to those who take their summer recreation at the psychological laboratory and are at home in child study and the central nervous system. And yet, even though the author treats all these things as if they were not, he has written in a most helpful way because of his grasp of the real purpose of education and of the deeper structure of the mind.

Like many a good story, however, the book does not carry one along at first. The somewhat labored distinctions between sensation and 'scept'—an unpardonable word—percept and concept, perception and conception, fail to arouse much interest, and may discourage many a conscientious reader who feels that he can not go on unless he masters these. Less abstractions here, and more reliance on illustra-

tions drawn from the field of illusion, which the author neglects entirely, would perhaps have served the purpose better. Association, although given an entire chapter, is kept well within bounds, and not made an all-explaining principle; so that he can perhaps the more readily believe not only in the uses of interest but also in those of drudgery and of sheer resolve. Expression is only touched on here and there, and then with more regard for its social value than for its reactive and clarifying effect upon the mind. It is a great means of self-mastery and in teachers' psychology should have a prominent place. The intellectual as well as the moral use of skill in hand-work and games, which, of course, is one phase of expression, is, however, briefly but forcibly stated by the author. If mention were to be made of scant justice done to other subjects pedagogically important, there is certainly not given to imitation and suggestion generally the treatment that the present interest might be expected to invite.

The different threads of the mental life are kept well together. One does not have that constant view of various strands at once, which a writer like Höffding gives; but for the teachers' purpose the same end is approached by some good images—the spirited horses and coach and driver (the provisional figure for the different sides of mind) having finally to be consolidated into a centaur to express the true relation, and again where he says that “the cognitive aspect of experience \* \* \* gives the form and grouping of the picture of consciousness; the emotional aspect \* \* \* gives the color and tone of the picture.” But best of all is his happy insistence on the ‘margin’ and ‘background’ of consciousness, with their vital relation to all that comes within the focus of attention. The art of teaching is not merely to provide for a suitable play and clearness of this intellectual fixation-point, but to get the right things ‘inextricably woven into the mental background,’ and to lay up there stores of ‘strength and wisdom and emotional prejudices of a goodly human kind.’ What a benefit if this mere phrase from the book—‘emotional prejudices of a goodly human kind’ (quoted by the author from Miss Simcox)—could itself become inextricably interwoven into every teacher’s mental background! But the book is temperate throughout, and there is no overrating of feeling or of will at the expense of the more purely intellectual processes, as if the child could be stanch and steady irrespective of his intellectual insights.

Education, on the contrary, is seen by the author to be a many-sided affair, and no cheap and ready formula is offered for its attainment. It is not to be reached without doing justice to the intellectual

and emotional and volitional sides of life, and so directing these that the child is put in *rappport* with the great spiritual possessions and ideals of mankind. He must be brought face-to-face with facts and thus make his conceptions tally with them, but he must develop his powers of appreciation and of sociability. The closing chapters on 'literature' and on 'character and conduct' ought to be read, even if all the rest be skipped. The teacher is shown that in some way the inner warmth of art and especially of literature must be imparted; and, in so doing, *knowledge* of the work—its intellectual aspect—must not be confused with the peculiar æsthetic enjoyment. Education also means the growth of character—the adoption of the social aim, not in a spirit that is sentimental and visionary, but with an active interest in small and unimposing social gains wherever possible. It means finally the cultivation of the *religious attitude*, as distinguished from special forms of belief. The author is fully aware that these are the most delicate and searching parts of the teacher's work, and are to be accomplished less by direct instruction than by a fine spiritual contagion. The teacher's own sympathies and appreciations here, rather than his precepts, are what count.

G. M. STRATTON.

UNIVERSITY OF CALIFORNIA.

*Ein einfacher Apparat zur Bestimmung der Empfindlichkeit von Temperaturpunkten.* F. KIESOW. Philos. Stud., XIV., 4, 589-590.

Dr. Kiesow here describes an apparatus for finding heat and cold spots. Heretofore a difficulty has been experienced in having to use instruments that would not keep an even temperature or whose temperature could not readily be altered. The present instrument seems to have overcome the latter difficulty but not the former.

The apparatus consists of a hollow cone in which are two pipes with leads from bottles containing hot and cold water respectively. The temperature is regulated by raising or lowering one of the bottles, so that the hot or cold (as the case may be) water flows towards the other. This permits a ready change in the temperature of the instrument, but the arrangement for constancy is awkward. The constant cooling of the instrument necessitates a continual shifting of the bottles to keep the liquid always of the same temperature.

An electric apparatus, though more expensive, might be devised to overcome all the difficulties.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.



*L'Asymétrie sensorielle.* J. J. VAN BIERVLIET. Brussels, Hayez. 1897. Pp. 43. (Repr. fr. Bull. de l'Acad. roy. de Belg., Vol. XXXIV, 1897.)

This paper is an attempt to correlate right- and left-handedness with a functional preponderance of the same side in other senses. The evidence is purely experimental. One hundred subjects, mostly university students, were tested in the muscular sense, hearing, vision and touch. The muscular test consisted in pulling with each hand a load attached by a string. The load raised by the stronger hand remained unchanged and served as standard. In the first test, the two loads were started equal, and the one judged heavier was gradually decreased till it seemed equal to the standard. Another test was then made, with an ascending series of the variable weight. In all, three descending and three ascending tests were made on each subject, each with four different standards (500, 1000, 1500 and 2000 gm.). Of the 100 subjects, 78 were right-handed and 22 left-handed according to this test—none were 'ambidextrous.' With but two or three exceptions, the results when averaged were remarkably uniform, the ratio being about 450 gm. with the weaker, to 500 gm. with the stronger hand, whichever it might be; the same ratio held for the other weights.

For the sense of hearing, two shot-fall apparatus, almost exactly similar, were made, and each enclosed in a sound-proof box, with a tube running out to one ear of the subject. The two balls were dropped in rapid succession, and the subject compared the intensity of the sounds. For right-handed subjects the right ear was taken as standard, and the height of the left shot varied till the two sounds seemed equal; the procedure was reversed with left-handed subjects. Five series each were taken with increasing and decreasing intensity and averaged together. After a number of cases of partial deafness were thrown out, the variations of the rest were all in the same direction as the muscular tests, and the ratio almost exactly the same.

The visual tests consisted in measuring the distance at which type of a certain size could be read. Considerable difficulty was experienced from the various minor defects of vision, and many cases were out of all relation to the normal. Rejecting these, the difference between the two eyes, in almost all the rest, was again in the same direction and nearly the same ratio.

Finally, the tactile sense was tested by means of Weber's sensory circles. The author does not describe any means used to secure exactly corresponding regions of the two hands. Ten tests each were made in ascending and descending series. The right hand was found

to have a lower threshold than the left in right-handed persons, and *vice versa*, and the ratio was about the same as before.

In the muscular tests the ratio in question was 9.00 to 10 (right-handed) and 9.02 to 10 (left-handed); in the auditory tests 9.10 to 10 for each; in the visual, 9.08 to 10 and 9.04 to 10 respectively; and in the tactile, 9.06 to 10 and 8.93 to 10 respectively. The persistency of this fraction ( $\frac{9}{10}$ ) seems remarkable, and should be submitted to further test; if verified, it will rank with the fractions determined for Weber's Law, or outrank them in importance. The uniform preponderance of the same side through the four senses tested is also notable. The author declares his belief that it points to an anatomical rather than a physiological basis for right- and left-handedness.

HOWARD C. WARREN.

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### ETHOLOGY.

*Ethology: Standpoint, Method, Tentative Results.* THOS. P. BAILEY, Jr., University of California. University Press, 1899. Pp. 30.

*Bibliographical References in Ethology.* THOS. P. BAILEY, Jr., University of California Library Bulletin, Vol. 13. University Press, Berkeley, 1899. Pp. 25.

This account of a new undertaking in the University of California is deserving of more than a passing notice. Here is a psychologist, a philosopher, and a student of education, devoting all his energies to the study of character. His title is inspiring: 'Associate Professor of Education as Related to Character.'

At the same time it is a commentary on the present condition in higher educational circles. Such a title would not be possible if our educational leaders recognized that the whole problem of education is one of character.

At the same time we feel obliged to protest against that view of psychology which finds it necessary to create a new science in order to make the study of character legitimate. True, the idea that psychology is unsympathetic, mechanical, lifeless, is abroad in the land, but that is not the view of our best psychologists, with whom the cry of 'Back to real life,' is strong and clear. We can assure our author that there are many psychologists who will welcome ethology as it is here outlined, as a new chapter in their own science, and that there are yet more who believe the sole aim of all branches of psychology to be the better understanding of mind with a view to its development.

This certainly is the belief which has given psychology its place in our colleges, and the one permitting the life of all psychology to-day in our own country. Experimental psychology, particularly, has grown upon it.

At the same time there may be no harm in giving this group of problems, the most important in all psychology, a special name, and assigning them to a special department in our University at least. There are people who will grant the reality of character and the importance of its study when they see it rechristened with a new Greek name. We expect help from Ethology in the University of California, but should not like to see a chair of Ethology in every college. We do hope that the work itself will be felt in every course in psychology in the country.

If the author finds it difficult to describe his new methods, the tentative results and the many lines of effort it certainly is impossible to reproduce them here, to say nothing of giving the criticism for which he asks.

The perspective drawing of a cave, with surface lines indicating lines of character growth, and cross sections showing successive stages of character development in the race and in the individual, and also corresponding stages in education from the Kindergarten to the University, is a helpful way to bring before the eye many of the elements which enter into character. It is, however, impossible to represent in this way the relative importance of the different elements.

Perhaps a place might be made for the influence of personality and for authority. This latter is suggested by the remark of a psychologist in a mission field—an excellent place for character study, by the way—that in their schools the discipline counted for more in character building than did all the secular and religious instruction. At the corresponding point in this diagram we see only the spontaneous development of boy nature under the influence of the various studies.

C. B. BLISS.

#### GENETIC, EDUCATIONAL AND SOCIAL.

*Psychologische Analyse der Thatsache der Selbsterziehung.* G. CORDES. Berlin, Reuther u. Reichard. 1898. Pp. 54. M. 1.20.

For the material of his enquiry Dr. Cordes turns to the experiences of his own life, stripping these as far as possible of all that makes them personal and unique, and dealing only with those aspects which are typical of the process in all men. The author's interest being a



psychological one, it lies as far from his purpose to enquire into the metaphysical possibility of self-education, on the one hand, as to make an ethico-pedagogical application of his results on the other.

By 'education' is to be understood the activity of one person—the teacher—which exerts upon another—the pupil—such enduring influence that his mental processes and outward behavior realize an ideal of thought and conduct existing in the mind of the teacher. Under this conception one can speak of *self-education* only by analogy, which is yet a real analogy, since here, too, an activity is found which exerts an enduring influence upon our psychical processes in consequence of ideals which exist in consciousness. To indicate the conditions and elements of this process is the object of the monograph, which can here be only briefly summarized.

At the outset two aspects of the matter present themselves: first, as to the presuppositions—the psychical material and means—of self-education; and secondly, the processes themselves which it involves. The presuppositions, putting aside disputes as to freedom or its contrary, and the possibility of self-observation, involve three things which correspond respectively to the personality of the pupil, the ideals of the teacher, and the educating influence which mediates between them. The whole complex fact of past experience and present character gives the first, in which, without following the detailed analysis of the writer, are to be separated the individual psychical acts and the personal disposition, whether resulting from these acts or due to inheritance. In the second place there must be set over against this, another order of psychical experiences, non-existent as yet for the practical subject, which consists of the ideals into which the present psychical processes are to be transformed. These ideals, derived from the lives of other persons either through indirect suggestion or generalized observation, become effective through the strong emotion with which they are conceived, an emotion depending upon a comparison of the two orders of experience with respect to their worth, and a resultant higher valuation of the ideal. The third of these presuppositions is the will for which this ideal order of experiences becomes a motive. The preceding judgment of worth is an effectless reflection which is energized by the will as a process of choice.

The second consideration is as to the actual processes of self-education. If one defines self-education as the shaping of the personal disposition so that each individual life-experience shall correspond to our highest moral valuation, then the general desire for betterment must be supplemented by a definite transformation of concrete indi-

vidual impulses if it is to be realized. This education takes place in three directions: First, with respect to intellectual processes, the whole order of ideas, concepts and judgments is to be shaken up and transformed; and these ideas and concepts, which possess each its particular emotional overtone, must be stripped of this overtone and united to a new quality of feeling. Secondly, with respect to emotional processes there are to be revalued under the criterion of the ideal order of experience the sensuous feelings, or emotional overtone of sensations, common feeling, by which is understood that fusion of inner and outer sensations in which our general well-being or ill-being is expressed, and the passions, through which not only are the ideas intensified, but they together with the will-processes are modified and transformed. Thirdly, with respect to the will-process itself self-education exerts a three-fold influence. First, in regard to the will's reaction upon motives, education is expressed in a more swift and decisive process; secondly, in regard to activity in general, it is expressed in an increase in the total will-power or energy of the subject; and thirdly, in regard to the voluntary direction of the attention, it is characterized by a greater control over the objects which shall occupy consciousness. The short bibliography which Dr. Cordes appends to his clear and detailed analysis would be of value if it were more precise. A general reference to Wundt's *Grundriss der Psychologie* or Nahlowsky's *Gefühlsleben* is too much like a wave of the hand to help in one's literary orientation.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

*Über Willens- und Characterbildung auf physiologisch-psychologischer Grundlage.* JULIUS BAUMANN. Berlin, Reuther u. Reichard. 1897. Pp. 86. M. 1.80.

In suggestive contrast with the introspective, theoretical work of Dr. Cordes (summarized in the foregoing note) stands this monograph of Professor Baumann, of Göttingen. The former is a subjective analysis of individual personal experience, the latter an objective study of the education of the will, the result of a life of personal observation as a teacher in the schools of Germany. The laws which Dr. Baumann sets down grew up as working principles in his teaching, and are intended to have a direct practical bearing. They are, the author tells us, a concise re-statement of ideas already set forth both in his *Handbook of Ethics* (1879), and in his *Introduction to Pedagogy* (1890), supported by the most recent results of physiological and pathological psychology.

We must distinguish between the will as theoretic choice and the will as psychological activity; in the latter sense it is expressed in a series of reactions and depends upon a psychophysical mechanism. With the development, educability and derangements of this will Professor Baumann here deals. The author refers briefly to the physical basis of mental life in general as indicated by the localization of brain functions, influence of drugs, effects of fatigue, and the like facts, then proceeds to a detailed statement of the physical relations which condition the will-functions, for the evidence of which he turns to the various phenomena of pathological will-conditions, abulias, amnesias and automatisms. It is through these interferences with the psychophysical mechanism by which the attitudes of the practical individual are expressed, interferences which pervert or inhibit his desires, that the conditioning of the will upon these processes is brought most forcibly to our notice. The undeveloped will is, then, that psychophysical organism in which the orderly connection of these parallel activities has not been established upon the basis of practical experience, and the pathological will is that in which the customary synthesis of perception or desire with motor reactions has been interrupted. The educability of the will depends upon the possibility of organizing and extending this system of coördinated physical and psychical activities, and the development of it consists in the actual process of transforming the elementary impulses and powers into such an orderly series of desire- and choice-fulfilling acts. In the child only the negative conditions of a willing subject are given. The vague discomfort is there, the vague desire, but the stimulation, whether peripheral or central, does not call forth, as in the adult, definite and adapted reactions. The capacity for reaction and the impulse toward original activity exist in the child but are not yet coördinated. In this coördination consists the education of the child-will. Professor Baumann next proceeds to an analysis of the chief psychological laws involved in the process of volitional education and the training of character, with especial reference to the development of moral qualities.

But first, since the healthy will involves a good mental tone and sound physical state, a fundamental condition of its training lies in constant care for the health and attention to exercise, rest and refreshment, both bodily and mental. The development of the will is twofold, corresponding to the active and receptive aspects of the personality: first, increase in the precision and energy of the will in those activities which we already possess; and secondly, extension of the will-activity to new objects and interests. The primary law of the



first form is practice; the activity in process of acquisition is established only through repetition, and the activity once under control of the will must never be allowed to lapse wholly from use. Spontaneous imitation the author is inclined to reject as an element in the education of the will, on the ground that it is only the realization of a tendency already existing, and is strictly limited in its functions. Voluntary attention is emphasized as a moment in the process parallel to the factor of repetition. To these must be added the influence of success and failure in effort, indirect training, example, emulation and the like, as factors in the development of the will. All these laws which condition the form of the individual will-act enter also, with their combinations, into the formation of character, that permanent disposition towards organized systems of activities which the individual act tends to beget, and from which reciprocally it springs.

Professor Baumann's monograph closes with a consideration of the pathology of mental and moral impulses, and a discussion of the theories of Beneke and Herbart concerning the education of the will.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

*L'Année Sociologique.* É. DURKHEIM. Deuxième Année (1897-98). Paris, Alcan, 1899. Pp. vi + 596, 10 fr.

The two volumes now issued of this annual make a capital start. They are similar in scope to the *Année Psychologique*, having a Part I. devoted to *Mémoires originaux*, and Part II. given up to *Analysis* of books and articles published between July 1st of one year and June 30th of the next. Sociology is understood in the widest sense. The original memoirs are, for the most part, outside of our scope, but we may call attention to Professor Simmel's remarkable paper, in the first *Année*, on 'The Persistence of Social Groups,' now translated in the *Amer. Jour. of Sociology*, March, May, 1898. It contains much psychological matter on the different phases of so-called 'honor.'

J. M. B.

*L'Évolution mentale chez les Animaux.* CH. LETOURNEAU. Revue de l'École d'Anthropologie de Paris, Vol. 9, V., 15 May, 1899. Pp. 137-152.

This is a lecture delivered as an introduction to a course on the Evolution of Morality, at the School of Anthropology of Paris. In method and content the article is on purely evolutionary lines, and is divided into seven topics, as follows: The Problem of Consciousness,

Motivity, The Genesis of Desire, Sensation, Feelings and Emotions, Intelligence and Reason, and Domestication and Civilization. As a sort of summary of the development briefly traced under these heads, we may quote his words thus:

“At first, in the protozoa, we see only confused movements of the protoplasmic substance (*Amœba*). Then, in the lowest radiates, the nervous tissue begins to differentiate, to control the movements of the contractile substance, and even to be aware of sensations and to preserve a trace of them (*Medusa*). Among the higher radiates this nervous memory is perfected and from this there result complicated reflex acts, which seem combined, coördinated, for the attainment of an end, while being almost certainly unconscious. Organic and psychic progress is accentuated among the molluscs, where one sees plainly appear the organs of special sense, already well developed in the cephalopods. In the higher molluscs the relative perfection of the sense organs and of the nervous system, which, however, as yet ganglionic, authorizes admission of the existence of a well-developed nervous consciousness, of distinct sensations, of simple feelings, and even of an intelligence still rudimentary. Finally, in the divisions of the vertebrates, and particularly among the first of the mammals, the existence of mentality very analogous to that of man can scarcely be doubted.”

Dr. Letourneau considers thought ‘a complex product of nervous consciousness,’ while reason he judges to be nothing else than “coördinated application of the elements of nervous consciousness to particular and desired ends.”

GEORGE V. N. DEARBORN.

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## NEW BOOKS.

*Psychologie mit Anwendung auf Erziehung und Schulpraxis.*

KARL HEILMANN. Leipzig, Dürschens Buchhandlung, 1899.  
Pp. 86.

*L'Année Psychologique.* A. BINET. 5<sup>me</sup> Année. Paris, Schleicher Frères, 1899. Pp. 902. 15 Fr.

*Wörterbuch der Philosophischen Begriffe und Ausdrücke.* R. EISLER. Vierte Lieferung. Berlin, -Mittler, 1899. Pp. 289-384.

*Sensazioni vibratorie.* N. R. D'ALFONSO. Seconda edizione Roma, Soc. Dante Alighieri, 1899. Pp. 39.

*Through Nature to God.* JOHN FISKE. Boston and New York, Houghton, Mifflin & Co., 1899. Pp. xv + 194.

*Spinoza and Schopenhauer.* SAMUEL RAPPAPORT. Berlin, Heyfelder, 1899. Pp. 148.

*Nouvelles Esquisses de la Philosophie Critique.* A. SPIR. Paris, Alcan, 1899. Pp. 30 + 147.

*I Sogni, Studi Psicologici e Clinici.* SANTE DE SANCTIS. No. 17 in Pic. Bibl. di Sci. Moderne. Turin, Frat. Bocca, 1899. Pp. 390. 5L.

An extremely well written and interesting account of dreams by a competent psychologist. The successive chapters sum up adequately the literature of the subject and give bibliographies under the several heads: *i. e.*, 'Dreams and Mysticism,' 'Methods of Studying Dreams,' 'The Dreams of Animals,' 'Of Children,' 'Of the Aged,' 'Of Adults,' 'Of the Neuropathic' (of several different classes), 'Of Criminals,' 'Dreams and Emotions,' 'Dream Psychoses in Health and Disease,' 'Psychophysics of Dreaming,' 'The Marvellous in Dreams.' An English translation would probably serve a good purpose. J. M. B.

*The Philosophical Theory of the State.* B. BOSANQUET. London and New York, Macmillans, 1899. Pp. xviii + 342. \$3.25.

*The Races of Europe; a Sociological Study.* W. Z. RIPLEY. With A Selected Bibliography of the Anthropology and Ethnology of Europe (a supplementary volume published by the Trustees of the Boston Public Library). New York, Appletons, 1899. Pp. xxix + 624.

This work comprises Professor Ripley's Lowell Lectures which have already attracted much attention in their serial publication in the Popular Science Monthly.

*La Dissolution opposée à l'Évolution dans les Sciences physiques et morales.* A. LALANDE. Paris, Alcan, 1899. Pp. viii + 492. 7 fr. 50.

*Aberglaube und Zauberei von der ältesten Zeiten an bis in die Gegenwart.* A. LEHMANN. Deutsche Ausgabe von Dr. PETERSEN. Stuttgart, Enke, 1898. Pp. xii + 556.

*The Value of Religious Facts.* J. H. WOODS. New York, Dutton, 1899. Pp. 165. \$1.

*The Physical Nature of the Child and how to Study it.* S. H. ROWE. New York and London, Macmillans, 1899. Pp. xiv + 207. \$1.

*Friedrich Nietzsche, Aphorismes et fragments choisis.* H. LICHTENBERGER. Paris, Alcan, 1899. Pp. xxxii + 181.



## NOTES.

THE circular announcing the Fourth International Congress of Psychology has appeared. It may be had by addressing M. Pierre Janet, Secrétaire général, 21 rue Barbet-de-Jouy, Paris. The congress—of which we hope to make fuller announcement shortly—is to be held in Paris, Aug. 20–25, 1900.

OTHER congresses—to be held in connection with the Exposition—which may interest psychologists are: that for Philosophy, Aug. 2–7 (see circular of organization issued in the *Revue de Met. et de Morale*, July, 1899: another circular is in preparation giving an international 'Committee of Patronage' for this Congress), Secrétaire M. Xavier Léon, 39 rue des Mathurin, Paris; that for 'Instruction in the Social Sciences,' second half of July (having French organization and an international 'Committee of Honor'), Secrétaire M. Dick May, 22 rue Victor Massé; that on the 'History of Religions,' September 3–9, Secrétaires MM. J. Réville and Léon Marillier, Sorbonne, Paris.

WE regret to record the death, on June 14th, of Professor N. Grote, of the University of Moscow, the distinguished Russian psychologist and philosopher. Professor Grote was editor of the *Voprosii filosofii* and President of the Psychological Society of Moscow. The death is also announced (on June 13th) of Professor Nourrisson, the well-known academician and historian of philosophy.

PROFESSOR A. C. ARMSTRONG, JR., of Wesleyan University, is to be abroad the coming year on 'Sabbatical' leave, wintering probably at Oxford. The department will be in charge of Associate-Professor Dodge.

WE have received the first numbers of two new journals, the *Revue de Morale Sociale*, edited by L. Bridel, of Geneva (Paris, Giard et Brière, quarterly, 10 fr.) and the *Zeitschrift für Pädagogische Psychologie*, edited by F. Kemsies, of Berlin (Berlin, Walther, bimonthly, M. 8).

MESSRS. WILEY & SONS, New York, announce a work entitled *Statistical Methods with special reference to Biological Variation*, by Dr. C. B. Davenport, of Harvard University.

THE prospectus of the *Jahresbericht über Neurologie und Psychiatrie* has reached us. It is to be edited by Professor Mendel, of Berlin, with a corps of distinguished collaborators. The first volume will be devoted to the literature of 1897. Authors are requested to

send books and reprints for analysis to the publisher (S. Karger, Berlin, N.W. 6, Karl str., 15). A section devoted to Psychology will be in charge of Professor Ziehen, of Jena.

WE note the appearance of the German translation of Professor James' *Will to Believe*.

WE learn also that Professor Sanford's *Course in Experimental Psychology* is being translated into French by Dr. Schinz, and Professor Baldwin's *Story of the Mind* into French and Italian.

DR. W. O. MONTAGUE, of Harvard, has been appointed Instructor in Logic in the University of California.

PROFESSOR BALDWIN has been given a half year's absence from Princeton to see the *Dictionary of Philosophy and Psychology* through the press in England. He intends to sail on September 19th and wishes all the American contributions, proofs, etc., to be in his hands in the first week of September (address until September 10th, Buzzards Bay, Mass.). His London address is care Macmillan & Co., Limited, St. Martin's St. His courses at Princeton will be in the hands of Professor Warren.

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AFTER the appearance of this issue all communications for the editor, books for review, etc., should be sent to Professor J. McK. Cattell, Garrison-on-Hudson, N. Y.

# THE PSYCHOLOGICAL REVIEW.

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## ON THE VALIDITY OF THE GRIESBACH METHOD OF DETERMINING FATIGUE.<sup>1</sup>

BY DR. JAMES H. LEUBA,

*Bryn Mawr College.*

In the opinion of many psychologists the results published by Griesbach and his imitators, Dr. Ludwig Wagner and Dr. Vannod, were highly surprising. Experience with the æsthesiometer leads easily to the opinion that, although the ability to discriminate simultaneous touch sensations might very well be altered by fatigue, it would be impossible to measure its influence without taking into account many other factors. That, without regarding these other factors, the dependency of the discrimination sensibility on fatigue could be ascertained safely and easily enough to make of 'the method of Griesbach a practical means of determining and of comparing the degree of fatigue'<sup>2</sup>—a method applicable to the school problem, for instance—went against what many thought to be a legitimate interpretation of their experience.

Griesbach's paper, coming at the timely moment when the fatigue question was the topic of the day in many a teacher's circle, attracted a great deal of attention, and speedily found imitators among teachers apparently delighted to have at last in

<sup>1</sup>The larger part of the experiments here recorded were performed at Heidelberg, Germany, in part in the Laboratory of Professor Kraepelin, and with the improved Griesbach æsthesiometer belonging to the said laboratory. Our thanks are due to Professor Kraepelin for his assistance, and to Dr. and Mrs. Lindley, who acted as subjects during the experiments carried out in Germany, and generally coöperated with the writer.

<sup>2</sup>Wagner's 'Unterricht u. Ermüdung,' p. 112.



hand an easy means of settling the bitter discussion of school fatigue. The results of Drs. Wagner and Vannod confirmed the claim of the initiator, and to-day the method is widely accepted as filling the need of the teacher and of the physician for a comparative determination of fatigue-states.

The writer undertook last winter a series of experiments which were to be the beginning of an exhaustive investigation into the various factors influencing the discrimination of simultaneous touch sensations: the temperature of the surrounding atmosphere and of the parts affected; the blood circulation; the emotional state, etc. As the preliminary measurements clearly contradicted the claims of the authors cited, it was, for several reasons, thought wise to publish them without waiting longer for the completion of a work already several times postponed.

The point at issue in what follows is not so much the correlation of fatigue with discrimination sensibility, as the validity of the æsthesiometric method for the determination of fatigue.

The measurements on which we base our conclusions extended over 14 days and include some 6,000 separate judgments. They were taken at Heidelberg on three persons whom we shall designate by the numerals I, II, III. The writer was one of the subjects; his measurements were taken by Dr. Lindley. To these data, about 180 threshold-determinations were added at Bryn Mawr, involving about 2,000 separate judgments, obtained from six young women students at Bryn Mawr College, by three post-graduate students who had been prepared by a good deal of practice.<sup>1</sup> Dr. Lindley and myself had both worked before with the æsthesiometer; nevertheless, to insure from the start dexterous use of the instrument, tests were taken during two days before record was made of them.

At Heidelberg we used the instrument carefully, guarding as far as possible against the known sources of errors, such as cold spots, momentary hyperæsthesia easily induced, in some persons at least, by focusing the attention on one bit of the skin; the interference of after-images and the partial anæsthesia caused

<sup>1</sup> Miss M. Hussey, Miss G. Locke and Miss N. Wood, to whom I desire to express my thanks.

by too frequent and too rapid touches. We found it necessary to allow about 6 minutes for the determination of the thresholds of one person taken at two places; this included frequent intervals for the skin to return to its normal state. We proceeded as Griesbach had done: the threshold for one and that for two points were successively found for the forehead—along a line running horizontally across the middle of the forehead—and for the ball of the thumb. The place was marked, but we took care to avoid putting the points always on the same spots. The threshold for one point was sought by a gradual decrease; that for two by a gradual increase of the distance between the points of the instrument. Each threshold determination involved usually about eight judgments, sometimes more, especially when after having said ‘one,’ the subject fell back to two, although the distance had been further reduced. In a case of this kind the figure put on record was the one indicating the longest distance at which the two points were *definitely* felt as one.

In order to draw a parallel between the discrimination sensibility and fatigue, it is evidently not sufficient to have a measure of the former; we should also have a measure of the latter. To establish the discrimination curve and then to interpret its ups and downs in fatigue terms with one’s own feeling, or the probable fatigue effect of a lesson in geometry or in singing as the only guide, is evidently a ‘*pis aller*.’ If, in this respect, we did not do much better than the investigators in question, we had at any rate the production of fatigue under better control than was the case with the school boys and apprentices tested by them; we could so choose the objects of our attention and so direct it that a relatively constant increase of fatigue took place. Moreover, we were enabled to measure the amount of work done for two half hours daily during six days. This was afforded us by the experiments carried on at the time by Dr. Lindley, experiments in which all three of us participated.<sup>1</sup> They were so conditioned and controlled that the number of

<sup>1</sup>An account of these experiments tending to determine the influence of periods of rest of various lengths on the amount of work performed in a given time, is to be published in Kraepelin’s *Psychologische Arbeiten*.

additions performed in the half hour, or in the hour, could be taken as the measure of the mental work done. Unfortunately for us, the limitation of the adding to two daily half hours reduces seriously the value of this control. To submit oneself to the conditions required by such experiments, if they were to last for several hours during three or four days, would be an ordeal beyond the endurance of most men.

In the measurements taken at Bryn Mawr the method pursued was the one used by Wagner: the threshold was gradually approached from two extreme distances, one evidently too great and the other clearly too small; the former distances alternating with the latter. This procedure has the advantage of keeping clearly before the subject's mind the qualitative differences between what he is to call 'two' and what he is to call 'one,' while the other practice delays the recognition of 'twoness' and of 'oneness.'

We begin with the experiments performed at Heidelberg. The three subjects noticed early in the investigation, as other observers had done before them, that the passage from 'oneness' to 'twoness' is through a sensation of length: the touch loses its pointed quality and acquires that of a line gradually stretching until it breaks in the middle, thus producing two spatially disconnected sensations. A similar transformation takes place, but in the reverse order, when the threshold for one is sought. It was therefore agreed to keep answering 'one' however extended the sensation, as long as the break had not occurred.

In the following accounts of our experiments we shall, for the sake of directness and concision, refrain from entering into details which, interesting though they might be to the psychologist, do not bear directly upon our immediate object. For the same reason we shall give only as many of the curves we have obtained as appears to us necessary to establish our opinion.

Fig. I will enable the reader to compare the oscillations of the discrimination sensibility during the three days of severe mental work with those having taken place during three days of rest. Each curve is the resultant of three daily curves. The full lines are the rest curves, the dotted ones the fatigue curves. The time at which the measurements were taken is indicated by



the figures along the axis of ordinates. The thresholds are given in millimeters.

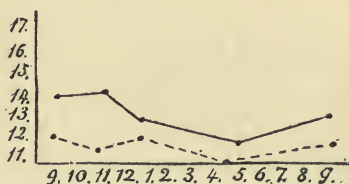
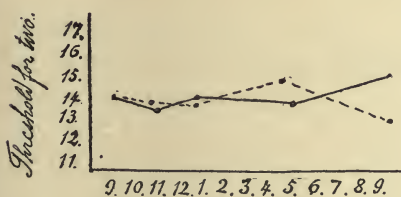
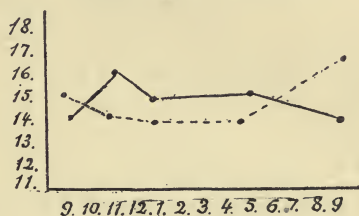
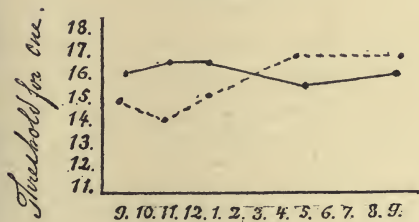
We kept at work from 9 or 9:15 A. M. until 1 P. M. without other interruption than the one occasioned by the taking of the tests at 10:45 or 11 o'clock. During the afternoon we had

Fig. I.

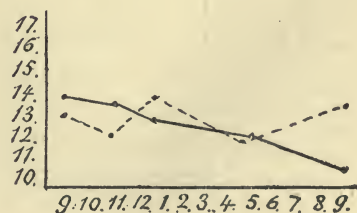
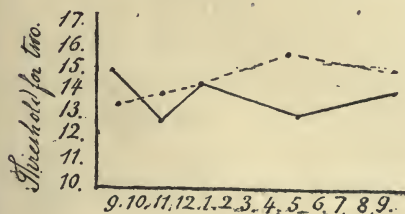
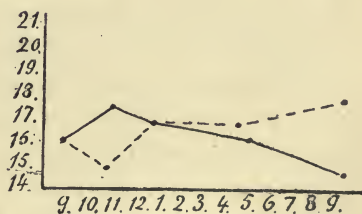
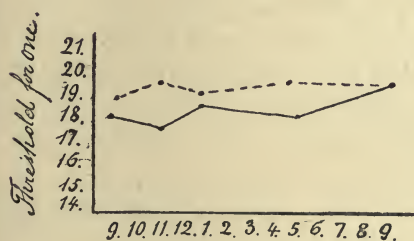
Forehead

I.

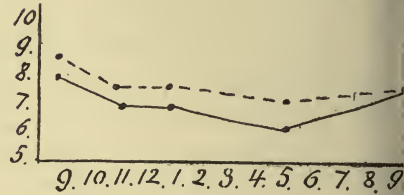
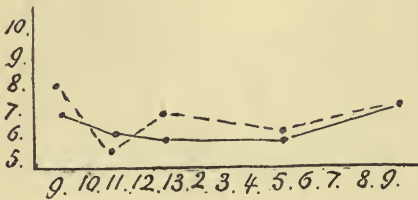
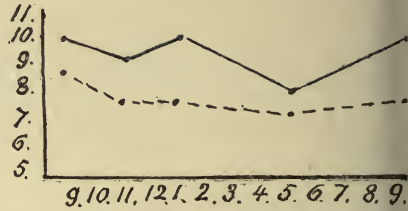
Ball of Thumb



II



## III.



from two to three hours of work before 5 P. M., and we set to work again immediately after an early supper—*i. e.*, from about 7:15 or 7:30 until 9:15 P. M. The subjects kept on working until their time came to be tested. I. spent most of her time reading German literature (Goethe); III. perused books on psychology and philosophy (chiefly Wundt's *Grundriss der Psychologie*), while II. computed the returns from the experiments before referred to, a task by no means easy. The German language introduced in our reading an additional element of fatigue; Goethe required on the part of I. close attention and induced very soon real fatigue. We did our best all through the day not to allow our mind to wander, even though it cost us frequent painful efforts.

We worked all three in the same room to eliminate the fluctuations of temperature and of blood circulation which would have been produced by the passage from the open winter air into the warm atmosphere of a room. Our life was well regulated; we retired early and nearly at the same hour each day and our coffee and tea drinking was kept as much as possible the same during the six days of experimentation.

The fatigue produced was well marked. The words used to describe it were, for the first day: 'Quite tired; a little

nervous' (III); 'very tired, understand no more' (I); 'worked with energy all day, but could not go on much longer' (II). For the second day: 'Tired' (III); I., who felt very tired before supper, was somewhat excited and disposed to go on when 9: 15 P. M. came; 'very, very tired, tired out' (II). At the end of the third day we were all three quite tired and extremely glad at the prospect of the coming days of rest. The last hour of the morning was usually quite burdensome, not that our work was lacking in interest, but that we were no more able to attend spontaneously; it was already fatigue and not simply nervousness. The reader might wish that we should give the curves for each one of the six days, together with a designation of our fatigue feelings at the moment of the measurements and not only the resultants. If anything more than what can be inferred from the resultant curves could be derived from the separate curves, they would be reproduced here; but it is not the case: the daily curves do not follow the fatigue feelings any better than the resultants.

As each one of us engaged for the whole morning in mental work engrossing his attention and requiring a considerable degree of mental tension and drove himself continuously at full speed, the reader acquainted with Griesbach, Wagner's or Van-nod's papers will expect our fatigue-curves to ascend more or less regularly from 9 A. M. to 1 P. M. The ups and downs of the curves of these investigators due, according to them, to the different degree of difficulty presented by the lessons, some of them being hours of relaxation, could not be expected in our case. At 5 P. M. the curves should not be found much, if any lower than at 1, the after-dinner rest being offset by from two to three hours of work; and we should expect them to reach the highest point at 9: 15.

Concerning Fig. I the following points are to be noted:

1. The vertical extremes between which the fatigue-curves wander remain very near together. For II. the distance between these limits goes once beyond  $3^{\text{mm}}$  and reaches that figure three times only; for I. it never reaches  $3^{\text{mm}}$  and four times only does it go beyond  $2^{\text{mm}}$ , while for III. it exceeds  $2^{\text{mm}}$  only once and then just by a fraction of a millimeter.



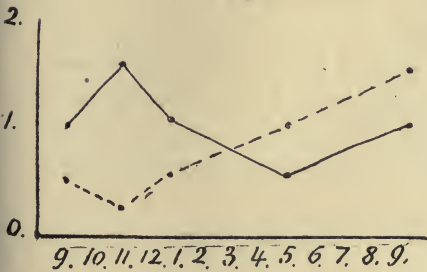
2. A striking and not to be expected lack of agreement between the curves for the two-threshold and those for the one-threshold. Concerning the four pairs of curves of I., only the thumb fatigue-curves agree tolerably well with each other, as the reader will see. With III. the fatigue and also the rest-curves for the thumb agree fairly well, but those for the head show more frequently opposite directions. The tracings of II. may be called satisfactory in this respect.

3. The corresponding forehead- and thumb-curves agree still less; neither in the tracings of I. nor in those of II. is there any general agreement to be found; on the contrary, the curves are on the whole the opposite of each other. III. distinguishes himself in that he shows two pairs of curves in agreement: the fatigue-curves of the one-threshold and the rest-curves of the two-threshold.

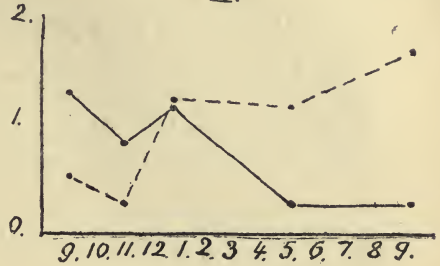
4. A comparison of the fatigue- with the rest-curves does not bring to light any general, decided tendency. The oscillations of the rest-curves are about as great as those of the fatigue-curves, although Griesbach states that "the sensibility under normal conditions [he means no fatigue] changes not at all or only very little." Sometimes the rest-curve ends higher above its beginning than in the case for the corresponding fatigue-curves; sometimes they follow each other almost parallelly. If we draw total average curves summarizing for each subject the four rest- and the four fatigue-curves, we get the tracings of Fig. II, in which the oscillations are so much reduced that we had to increase four times the scale of the drawing. The distances between the vertical extremes of each curve, expressed in millimeters, are as follows: for rest, 1 (I), 1 (II),  $\frac{1}{16}$  (III); for fatigue:  $1\frac{1}{4}$  (I),  $1\frac{3}{8}$  (II),  $1\frac{3}{4}$  (III). If we compare in each line the starting with the ending point, we find two of the rest-curves (I and III) ending exactly where they began and one (II) ending 1 mm. below; while of the fatigue-curves two end higher and one lower: + 1 mm. (I), +  $1\frac{1}{8}$  (II), -  $\frac{7}{8}$  (III). If we consider only the morning part of the tracings, we find that concerning I. two of the rest-curves end higher than they start, one ends at the same level and the other lower; while of the fatigue-curves two end lower, one at the same level and one higher. Concern-

Fig II.  
Resultant curves computed from Fig I.

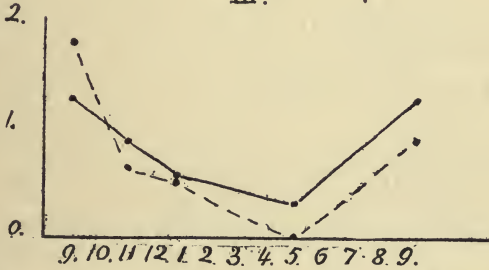
I.



II.



III.



The scale in Fig. II. is four times larger than in Fig. I.—i. e., four squares, instead of one, stand for one millimeter.

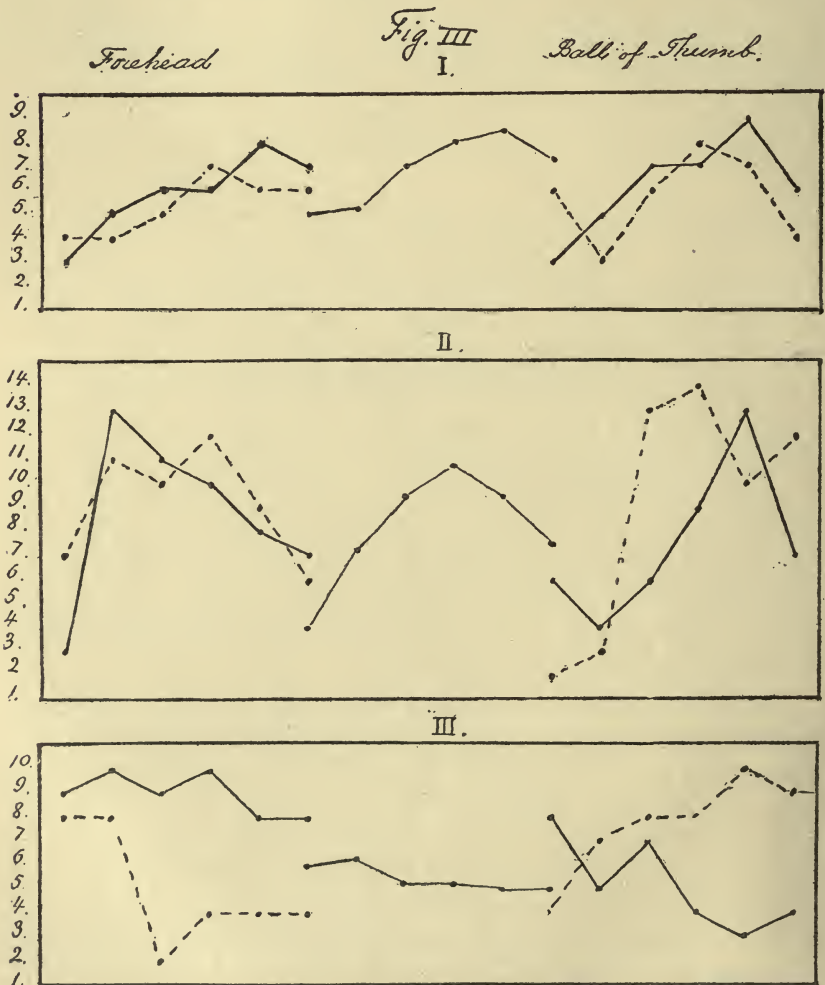
The starting points of the curves of Fig. II. have been arbitrarily determined.

ing II., two of the rest-curves end higher than they began and the two others lower, while the four fatigue-curves end higher. Concerning III., the rest-curves end lower in three cases and at the same level in the fourth one, while the four fatigue-curves end *lower* than their starting point.

It is clear that from Figs. I. and II. no general deduction regarding the effect of fatigue could be drawn with any confidence, and yet the curves put in regard the discrimination sensibility during three days of severe mental work and three days of rest. If the æsthesiometric method does not yield here unambiguous results, how could it be used in the class-room?

Let us pass to the curves of Fig. III. Each curve, with the exception of those in the middle, represents the changes in the discrimination sensibility as they occurred during one hour

of adding performed on five successive days. The distances from one point to the next, measured vertically, indicate the modification of the sensibility during one hour of adding. The hour was divided in two halves separated as follows: 1st day, no rest; 2d day, 5 minutes rest; 3d day, 15 minutes; 4th day, 30 minutes; 5th day, 60 minutes. The continuous lines represent the one-threshold; the broken line, the two-threshold. The curves in the middle are each the resultant of the four others belonging to the same subject. No value is to be given





to the position of the starting-point of the segments as, in order to get a curve, the end-point of one day was used as the beginning of the next.

Concerning Fig. III, we notice that: 1. As in Fig. I, the one-threshold curves agree only occasionally with the two-threshold curves. Compare, for instance, the thumb-curves of III. Those agreeing best are the forehead curves of II., and even then one of them indicates a decrease of sensibility during the adding of the third day, while the other shows an increase.

2. The agreement of the head- with the thumb-curves is no better than previously. In the case of II. and of III. the divergences generally reach opposition. In I. the one-threshold curves exhibit a noteworthy concordance.

3. Regarding the relation between fatigue and the sensibility, we have in the addition returns a means of measuring fatigue, and consequently of controlling the results yielded by the *æsthesiometric* method, which, if not absolutely reliable, is at any rate of some value. We should not take it for granted, even though we could assume that the initial psycho-physiological state of the subject was each day the same, that the degree of fatigue varies in inverse proportion to the length of the rest-period separating the two half-hours of work. The amount of work actually performed will be a much safer index of the fatigue increase. But can we accept the statement that the greater the amount of work, the greater the fatigue; and that, as far as the adding periods are concerned, all the work done is represented by the number of additions; and can we consequently assume that the fatigue increase bears, in the same person, a constant relation to the number of additions performed? Man is a too complex and not well enough unified machine for such an assumption to be true. Coexistently with the physiological activity involved in adding, many other processes take place equally efficient as modifying factors of the body metabolism, but not constant enough to constitute a fixed quantity. The muscular tensions which vary so much from one time to the other will, for one, contribute their share towards the fatigue increase and at the same time tend to bring about a reduction in the amount of the mental work performed. The

number of additions will not even represent the whole product of the physiological activity having a conscious correlate; the adding process is at times accompanied, at other times interrupted, by trains of thoughts and feelings varying greatly, from day to day, in duration and vividness. This supplementary mental work, the adding does not record; or if its effect is perceivable in the number of additions, it is as a *decrease* that it reveals itself. Stated in general terms, the objection here formulated is that the number of additions performed during a given time is not the whole of the psycho-physiological activity, and consequently cannot be an exact correspondent of the fatigue increase. Nevertheless it may serve as an approximate means, and therefore we give in the following Table the returns of the adding for comparison with the curves of Fig. III. It is evident that it would be sheer waste of time to compare these figures with the separate curves of Fig. III., as, if they agreed with one of them, they must necessarily disagree with another, since between head and thumb, as also between the one- and the two-threshold there reigns an apparently hopeless discrepancy. The only possible thing would be to make use of the resultants of the four curves of each subject, however slight the confidence deserved by a curve having such progenitors.

## A. FOR THE WHOLE HOUR.

I.	II.	III.
2d day -84	- 10.8	- 72.8
3d " +78	+ 17	+145.2
4th " +89	+374.2	-424.8
5th " +64	+ 38.8	+285.2

## B. FOR THE SECOND HALF HOUR.

2d day + 12	- 21.4	-101.9
3d " + 94	+ 10.6	+ 83.1
4th " + 59	+234.	-231
5th " +110	+ 49.6	+176.1

## C. DIFFERENCES BETWEEN THE FIRST AND SECOND HALF HOUR OF THE SAME DAY.

1st day -103	-148	+122	No Rest
2d " +5	-180	- 9	5 <sup>m</sup> "
3d " +95	-163	+ 12	15 <sup>m</sup> "
4th " +124	- 72	- 26	30 <sup>m</sup> "
5th " +180	+ 66	+ 41	60 <sup>m</sup> "

*A* gives the differences in the number of additions performed between the day named opposite the figure and the preceding one. For instance, *2d day - 84*, means that during the second day there was a decrease of 84 in the number of additions of the first day.

*B* takes into account only the second half of the hour and *C*, instead of comparing two successive days, gives the differences between the first and the second half hour of the same day. The influence of practice has been eliminated by the deduction of the practice gain. How the practice gain was determined may be ascertained from Professor Lindley's paper, already referred to. The figures of *C* represent, in our opinion, the nearest approach to a successful expression of the changes in the fatigue state, provided they are interpreted as follows: a falling off in the additions in the second half hour as compared with the first is a sign of fatigue; consequently we shall expect the curve to move upward whenever the second half hour yielded less work than the first, and vice versa. How far this interpretation is to be relied upon is not at all clear; but it seems to us the best use we can make of these figures. When construed in this way the resultant curve of subject II. follows remarkably well the figures; and, barring the segment of the first day, that of I. is also satisfactory. But inasmuch as the curve of III. does not at all reflect the oscillations shown by the figures, no general inference can be drawn.

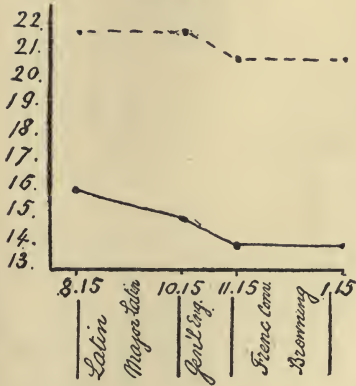
Against *A* and *B*, looked upon as fatigue indicators, it may be urged that the initial work-power of the subject is not taken into account, as it should in order that the figures be really indicative of proportional fatigue changes. For instance, the number - 84 is to be interpreted as indicating a smaller loss of work-power (less fatigue increase) during the second than during the first day. But if the initial fatigue was greater the second than the first day, a loss of 84 in the number of additions during the second day may very well mean greater loss of working power during the hour than was experienced the preceding day, although more work was done; we should then, if that supposition was true, expect the curve of the second day to show a greater rise than the one of the first, despite the fact that less work was accomplished.



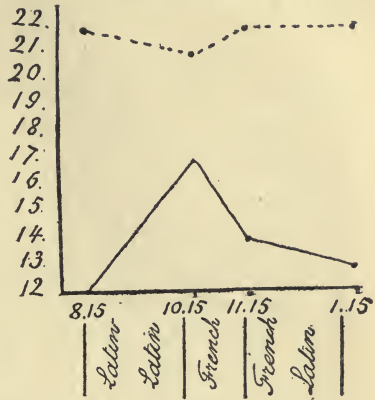
The measurements taken upon the Bryn Mawr students need not delay us long; they confirm the negative results of the others. Out of the thirty-six separate curves obtained, we pick out twelve (Fig. VI) possessing as fairly as possible the characteristics of the whole batch. The æsthesiometer was used at Bryn Mawr as Wagner used it—*i. e.*, instead of determining the one- and the two-threshold, the instrument was applied alternately with the points too far apart to be felt as one and too near together to give the impression of twoness; from these two extreme distances the threshold was gradually approached. To keep the attention of the subject and prevent the influence which regularly in the succession of the sensations expressed as 'two,' 'one'; 'two,' 'one' might have, the subjects were frequently touched with one point only. The measurements were taken without haste and with due regard for the circumstances on which exactness of result depends: absence of disturbing external stimuli; ignorance on the part of the subject of the method pursued; interruption of about one minute and a half after every four or five touches, etc. Moreover, to prevent the bewildering effect which too many touch sensations produce when crowded in a short space of time, we endeavored to reach safe results with as few touches as possible. Eight applications of the instrument were found the lowest practical number for each threshold. Under these conditions about five minutes were required for the determination of the two thresholds—forehead and cheekbone—taken at each sitting. No attempt was made in this series to find out and avoid the temperature spots, our intention being primarily to imitate Dr. Wagner's method, and to use it on persons younger than the Heidelberg subjects. Nevertheless we proceeded, even in these experiments, with more care than the German investigators. The students who served as subjects were requested to keep faithfully at work from 8 : 15 A. M. to 1 : 15 P. M.; they understood that we wanted them to get as tired as possible, and they willingly entered into our purpose. During the morning they attended either three or four lectures or recitations; the rest of the time they filled with private work. The two post-graduate students who took the measurements here recorded knew that the discrimination sensibility is

Fig VI.

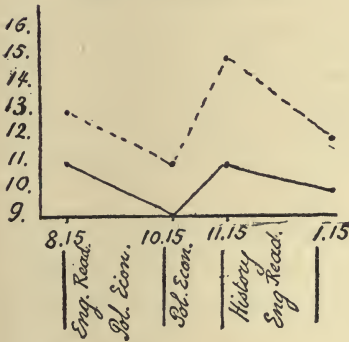
B. M. L. Age 19 1/2  
March 8.



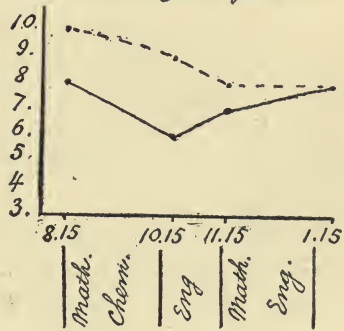
B. M. L. Age 19 1/2  
March 8.



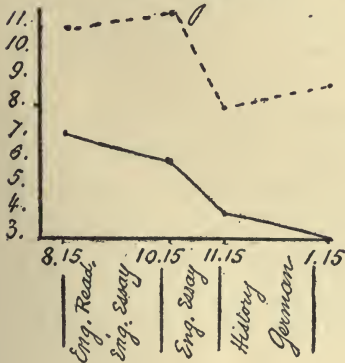
E. D. E., Age 20 1/4



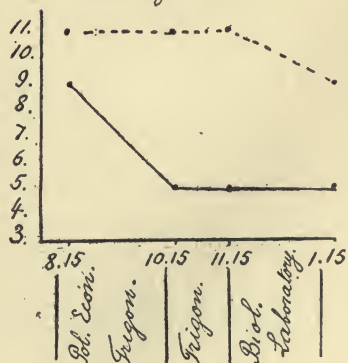
M. J. Age 21.



H. H. Age 18



S. Age 21.



thought to decrease with fatigue, and expected their returns to agree with this belief; instead of this, they show an almost constant increase in the sensibility. The figures increase only 15 times out of 108 possibilities as we pass from one measurement to the next, and only 5 of the 36 curves end higher than they begin; yet the last measurement was taken at 1:15 P. M., after five hours of mental work, interrupted only by the time necessary for the tests, the first of which was taken 15 minutes after breakfast. As to the correspondence of the forehead with the cheekbone curve, the reader will see that on the whole it is closer than in the previous curves, but how far yet from the admirable harmony reigning between Griesbach's tracings!<sup>1</sup>

The preceding facts warrant, it seems, the following general conclusion: If the ability to discriminate simultaneous tactile sensations is in some way under the influence of fatigue, it depends also and to such an extent upon other factors that it cannot serve as an index of the fatigue-state. These other factors are the temperature spots, the irregularities in the sensitiveness of adjacent bits of the skin surface, the temperature of the surface tested, the state of the blood circulation, the highly complex inner determinants of the intensity, duration, and kind of attention paid to the sensation, in so far as they are independent of fatigue, etc.

It is not our present purpose to enter into a discussion of the relative importance of these several factors; to do this profitably would require long series of skillfully planned and carefully carried out experiments. But we may, before closing, support our general conclusion by the results obtained and the opinion expressed by Tawney in a recent and painstaking work,<sup>2</sup> and also by a criticism of the method used and of the results obtained by Griesbach and Wagner.

According to Tawney—and this we have ourselves often noticed—it is of the greatest importance that the points of the æsthesiometer produce a sensation of equal subjective intensity. This cannot be secured by equal pressure of the points, since

<sup>1</sup> The full-line is the forehead curve; the broken line, the cheekbone curve.

<sup>2</sup> Guy A. Tawney, Ueber die Wahrnehmung zweier Punkte mittelst des Tastsinnes, Philosophische Studien, 1898, Vol. 13, p. 163.





Fig. VII

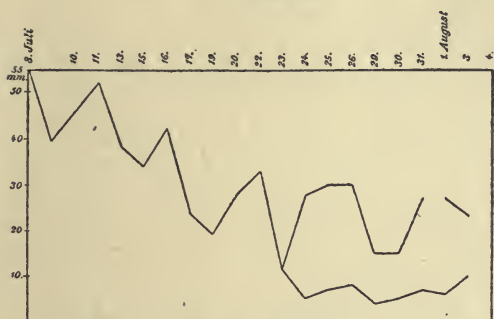


Fig. VIII

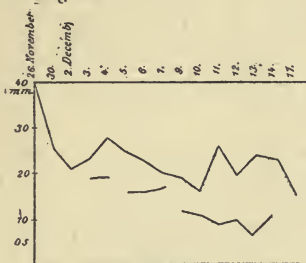


Fig. IX



Fig. X

Fig. XI

Figs. VII., VIII., IX., X. and XI. are taken from Guy A. Tawney's paper before mentioned.

neighboring bits of the skin differ considerably in the thickness of the insensitive superficial layer, and probably also in the sensitiveness of the deeper layer. He found, for instance, in one part of the shoulder blade that the point touching the skin lightly was felt more distinctly than the other, although heavily applied. Concerning the relative importance of fatigue and of the mood (*Gemüthslage*), he writes "Es Würde nämlich bemerkt dass die Gemüthslage der Versuchsperson eine sehr bedeutende Rolle spielt, während z. b. der Umstand dass sie bis 12 Uhr in der vorigen Nacht gearbeitet hatte, fast gar keine Rolle spielte." He found also that widely different results were obtained when the attention was transferred from the object touching the skin to the subjective sensation, or when its intensity was altered by means of suggestions in the form of information imparted to the subject on the purpose of the experiments. But the point of greatest interest to us in Tawney's paper is the great variations shown by the threshold of the same person, measured at the same spot and at the same time on different days. Fig. VII and Fig. VIII, pp. 16-19, give the thresholds of two subjects measured at 7:30 A. M., on the same part of the body (the dorsal side of the forearm in the case of Fig. VII; the dorsal side of the upper arm in the case of Fig. VIII) on successive days.

The thresholds recorded for these curves are, for each day, averages of four threshold-determinations. This, according to Tawney, accounts for the regularity of the decrease of curve VII, a decrease due to practice; but it does not prevent Fig. VIII from showing daily oscillations reaching almost 20 mm. We are not told how the preceding nights were spent, but the remark quoted above indicates clearly enough that, in the opinion of the author, irregularities of the curve cannot be explained by fatigue due to late work and insufficient sleep. Fig. IX shows similar oscillations; but as the time of the measurements was not always the same, we cannot make use of them. The time at which the tests recorded in Figs. X and XI were taken is not mentioned; we may suppose that the rule was followed, namely, to measure always at the same time of day. The following figures, representing the thresholds for the volar side of

the forearm, measured daily during 19 days, indicate again very great variations: 60, 52, 60, 64, 54, 52, 55, 50, 42, 40, 45, 40, 45, 40, 38, 41, 42. The table numbered XV in Tawney's paper exhibits a similar irregularity; the thresholds for ten days separated by intervals of one to six days' duration are: 15, 25, 30, 40, 45, 45, 25, 16, 35, 45. The preceding curves and figures are not picked out of a larger number; they represent the whole of the results obtained by Tawney bearing upon our investigation. He had himself no intention of studying the relation of fatigue to the discrimination sensibility.

When these results and those obtained by ourselves are put together, it becomes difficult to look upon the work of Griesbach, Wagner and Vannod with any other feeling than one of wonder. When the resultant curves of three persons for three comparable days of hard, persistent work and for three days of rest—to speak only of the part of our work which we offer here—move up and down apparently without any reference to a fatigue clearly felt and legitimately inferred from the work performed; when an hour of adding by three persons, during six days, yields no better result; when five morning hours of intellectual activity on the part of six college students interrupted by only short pauses, bring with them remarkably uniform increase in the discrimination sensibility; when, moreover, it is known that the same spot may yield on successive days and under apparently the same conditions, results varying up to 20 mm., the admirable consistency and uniformity of the results published by the said investigators become a problem whose solution is not easily found. To this should be added that these remarkable results have been obtained under circumstances far from favorable to exactness of return. Wagner, for instance, measured in 10 minutes—the length of the interval between the class sittings—from six to ten persons at one point, and Griesbach, in apparently the same time, two or three subjects in six different places. This appears to us verging on the impossible.

If we now turn to the consideration of the method used by the German investigators, we find that Griesbach is not very explicit in his description of the manner in which he proceeded. His method was the one we followed at Heidelberg:



only, judging from the rather vague utterances of his monograph and from a private communication, he allowed himself a good deal of freedom. How far one could, with the help of a little looseness in the use of the *æsthesiometer*—a looseness unavoidable, it seems, when the measurements are to be taken as rapidly as they were in this case—unconsciously influence one's returns when possessed by an idea which, if confirmed by experiments, would prove of great scientific and of much practical value, is an open question. We may be excused for formulating it on this occasion.

Wagner's method was similar to the one used in Bryn Mawr by our students and ourselves: too great and too small distances alternating until the threshold is reached. This procedure is decidedly to be preferred to the former under the circumstances in point, the alternation of distances giving clearly two, with distances yielding but one sensation, gives to the subject, from the very beginning, a clear knowledge of the difference with which his judgment is to deal. Otherwise, if, for instance, the experimenter begins with a very small distance and increases it until the threshold is reached, the subject who has not had the opportunity of comparing twoness with oneness will often show by his answers that he does not know what he is to call 'one' and what 'two.' We tested many persons with regard to this, and found in almost every case that the usual verbal instructions are not sufficient: before the subject is made acquainted with the sensations he is to call 'two' and 'one,' he may call 'two,' two points clearly below the threshold and give the same answer when touched by one point; this source of error will be greatly increased if the subject gets into his head that it is to his credit to feel two points as often as possible. This objection applies with all its force to the method used by Griesbach, since his subjects changed generally each day and were expected from the very first trial to give recordable judgments.

Wagner declares his results to be in perfect agreement with those of Griesbach, and concludes that "the *æsthesiometric* method of Griesbach is a practical means of determining the degree of fatigue and of comparing it quantitatively" (p. 12). There is shown, he holds, 'a clear relation,' between the dis-

crimination sensibility and six specified circumstances, among which we find not only the sort of instruction received, but also the teaching ability of the instructor. For the interpretation of separate turns of his curves he shows a boldness of which Griesbach was not guilty. No doubt his results are in close agreement with those of Griesbach, yet there is one particular in which the concordance appears defective; in looking over his tracings it occurred to us that their upward gain was made chiefly, if not entirely, between the first and the second measurement, and that they frequently ended below the point reached at the second test—*i. e.*, after the first hour of study. In order to reach a definite opinion on this interesting point the sum was made (1) of the measurements taken before the beginning of the lessons; (2) of those taken after the first hour; (3) of those taken after the last morning hour, with the results given below. Each table in Wagner's paper includes the measurements taken on from five to ten students of the same class, before the beginning of the morning's work and after each recitation of one day. There were usually five recitations, separated from each other by an interval of ten minutes. Each one of the figures composing the first vertical row of our tables is the sum of the figures representing the measurements taken before work in one of the tables of Wagner; the figures of the second row give each the sum of the measurements after the first class hour, and those of the third row the sums of the last measurements of the same table. We have added before the figures the names of the studies after which the tests were taken. We omitted the measurements taken after 'Religion' when given during the last hour, because the lesson was attended by only about half the students used as subjects. We acted similarly with gymnastics that intellectual work only be taken into account. Furthermore, when the last figure was missing we left out, for that day, the measurements of the person concerned.

TABLE A.  
MEASUREMENTS IN QUARTA.

55	French exercise,	79	Phædrus,	80	Thresholds of 6 subjects.	
42	Arith. (Rechnen),	67	Latin,	91		“ “ 8 “
86	French,	108	German,	120		“ “ 8 “
78	German,	110	Geom.,	135		“ “ 8 “
70	Geometry,	118	Latin,	106		“ “ 6 “
45	Arithmetic,	73	Latin,	97		“ “ 6 “
73	Arithmetic,	106	Latin,	136		“ “ 7 “
83	French exercise,	171	Latin,	173		“ “ 9 “
104	German compos.,		German,	175		“ “ 10 “
127	Latin exercise,	178	Latin,	180		“ “ 10 “
114	Geometry,	188	History,	167		“ “ 10 “
	Arithmetic,	160				
Totals: 877		1358		1460		88

The average increase of the threshold between the initial and the final test is, accordingly, in millimeters,..... 6.6  
Between the first and the second measurement,..... 5.5  
Between the second and the final measurement,..... 1.2

TABLE B.  
MEASUREMENTS IN UNTERTERTIA.

27	Latin,	35	Geogr.,	44	Thresholds of 4 subjects.	
47	History,	51	Latin,	43		“ “ 4 “
55	Greek Ex.,	77	Geom.,	65		“ “ 6 “
93	German,	114	History,	99		“ “ 8 “
57	Greek,	65	Geom.,	73		“ “ 5 “
83	Greek Ex.,	114	Natural Sci.,	107		“ “ 8 “
81	Latin Ex.,	107	Geogr.,	98		“ “ 8 “
97	Greek,	107	Geom.,	106		“ “ 8 “
87	Greek Ex.,	108	Natural Sci.,	118		“ “ 8 “
90	Latin Ex.,	119	Geogr.,	114		“ “ 8 “
Totals 717		897		867		67

The average increase of the threshold between the initial and the final test is, accordingly, in millimeters,..... 2.2  
Between the first and the second measurement,..... 2.7  
Between the second and the last measurement,..... —0.4

*Remarks on Table B.*—Wagner explains as follows the surprising fall of the figures at the end of the morning in the second series (47, 51, 43): “ \* \* \* alles Erstaunen verschwindet und die Theorie erhält gerade hier eine interessante Bestätigung, sobald man erfährt, dass alle 4 Stunden dieses Tages von angehenden Accessisten gehalten wurden,” p. 63. Unfortunately, the fourth series exhibits a similar drop (93, 114, 99) without ‘Accessisten.’



Notice, also, that the 6th (83, 114, 107) and the 9th series (87, 108, 118) exhibit inverse changes of the sensibility, although the subjects of study were for both days the same: Ovid, Latin, Religion, Natural Science.

TABLE C.

MEASUREMENTS IN OBERTERTIA AND UNTERSECUNDA.

35	Caesar,	71	History,	68	Thresholds for 6 subjects.	
75	German compos.,	126	Latin Gram.,	117		“ “ 8 “
32	Latin Grammar,	39	History,	49		“ “ 4 “
37	Greek,	36	Latin,	54		“ “ 5 “
72	Greek,	99	Latin,	96		“ “ 7 “
44	Algebra,	58	Xenophon,	67		“ “ 5 “
Totals: 295		429	451		35	

The average increase of the threshold between the initial and the final test is, accordingly, in millimeters,..... 4.5  
 Between the first and the second measurement,..... 3.8  
 Between the second and the last measurement,..... 0.6

What do these figures mean? Can they be reconciled with the claims made in favor of the æsthesiometric method? Are they to be interpreted as meaning that fatigue takes place almost entirely during the first hour, and remains nearly the same through four additional hours of work, one or two of which require as much exertion as the first? Before proceeding with this problem let us notice that, in this respect, Wagner's results agree neither with those of Griesbach nor with ours. During the first hour of work the sensibility of the three Heidelberg subjects increased (see Resultants, Fig. II). The same is true of the Bryn Mawr students, while the Griesbach curves continue to ascend more or less regularly after the first hour and generally end at the close of the morning (12 o'clock), higher than after the first hour—we refer chiefly to the cheek bone and forehead curves. It might be that this lack of agreement between the German investigators is due to the unequal difficulty of the subjects with which the morning's work opened in the respective classes to which their subjects belonged. As a matter of fact, in the classes with which Griesbach dealt, natural history and modern languages are frequently at the beginning of the schedule. But whether this be the ground of their disagree-

ment or not, the problem before us remains. It will not do to have recourse at this juncture to the different degree of attention required by the several branches of study, for, if we consider only the subjects of the last hour, leaving out of count the three middle ones, we see that they cannot be said to be clearly inferior to the first in respect of their fatigue-producing power; and even though they were of a somewhat less exhaustive kind, the curves would not thereby be explained unless it could be shown that between the first and the last hour the sensibility, in consequence of the recuperative quality of the three intermediary subjects, had nearly returned to the norm.<sup>1</sup> But to accept this would be equivalent, it seems to us, to giving up the claims of the believers in the method. It cannot be said either, by way of explanation, that the 10 minutes rest between each hour was enough to produce return to the normal; and that, consequently, the figures show the fatigue increase to have been, on the whole, about the same for each hour. It is evidently not true that two, three, four or five hours filled with the Gymnasium studies just mentioned leave the student just as fresh 10 minutes after.<sup>2</sup> We should rather be disposed to affirm that 10 minutes after the first hour of work a student might be quite fresh, while 10 minutes after the fifth hour he would be quite tired and often exhausted. Another explanation, plausible this time, but destructive of the claims under consideration presents itself to us: although nothing is said as to the place where the first tests were taken, we may assume that it was in the school building, the first measurement being taken as the students arrived from their homes. Now the usual condition of a young man after an early morning

<sup>1</sup>The intermediary subjects for the days on which the measurements were taken, were: History, Geometry and Phædrus; Phædrus, Grammar, and Geography; French, Gymnastic and History; Geometry and History; Drawing, Religion and Geography (this group is found only once); Drawing, Algebra, Ovid; Geometry, French, Greek and other similar groups. Considering Gymnastic, Wagner says (p. 126) summarizing: "The hour of Gymnastic has \* \* \* in  $\frac{1}{3}$  of all the students (according to the most favorable construction of the figures) produced a relative recuperation; in the two other thirds it induced a clearly marked [ganz ausgesprochene] fatigue."

<sup>2</sup>Or shall we assume that it is only during the first hours that the Gymnasium student really works and that during the rest of the morning, either because of exhaustion or of laziness, he foregoes all tiresome mental effort? Probably no gymnasium director would countenance such a supposition.

walk and the exhilarating encounter with classmates is one of comparative bodily and mental alertness. An hour later, after a recitation in Latin, Greek or Geometry, the blood circulation, the respiration and the general feeling may be assumed to have changed considerably. Under such dissimilar circumstances we should hardly expect to get comparable æsthesiometric results. We have taken some measurements with the hope of determining the influence of these physiological changes, but we have been unable to have our subjects fulfill sufficiently well the conditions necessary for exact experimentations to warrant any positive conclusion. The temperature of the part of the body tested and of the room in which the measurements are taken should also be taken into consideration in an attempt to interpret the curve-peculiarity under discussion. Wagner experimented during February and the first part of March; consequently both the temperature of the skin of the face and that of the room would change materially during the first hour. Loewenton found that the threshold was elevated by an increase of temperature of the room. We need not insist on the possible influence of these two factors; the reader will see how they might have combined to bring about the relatively low figures of the first measurements. If accepted, this explanation would invalidate the conclusions drawn by the German investigators: the chief rise of the curves would not be due to fatigue, but to other causes. Unfortunately, we do not know whether Griesbach's different figures must be interpreted as discrediting this solution, for we are not informed as to the circumstances on which the comparableness of the figures of the two investigators depends. As to our own curves, all that can be said on this point is that they do not show a corresponding rise during the first hour; and that the cause we have suggested for this rise as it occurs in Wagner's curves existed neither in the case of subjects I., II. and III., nor in that of the Bryn Mawr students, since the latter and I. and II. were tested in their own rooms, while the measurements of III. were taken 15<sup>m</sup> after his entrance into the rooms occupied by I. and II.

Returning to the general problem of the discrepancy existing between our results and those of the German investigators,



we may, in closing, advert to such thought as the following: the Heidelberg and the Bryn Mawr subjects might be, one and all, abnormal persons; or, we may have failed to get correct thresholds because of lack of skill in handling the instrument; or, our subjects were older than those of Griesbach and Wagner;<sup>1</sup> or, the discrimination sensibility of German youth is not comparable in its behavior to that of American men and women—thoughts which appear to us either inadmissible, or insufficient to silence the suspicion that the conclusions of the papers here considered are not well founded.

In this state of indecision we must let the matter rest for the present and until a thorough and systematic investigation of all the factors affecting the discrimination of simultaneous touches enables us to assign to fatigue its particular rôle. Two groups of factors will have to be taken into consideration: (1) those affecting the peripheral organs—temperature, thickness of epidermis, the peripheral blood supply, etc.; (2) those affecting the general psycho-physiological condition of the subject, and more especially his ability to attend. An *à priori* consideration of the influence possessed by these factors leaves but little ground for the hope that the discrimination sensibility to simultaneous touches may serve as a practical test of fatigue, for many of them (temperature, blood circulation) vary under unchanged fatigue conditions. As to attention—considered independently of its relation to fatigue—it lacks the constancy and steadiness which are absolutely required if the experimenter is to draw his inferences from a very small number of measurements. It is a rhythmic function, and, moreover, is readily and rapidly modified by the will to attend—a quantity that cannot be maintained constant.

<sup>1</sup> No age limit is set by them to the applicability of the pretended relation existing between the discrimination sensibility and fatigue. When testing persons of the age of our Bryn Mawr subjects, Griesbach found the same results as when dealing with younger persons.

# ON THE INVALIDITY OF THE ÆSTHESIOMETRIC METHOD AS A MEASURE OF MEN- TAL FATIGUE.

BY DR. GEO. B. GERMANN,

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While engaged in reading the literature on fatigue some two years ago, I became interested in Dr. Griesbach's investigation on the relation between mental fatigue and the discriminative sensibility of the skin.<sup>1</sup> Dr. Griesbach, it will be recalled, claimed to have ascertained a close and definite correspondence to exist between the extent of sensation areas and the fatigue incident to school and other mental work, the main hypothesis being that fatigue increases the size of such areas, while rest diminishes their extent on any defined portion of the skin. His interesting results apparently confirmed this hypothesis, but I doubted the validity of his method and therefore his results. That method consisted in rapidly increasing minimal distances and decreasing maximal distances between the æsthesiometer points until the extent of the sensation area was determined. Furthermore, apparently only one determination was ascertained in each case. Griesbach fails in his paper to indicate how he satisfied himself that he had obtained this end within any reasonable degree of accuracy. It is a matter of common experience that as the sensation-area limit is approached, tactile illusions become numerous, and any single determination near the limit may be more of a guess, or perhaps a purely illusory statement, than a safe judgment. The employment of the method of right and wrong cases is undoubtedly the safest method whereby to determine the accuracy of a series of judgments relative to tactile discriminations.

Wishing to get at the facts of the case, I undertook the following investigation during the period from February 24 to

<sup>1</sup> *Archiv für Hygiene*, 1895, Vol. XXIV., 124-212.

March 25, 1898. As subject I made use of my sister *S*, age twenty-three, a student at Barnard College, an earnest and diligent worker, health good, nervous condition normal.

The method employed was that of right and wrong cases. Jastrow's æsthesiometer was used during the entire investigation. This æsthesiometer is so constructed that when the points are placed upon a horizontal surface the pressure upon that surface is equal to the weight of a constant portion of the instrument. In order to obviate the inequality of pressure incident to tipping the æsthesiometer sidewise in securing a one point contact, the instrument was slightly modified so as to secure the desirable equality of pressure without the usual inclination.

All of the usual and necessary conditions attaching to æsthesiometric experiments, such as equality in the temperature of the room, absence of undue surface tension of the skin, blind-folding the subject, securing focalized attention, etc., were carefully observed and rigidly adhered to. All determinations were made upon a circumscribed area of the skin of the back of the right hand between the second and third metacarpals and about two-thirds distant from the corresponding carpals. A previous series of morning determinations, checked by means of the method of right and wrong cases, had indicated the length of the sensation area of the circumscribed region tested to be, on the average, a slight fraction of a millimeter over two centimeters. This (2 + cm.) was the constant distance between the æsthesiometer points employed during the investigation.

Fifty contacts were made during each of the first thirty-five tests, while during each of the other seven tests I had an opportunity to make one hundred contacts, thus affording a total of 2450 separate discriminations to be made by *S* during the progress of the investigation. Each contact lasted about one-half a second. An interval of ten seconds was allowed to elapse between successive contacts. On 27 out of the 30 days covered by the investigation a total of 42 tests was made. Of these 42 tests, 20 occurred in the morning between 8 and 10 o'clock, previous to any definite study, while the remaining 22 tests were made in the evening, slightly distributed, but for the most part



between 9 and 10 : 15 o'clock. At least eight hours of the interim between the morning and the evening experiments were always fully occupied by *S* with her collegiate studies, of which two hours' work usually preceded the evening test.

In collating my data I have, in order to reduce the results of the experiments to a very simple form, determined (*a*) the total percentage of errors in discrimination occurring during each test, (*b*) the percentage of errors occurring during each test in the discrimination of two points only, and (*c*) the percentage of errors occurring during each test in the discrimination of one point only. The references to (*a*), (*b*) and (*c*) are in the following statements designated by total, two and one, respectively. The results of the investigation may be most readily collated as follows :

(1) MORNING.			(2) EVENING.	
	AVERAGE.	VARIATION.	AVERAGE.	VARIATION.
Total.	15.1 %	6.8 %	12.6	9.1 %
Two.	17.3 %	10.1 %	20 + %	16.4 %
One.	12.8 %	9.5 %	5 +	5 %

*Explanation.*—Glancing at statement (1) we perceive that during the morning tests the total percentage of errors arising in the discrimination of both two points and one point amounts to 15.1%, with a variation of 6.8%; that the percentage of errors arising in the discrimination of two points alone amounts to 17.3%, with a variation of 10.1%; and that the percentage of errors arising in the discrimination of one point alone amounts to 12.0%, with a variation of 9.5%. The interpretation of statement (2), which refers to the evening tests, is to be made in the same way.

Of the total number of tests, 14 pairs were conducted on the morning and evening of the same days. It is both interesting and significant to consider these pairs apart from the entire series, and I believe that the following statements will serve to throw the results into bold relief.

	(3) A. M. = P. M.	(4) A. M. > P. M.	(5) A. M. < P. M.
Total.	4	8	2
Two.	1	9	4
One.	2	9	3

*Explanation.*—Statement (3) indicates the number of times that the morning (A. M.) and evening (P. M.) tests of the same day contained the same total percentage of errors in the discrimination of both two points and one point (total), of two points only (two), and of one point only (one). Statement (4) indicates similar results with respect to the number of times that the percentage of errors was greater in the morning than in the evening; while statement (5) refers to the number of times that such percentage of errors was less in the morning than in the evening.

A comparison of statements (1) and (2) brings to light several important facts. The percentage of all errors occurring during the morning series is  $15.1\% \pm 6.8\%$ , while the percentage of all errors occurring during the evening experiments is  $12.6\% \pm 9.1\%$ . That is, less errors occurred in the evening than in the morning, if the variation be discarded. Including the variation, we have practically equal results at the upper limit, while at the lower limit the errors are less and in favor of the evening tests. It is also readily seen that the percentage of errors in the discrimination of two points is slightly less in the morning than in the evening (this taken by itself would favor Dr. Griesbach's hypothesis); and that the percentage of errors in the discrimination of one point is more than twice as great in the morning than in the evening.

If we now examine statements (3), (4) and (5), the general trend of my results will become much more clearly defined. We see at a glance that out of the 14 days there considered, on 8 days the total number of errors was greater in the morning than in the evening, on 4 days the total number of errors of both morning and evening was equal, and on only 2 days did the total number of errors in the evening exceed the number occurring in the morning. Furthermore, on 9 days was the number of errors occurring in the discrimination of two points greater in the morning than in the evening, on 4 days less in the morning than in the evening, and on 1 day equal. The interpretation of the results of the one-point discrimination can be readily made in a similar manner.

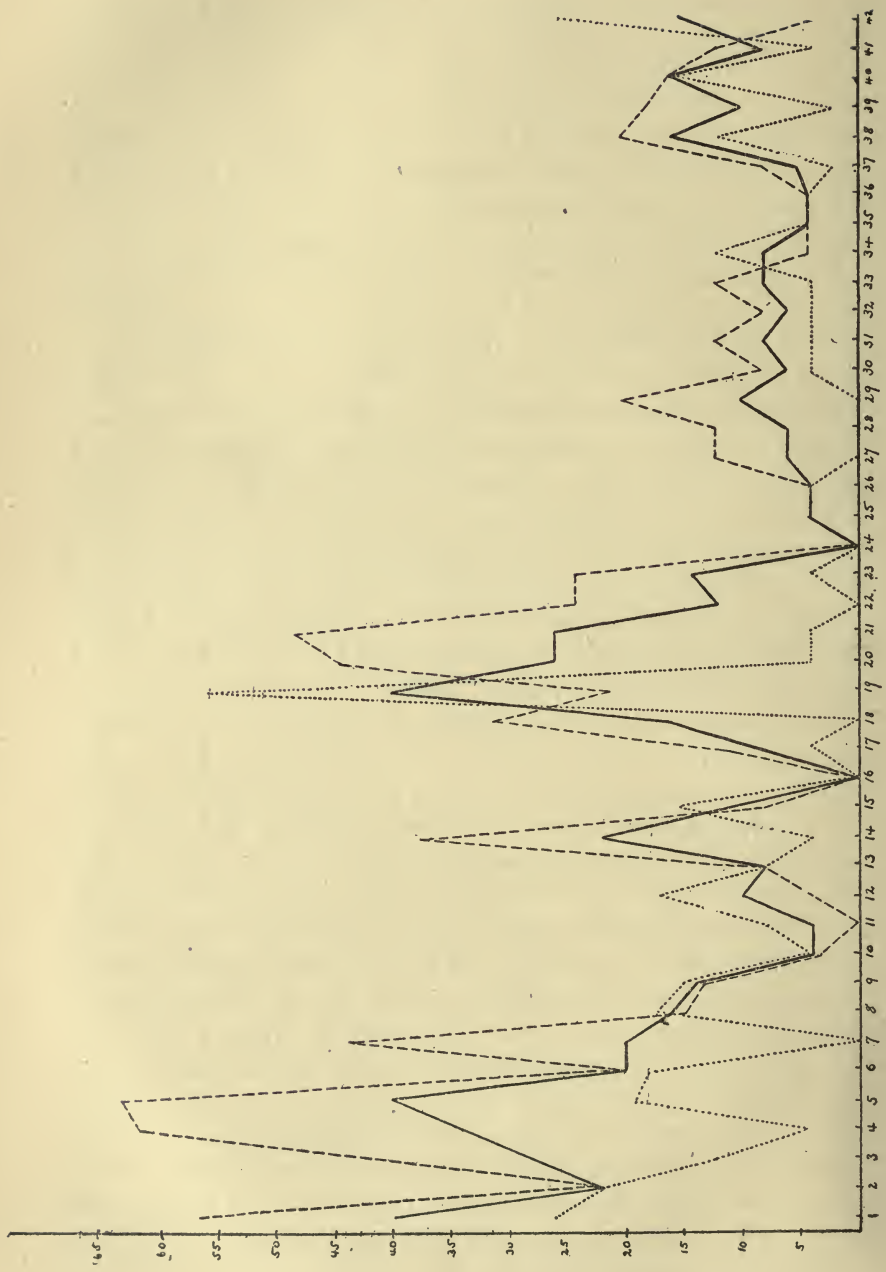
Now according to Dr. Griesbach's hypothesis, mental fatigue

diminishes cutaneous sensibility, and this diminution in sensibility is normally accompanied by and correlated with an increase in the extent of any single sensation area. Were this normally and universally true, then in a series of experiments where the distance between the æsthesiometer points remained constant we should be led to expect an appreciable increase in the number of errors in discrimination, at least in the discrimination of two points, toward evening and after a day of severe mental work. But my results plainly indicate that in a sufficiently prolonged study of these phenomena in the case of a normal, healthy and active student no such appreciable increase in errors occurs. In fact, an examination of the above statements (1) to (5) *in toto* may have the tendency to force the conviction that just a diametrically opposite condition of affairs prevails; so that were I inclined to be rash I might be tempted to advance the hypothesis that, in the case of at least one student, mental work and its concomitant nervous strain have a tendency to refine cutaneous discriminative sensibility, probably owing, I should then be tempted to add, to a general hyperæsthesia induced by a general diffusion of neural energy. But I do not advance any such hypothesis.

The results of this investigation are summarized in the accompanying curve.

*Explanation of the Curve.*—The line of abscissæ represents the successive tests, while the line of ordinates represents the percentage of errors occurring during each test. The three main lines of inquiry are here indicated. The dash curve indicates the percentage of errors occurring during the tests in the discrimination of two points; the dotted curve indicates similar errors in the discrimination of one point, and the continuous heavy line indicates the total percentage of error in each test. The following numbered tests represent morning experiments: 2, 5, 6, 8, 10, 12, 13, 15, 17, 19, 21, 23, 25, 29, 31, 33, 37, 38, 40, 42. The others represent the evening experiments. The following tests occurred in pairs—*i. e.*, on the morning and evening of the same days: 6 and 7, 8 and 9, 10 and 11, 13 and 14, 15 and 16, 17 and 18, 19 and 20, 21 and 22, 23 and 24, 25 and 26, 29 and 30, 31 and 32, 33 and 34, 40 and 41. It will be observed that the





above continuous heavy curve contains several of the characteristic properties of a practice or habit curve. This is what we should be led to expect from the work of others, who have ascertained that a refinement in tactile discrimination ensues as the result of the continued exercise of this sense. It may, furthermore, be of interest to note that during the progress of the experiments *S* several times remarked of her own accord that she believed she could feel the two points better than at the beginning of the series. Beginning with the twenty-fourth test, the variations are not so great as previously, nor are the percentages of errors very large. Yet the record of the subjective condition of *S* indicates a state neither more nor less favorable or unfavorable toward the end than at the beginning. In order to anticipate any objections that might be raised on the ground of the experimenter's clumsiness at the beginning of the series, I wish merely to remark that I thoroughly tested my method and gained facility before applying it. Of course, added practice begets added facility.

From the above results, I believe, we may reasonably conclude that the æsthesiometric method in a special normal case, at least, does not furnish a constant nor even relative index as to the amount of mental fatigue experienced by the individual. I have found in several investigations on sensation areas, by Judd and Tawney, an enumeration of normal fluctuations that correspond quite closely with the amplitude of variation which Griesbach, and more recently Wagner, ascribe to the influence of fatigue. Furthermore, the subjective state of *S* was carefully ascertained during the progress of the investigation. A general comparison of that subjective condition with the percentage of errors during each test gives further evidence in support of my conclusion that *in at least one normal case the percentage of errors in cutaneous tactile discrimination bears no constant nor even relative correspondence to the mental fatigue experienced by the subject.*

I am convinced that, in special cases, the æsthesiometric method is absolutely inadequate for the determination of mental fatigue. Moreover, I strongly doubt its validity in any case.

## A PLEA FOR SOUL-SUBSTANCE. II.

BY W. P. MONTAGUE,

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*'De Natura Animæ.'*

This inquiry into the nature of the soul, or, more exactly, into the nature of soul-substance, is the sequel to a former paper on the existence of soul-substance. The contents of that paper were as follows:

1. An introductory statement of the causes for the unpopularity of conceptions of substance in modern philosophy and particularly in modern psychology.

2. A description of the central problem of explaining the apparent interaction of the world of Mind or teleological law, and the world of Matter or mechanical law.

3. An outline of the five hypotheses actually used in the solution of this problem of the seeming causality between incommensurates, to wit:

*a.* Absolute Teleology—the denial of efficient causes.

*b.* Materialism—the denial of final causes.

*c.* Occasionalism—the co-reality of mechanism and teleology admitted—their mutual interaction being explicable only by miracles.

*d.* Parallelism—the admission of both realms as real, but their apparent interaction explained as an illusion due to a complete parallelism.

*e.* Spiritualism—the theory of a soul-substance different from mind and matter, yet partaking of the nature of both—therein explaining the possibility of real causality between the two spheres.

4. An exposition and attempted refutation of the first four theories—especially of Parallelism.<sup>1</sup>

<sup>1</sup>In this refutation of Parallelism I made use of the fact that concomitant variation excluded Parallelism. There seem to me to be a certain obscurity and



5. An indirect proof of the existence of soul-substance based on the failure of all other possible solutions of the problem. Direct proof based upon the axiom of "No action at a distance."

6. Conclusion—exposition and proof of the three requirements to be fulfilled by a valid conception of Substance in order to distinguish it from (*a*) the 'Ding an Sich,' (*b*) the 'additional attribute,' (*c*) the 'totality of attributes.'

It is the explicit purpose of the present paper to show the nature of the soul-substance the existence of which is to be regarded as proved in the first paper. To accomplish this task it is necessary to show that experience affords us an example of a mode of sequence which, while it is neither merely mechanical nor merely teleological, is nevertheless (1) simple and intelligible in itself; (2) related to efficient and final causality as genus to species, and (3) as species to genus.

Such a mode of experience would be the direct expression or definition of the nature of soul-substance. Its substantiality

imperfection in the argument as there given, and I therefore take this opportunity of supplementing it by the following statement:

Notwithstanding the fact that Mill (*Logic Bk. III. Ch. 8*) regarded the Method of Difference as superior to the Method of Concomitant Variations, yet it seems to me that the latter method is by far the more cogent, supplementing, as it does, the Method of Difference very much as that method supplements the Method of Agreement. The Method of Agreement gives a probability that *A* is at least part of the cause of *B*. The Method of Difference gives a certainty that *A* is at least part of the cause of *B*. The Method of Concomitant Variation gives a certainty that *A* is part of the cause of *B*, and also a probability that *A* is the whole cause of *B*, which probability approaches certainty as the concomitance approaches perfection. The ground of our belief that *A* is not merely a part or 'collocation' of the cause of *B*, but the whole cause itself, is the fact that for every part of *B* there is a corresponding part in *A*, and that there is nothing in *A* without its correlate in *B*. The concomitant variation of the ratiocinative and physiological sequences, which is admitted by the parallelist to be perfect or complete, carries us beyond parallelism; for if it is complete, it implies a correspondence of every infinitesimal part of the one process to every infinitesimal part of the other. Such a type of relation is perfect as to its homogeneity and is all that we can mean by causality. Just as two parallel lines when prolonged to infinity cease to be parallel, and just as two similar objects if their similarity were infinite would cease to be similar and become identical—just so a psycho-physical parallelism when it is made as perfect as it claims to be—and must be—ceases to be parallelism and becomes causality. In short, the parallelistic theory when closely examined is seen to be necessarily and peculiarly self-transcendent.

with respect to the mental and material attributes would be provisionally manifested in its appearance as the common Limit approached on the one hand by a series of teleological sequences, becoming more and more perfect, *i. e.*, more free from the taint of contingency—and on the other hand, by a series of mechanical sequences approaching mechanical perfection or absolute independence of all teleological references.

The resemblance between the concept of Limit and the concept of substance is sufficiently striking to demand careful consideration. What is a mathematical limit? It is the goal or end approached by a series, *e. g.*, the sum of  $1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \dots$  approaches 2 as its limit. Now 2 is a perfectly definite thing with a perfectly real and definable nature; at the same time it differs absolutely from the sum of  $n$  terms of the series, when  $n$  is any number we please. If we represent the sum of the first  $r$  terms of this series by  $\Sigma_r$ , we may express our series thus:  $\Sigma_1 \Sigma_2 \Sigma_3 \Sigma_r \dots 2$ , where 2 is the limit or last term of the series. It is to be noted that the members of this series (so far as they are viewed serially, *i. e.*, as interrelated) have one common quality which makes us classify them as members of one series. The influence of the limit of a series is present throughout the series determining the relations of the members to one another precisely as the Universal of a class is present in each of the particular members.

The limit differs, however, from the mere Universal in two highly important points:

1. While it is admitted by all except Platonists that the Universal can never exist apart from or independently of its particulars, yet it is perfectly evident that the Limit can and invariably does exist as prior to and aside from the members of the series which approach it.

2. The Universal can never be made a member of its own genus (although Aristotle accused Plato of doing that very thing); while, on the other hand, the Limit is always a member of the series which it determines, *e. g.*, 2 is the last member of the series given above, and it is also a member differing from all previous members in that it alone is an integral number. Thus it appears that the Limit has not only the generic char-

acter common to all universals, but that it also has the individual character of existing and of being known independently of anything outside. And last and most remarkable, it has the characteristic of being a member, and a wholly unique member, of its own class (series).

When we affirmed that the substance concept must possess the three qualities of being :

1. Self-intelligible.
2. Related to its attributes as genus to species.
3. Related to its attributes as species to genera. We might well have felt that modern philosophy was fully justified in repudiating such a thoroughly inconsistent and paradoxical notion as that of Substance. And yet in the familiar and useful conception of a Mathematical Limit we are able to see with the greatest clearness and certainty all of these three essential properties of Substantiality. By virtue of its knowability and definiteness, the concept of substance, like its mathematical brother the Limit, differs from Pure Being or from the Ding an Sich; by virtue of its generic character it differs from all particular qualities or atoms, and by virtue of its specific character it differs from universals or ideas.

In view of these considerations we shall be guided in our search for the nature of soul-substance by the well-known method of Limits. We have for our problem given the nature of the attributes (*viz.*, mind and matter), and the fact of their interaction—to discover the nature of the Medium (*viz.*, soul-substance), in virtue of which the relation of these attributes is possible.

Our undertaking will be divided in two general parts.

1. The search for the limiting forms or perfect types of mechanical or material relation.
2. The search for the limiting forms of teleological or mental relation.

First, then, we have to seek for the limiting or perfect form of mechanical causality or of the relation between facts as such. Take the following case: I hear the word 'Jacques,' and immediately there presents itself to my mind a picture of a melancholy fellow in a green doublet; following upon this picture there comes another picture of another melancholy man in a



red cloak. I am a school boy commanded to write a composition on 'As you Like It,' and I write the sentence, "Jacques resembles Hamlet." This is a typical psychical sequences, and without attempting the impossible task of a complete analysis of what occurs, it will at least be useful for our purpose to note some of the more obvious factors at work in the process. In the first place, Hamlet and Jacques both possess the quality of 'melancholy,' hence I have a rational ground for asserting their resemblance. Hamlet is associated with Jacques as co-member of the same species. This then is the teleological ground of my judgment regarded as a psychological sequence, and when so regarded it is usually called Association by Similarity. In the second place, I write down the sentence, "Jacques resembles Hamlet." I write this because I want to finish my exercise in composition as soon as possible in order to be free to go skating. I do it to fulfill a want or need of my immediate organism. This desire for satisfaction, or aversion to dissatisfaction, may be called the organic or biological cause of the action. In the third place, the image of Jacques in a *green* doublet calls up the image of Hamlet in a *red* cloak—why this change of color? Upon reflection I cannot remember ever having seen Hamlet dressed in red, and yet it is this color and no other that is presented with vividness to my mind's eye. We know, however, that green and red are complementary colors, and that one is apt to call up the other owing to what is probably a chemical change in the substance of the retina. Let us then call this the chemical cause of the process. And now there is one more type of causality at work in the production of this judgment. It so happened that I read 'Hamlet' and 'As You Like It' together. As a consequence of this fact, the sense impressions of the one play are very closely bound up with those of the other. The two sets of personages are associated by Contiguity. No one doubts the validity of this psychical law of Association by Contiguity. Some psychologists, in their praiseworthy ambition to banish final causes from their science, even go so far as to reduce Association by Similarity to a mere complex kind of Association by Contiguity. Inasmuch as Contiguity is a mechanical and temporal affair, while Similarity

is teleological and non-temporal, we may assume, on the strength of the criticism of Psychophysical Parallelism given in the former paper, that this attempt at simplification is doomed to failure. It may perhaps be asked on what grounds we select the law of Association by Contiguity as the distinctively mechanical type of mental causality. By way of answer let us consider what probably happens when two ideas are associated by Contiguity. A sense impression  $\alpha$  is suggested to the mind at a given time  $t_1$ . Another sense impression  $\beta$  is suggested to the mind at a later period of time  $t_2$ . Physiological psychology warrants the supposition that at the moments when  $\alpha$  and  $\beta$  are perceived there are two physical modifications  $a$  and  $b$  induced on the matter of the brain, and the same science also warrants us in supposing that, corresponding to the mental process of  $\alpha$  arousing  $\beta$ , there is a physical process consisting in the communication by spatial transition of something in  $a$  to something in  $b$ . Experimental psychology proceeds further, and assures us that, other things equal, the rapidity and the certainty or constancy of the association between  $\alpha$  and  $\beta$  varies inversely as the length of the time interval separating  $t_1$  and  $t_2$ . Granting these facts, we have the case of a mental sequence conforming exactly to the laws which govern a purely physical change, *i. e.*, a change from one position in space to another. The rapidity and certainty of this change moreover are measured by the 'distance' between the two positions. From this it follows that we are justified in the assertion that in Association by Contiguity we have a case of psychical sequence conforming to the quantitative laws of mechanics.

So much for the four kinds of causes which we may call the Teleological, the Biological, the Chemical and the Mechanical. Let us now leave the domain of mental life in which these four kinds of cause are usually found operating together, in order that we may study in comparative isolation, and so far as may be in serial order these several types of relation. At one end of the series we have purely 'final causality,' at the other end we have 'efficient causality.' Between the two we have the biological causality, which is predominantly teleological, and the chemical causality, which is predominantly mechanical. When

we leave the neutral or mixed ground of psychology for the field of biology, what difference do we notice in the laws of the two sciences, as to their respective types of relations between facts?

In biology, all changes or sequences are explained on the basis of the desires or needs of the organism. Two phenomena  $\alpha$  and  $\beta$  are seen to follow one another in time. If the sequence fulfills the three conditions requisite for the valid inference of causality, viz., mutual presence and absence and concomitant variation—then the biologist is justified in assuming that  $\alpha$  and  $\beta$  are connected with some specific need of the organism and therein connected with one another. This medium of relation has two aspects—a *qualitative* aspect which depends upon the kind of organism in question and the particular circumstances in which it is placed, and a *quantitative* aspect which is simply the strength of the desire. Given the knowledge of these two conditions the actions or sequences of an animal may be predicted. In the higher organisms the qualitative aspect is much more pronounced than in the lower organisms. In the case given above in which I associated Hamlet with Jacques, the quantitative aspect of the sequence, regarded biologically, would be the degree to which my judgment of resemblance contributed to the preservation of my organism. Obviously this was very small indeed. If I had made any other assertion or had failed to make any, the vitality of my organism would have suffered to an extent almost inappreciable. Supposing, however, that, instead of desiring to write a composition on a play of Shakespeare's, I had desired to satisfy a particular craving for a certain kind of food—in this case the quantitative aspect of the causal relation would play a much greater part. In a healthy organism all cravings are normal, and the satisfaction of any one is a direct contribution to the vitality of the animal. In so far as these cravings are numerous and varied the same needs of the organism can be satisfied by different objects. An animal desires one kind of food, but if this is unattainable, his desire can be almost as well satisfied by another. In short, just what the particular acts of an animal will be depends upon qualitative conditions much more than on conditions of quantity. Nevertheless as we descend in the scale of organic complexity the qualitative factor



in the actions decreases in importance, until in the lowest types of animals or the highest types of vegetables we find a few well-defined desires, usually arranged in a pretty definite hierarchy, and it becomes more and more difficult to satisfy one desire with the objects of another, or to leave a desire unsatisfied without destroying the whole organism. We express this decrease of qualitative and increase of quantitative causality by saying that the lower the organism the less selective are its acts, the more is it dependent upon or determined by its environment, and the more certainly predictable are its changes. Its nature is less *intrinsic* and more *extrinsic*. Finally when in the descending scale we leave the lowest type of vegetable and enter upon the study of the actions of the crystal, we find the qualities of this semi-organism arranged not merely in a general hierarchy, in which each desire is indefinitely stronger or more important than its neighbor next below, but rather do we find a state of things in which each quality bears a definite and mathematically determinable relation to every other quality. The changes in the crystal become subject to the *à priori* laws of quantity when once we have learned empirically the specific or qualitative nature of the crystal. We may put the matter thus—why and how the crystal should be what it is is not explicable, *i. e.*, not dependent upon any objects external to it, but being what it is, all its changes may be explained or predicted.

Let us now pass from the sphere of Biological Causality to that of Chemical Causality. At first sight the new class of phenomena does not appear to differ very much from the phenomena of crystallization. We have a substance known to possess certain properties or qualities, known also to be decomposable into a definite number of certain other and simpler substances called elements, and finally known to possess definite and unchangeable relations both qualitative and quantitative to all other substances. We notice, however, that we can decompose a chemical substance into its elements and then put these elements together and get back the original substance. Now with no type of organism or of crystal is this reverse process possible. And this is the most noteworthy difference between the substances of biology and the substances of chemistry. The chem-

ical substance has apparently no intrinsic life of its own, not even a crystalline power of initiative. It has, to be sure, a very rich qualitative nature, but we can force it to run through the whole gamut of its changes simply by altering its relations to other substances. An organism, however, refuses to be put through its tricks against its will. Break a crystal or an organism and you cannot mend it. Break the chemical compound and usually nothing is easier than to mend it. Hence in Chemical Causality we first come to what appears to be a *reversible series*—the possibility of a change which is not also a growth. A chemical substance can be changed in two directions—can grow old and can grow young, *i. e.*, not grow at all, but only alter. We have, to be sure, the interesting fact that all chemical processes have strong preferences of direction in these reversible changes. It is very easy to produce water by combining hydrogen and oxygen, while it is somewhat more difficult to decompose the water into its elements. And this fact is interesting, I say, as showing that the break between the organic and the inorganic world may not be an absolute matter, but only one of degree. A chemical substance which persisted in altering in one direction only in spite of all external agencies could very properly be classed as an organism.

As we turn from the more complex substances to the less complex, we naturally find the qualities of the substance growing more and more simple and the quantitative factor coming more and more into prominence. Until the discovery of Mendeleef's Law it seemed that the process of simplification had come to a final stop with the classification of all chemical substances into various quantitative compounds of the original elements. Mendeleef's Law, however, shows that these elements are not only related to one another quantitatively, but that the several qualities which distinguish the elements from one another form a somewhat irregular, though undeniable series, analogous in type to the series of spectrum colors. The nature of this series is such as to enable us to predict the qualitative nature of an undiscovered element simply from a knowledge of its quantitative relation to Hydrogen. Thus in this last discovery we see the element of quantity all but supreme, and it is

an easy step from these quantitatively related elements of Physical Chemistry to the quantitatively related states of matter which form the subject-matter of physics proper. Ice changes to water and water to gas, and these three qualities are all produced in turn by simply changing the motion of the molecules. Motion, however, is but a function of Space and Time, and to reduce all change to a change of molecular motion is equivalent to reducing all causality to the change of spatial position by a material body. The laws that govern molecular motion are not yet known, hence physics is still to some extent dependent upon the empirical observation of qualities. But now the qualitative element is merely the *ratio cognoscendi* of the causal laws and not as in biology, the *ratio essendi*. In mechanics and kinematics we at last arrive within sight of pure quantitative causality untainted with any spark of teleology, *i. e.*, of quality, unless indeed the quanta of mass and distance be themselves called qualitative.

Mechanical change is change of position. Mechanical causality is the law which governs this change of position, and it is a simple function of the initial velocity, the mass and the distance. The velocity or measure of motion is, as we have seen, simply the limiting ratio of a particular filled space to a particular filled time, while the mass of a body, as Karl Pearson so well shows, can be expressed or measured in terms of the acceleration produced upon other bodies. The velocity of the falling apple has a definite acceleration, which is the effect and measure of the mass of the earth. Since mechanical change can be adequately expressed wholly in terms of space and time, and since time relations permit of spatial representation, we have it in our power to symbolize adequately every mechanical change by a geometrical graph, called by its discoverer a 'Hodograph.' When a body moves according to mechanical law we can regard any antecedent state of that movement as the cause of every consequent state or effect. In short, it is only in the case of the freely moving particles that we can accept with a clear conscience the Humean identification of causality with universal sequence. If we feel it necessary to ask for the third substance, or thing in virtue of which the two terms of ante-



cedence and consequence are causally united, we simply point to the empty time and space intervals separating the two terms. The very fact that the pure movement between two 'space-time' or 'Hodographic' points is not only a continuous change in the popular sense of the word continuous, but a homogeneous or truly continuous change, is enough to satisfy the demand of reason for the third thing or medium as ground of relation between two terms. Two commensurate quantities are just as truly related through the fact of continuity as are two qualities through the fact of participating in a third or generic quality.

Now not only has Professor Pearson (after Clifford) reduced Mechanics to a species of geometry, but geometry itself is reducible to a species of algebra, called analytic geometry. In virtue of this latter science we are, with respect to a fixed point chosen arbitrarily and called an origin, able to express all position in space as a complex algebraic quantity, the degree of complexity depending upon the number of axes necessary to distinguish every point from every other.

If all positions were confined to a straight line, we could determine each of them by a single algebraic quantity ( $x$ ). If, again, all our points were in a plane we should require a dual quantity for the definition of a point ( $x, y$ ). In our actual three-dimensional space we require three axes from which to measure, and consequently a point can only be algebraically defined by a three-fold quantity ( $x, y, z$ ). When we introduce in addition to the merely spatial relations the *kinematical* factor of the temporal velocity with which the particles are altering their positions, we are obliged to bring a fourth element ( $v$ ) into our quantity in order to define it as distinct from its 'spatio-temporal' or hodographic neighbors; and finally when we take into consideration the *mechanical* factor of Mass or accelerated velocity ( $w$ ), we may be said to be dealing with changes in a five-dimensional world, and consequently to require a quantity of five-fold complexity in order to express the whole state of a body with a given mass and velocity, at a given point in space and time, in such a way that its future states, *i. e.*, its future relations to another similarly determined body (*viz.*, the center

of mass of the system in which it is), may be predicted with certainty. Thus any body which being a member of a mechanical system is at any moment of time  $t_1$  represented by the five coordinate quantities  $(\{x_1, y_1, z_1\}, v_1, w_1)$ , signifying respectively its position in three-dimensional space, its velocity or position in time, and its acceleration or mass, all relative to a fixed point  $O$  which is both origin and center of mass—will at any subsequent moment  $t_2$  have changed to a state which can be represented by  $(\{x_2, y_2, z_2\}, v_2, w_2)$ , where  $(\{x_2, y_2, z_2\}, v_2, w_2)$  is a determinable function of  $(\{x_1, y_1, z_1\}, v_1, w_1)$ , and as before said, we can regard the body in the first state as the *cause* of the body in the second state. For brevity let us call these two complex quantities  $A_1$  and  $A_2$ . Now we know for various reasons (among others the fact of the loss of energy necessary in every system) that no change is wholly cyclic, *i. e.*, the state of a material system never repeats itself in time. With respect to  $O$ ,  $A_1$  and  $A_2$  or  $A_n$  and  $A_{n+m}$  must differ, and what is more important, they must differ positively or in one direction only—for if they did not the history of a system would repeat itself and we should have a cyclic change, which is impossible.  $A_{n+m}$  can never be equal to  $A_n$ , and consequently  $A_n, A_{n+1}, A_{n+2} \dots A_{n+m}$  must be quantities which stand in an irreversible series—a series such that any member  $A_r$  must be ‘further’ from  $A_{r-2}$  than from  $A_{r-1}$ . This character is, however, precisely the character of our own numerical series, and we may, therefore, say that the series in question is not only an irreversible but an increasing series. But it must be remembered that all this is only true with respect to our own chosen origin  $O$ . We can always select another point in space  $O'$  as origin and center of mass, with respect to which the series  $A_1, A_2, A_3$ , could be read in the reverse direction, where, for example,  $A_2$  with respect to  $O$  could be shown to be less than  $A_1$  with respect to  $O'$ . Hence it is only on the supposition that  $O$  is a fixed point that we can show that mechanical change is irreversible or absolute, and not reversible or relative. What does this possibility of selecting another origin mean? It means simply that no given system is absolute, but only relative, and that consequently the changes in a system can only be predicted in so far as that system

is taken as absolute, *i. e.*, as either isolated from or inclusive of the rest of the universe. This is precisely the assumption upon which Science proceeds. In a small system of bodies the error due to the interference of other systems is great. As the system is enlarged the changes become more regular, and the error decreases, owing to the diminishing interference of other systems until it finally can be neglected. Could we grasp the universe as a whole, we should then have a system in which every element necessarily changed in one direction, and could consequently be expressed as an increasing quantity or series of quantities with respect to an absolute center of Mass or Origin.

We have now reviewed the various types of causality in the order of descent. Starting from the mixed stage of psychical causation, we traversed the fields of biology, chemistry and physics, gradually eliminating the qualitative or teleological element until we finally reached the perfect or limiting form of mechanical causality in which the cause became the antecedent and the effect the consequent; the sole relation between the two being the relation of a less quantity  $A_1$  to a greater quantity  $A_2$ . Pure quantitative increase is then the limit or substantial form of relation in the mechanical world or world of Facts. Let us now endeavor to find the limiting form of teleological causality, the relation between Meanings.

Returning to our point of departure, the case of mixed causality or psychical association, we may remember that the process in which the psychical state 'Jacques' called up the psychical state 'Hamlet' (which we expressed in the judgment, 'Jacques resembles Hamlet') was grounded or explicable on a dual relation between the antecedent subject and the consequent predicate. These two types of causal relation are named by psychology Association by Similarity and Association by Contiguity, the former being a case of teleological or final causality; the latter, a case of mechanical or efficient causality. In order to find out just what mechanical causality meant, we were obliged to work downward through the various conditions which governed this particular sequence, regarded on its factual or particular side, until in the course of our process all those teleological elements which permeate the factual order were one



after another eliminated. We have then now to follow the exactly opposite course; instead of gradually eliminating the biological and chemical qualities which taint the space-time world of the factual order with a teleological meaning—a normative significance—which is logically foreign to it, we must now proceed to eliminate the hypothetical and assertorial particularity which taints the non-temporal world of teleological norms with an irrational and mechanical character which detracts from its purity. Indeed this is one of the most noteworthy and remarkable characteristics of the universe—that although the ‘world of norms’ is diametrically opposed to the ‘world of facts,’ yet each of these diametrically opposed worlds is tainted and permeated with the characteristics of the other. Biology and chemistry are certainly *factual*, as distinguished from *normative*, sciences, and yet we have seen to what extent they imply the qualitative or teleological element. Just so logic and ethics are distinctively normative sciences, nevertheless they imply as we shall see all sorts of factual considerations. And it is this fact of separation without purity which makes the method of limits the only proper instrument for attaining a comprehension on the one hand of what an Idea or norm really is, and the nature of its relation to other Ideas or norms; and, on the other hand, of what a mechanical or material fact is and its relation to other facts. The Judgment ‘Hamlet resembles Jacques’ is not a purely normative judgment, that is to say, it does not adequately represent that absolute relation between two meanings which we call Truth. For in the first place there is no such person as Jacques and no such person as Hamlet. They live in a world which does not truly exist, and it is only by my assumption of this make-believe world as a real world that my judgment is true. In short my meaning would be more truly expressed if I said, “If Jacques were real he would resemble Hamlet.” This judgment is then a judgment of possibility, and as such its truth is imperfect. We call this kind of imperfect judgment by the name ‘Hypothetical,’ which means that the relation is not grounded in reality but on a hypothesis. But you will at once reply—Hamlet and Jacques are not merely hypothetical personages, mere empty possibles, a mere possible

is nothing, and obviously Jacques is not nothing but a very important being whom the world could ill dispense with. In fine, Jacques has some sort of actuality as a state of my consciousness and of many consciousnesses, of which Shakespeare's is one. As such then it is more than a hypothetical relation in which he stands to Hamlet. Taking Hamlet and Jacques as states of consciousness it is *really* and not hypothetically true that they resemble one another. But what is the ground of this relation? Is it a teleologically necessary connection, or does it just happen to be what it is? We can conceive Shakespeare to have made a Jacques who would be merry instead of melancholy, and who consequently would not have resembled Hamlet. From this consideration we derive the important conclusion that the teleological similarity of Jacques to Hamlet is itself dependent on certain unteleological brute facts in the temporal world in which Shakespeare lived. Our grounds then for making the judgment are to some extent at least purely factual, and we express this imperfection by saying that the judgment is 'assertorial.' But just as we saw above, that it was unfair to regard the judgment as merely hypothetical for the reason that Hamlet and Jacques were something more than purely possible beings, so here also we must admit that it would be unfair to call the judgment merely assertorial. Hamlet and Jacques are related not merely as facts, but also to some extent as necessary facts. Given the fact that both conceptions involve the character of melancholy, it is rationally or teleologically necessary to admit that they resemble one another. In short I must acknowledge that these semi-hypothetical facts, being what they are, bear a certain relation to each other which I, as a rational being, cannot disregard nor look upon with the indifference with which I have a perfect right to look upon pure matter of fact. A pure fact might be other than it is, but the relation between these particular facts when once they are accepted could not be conceived to be other than rational and necessary. This character of *necessity* makes the judgment in which it is present an 'apodictic' judgment. These three degrees of modality are always to be found in teleological sequences, *i. e.*, sequences whose terms are related in virtue of their common participation in

some third quality. And now that we have shown in a somewhat labored fashion that our chosen example of the mental sequence in which Jacques calls up Hamlet not only contains biological, chemical and physical causality on the mechanical side, but also hypothetical, assertorial and apodictic causality on the teleological side, it will be necessary to study these types of teleological causality in isolation in order to discover the limit approached by the teleological relation as it is gradually freed from the unteleological factors which usually accompany it with the result of obscuring its true nature.

Absolutely hypothetical judgments or judgments about purely possible entities would possess no psychical cogency, would exercise no constraint over the mind of the thinker. An example of such a judgment would be the assertion—"If there is a jaberwock, he would eat a griffin." This judgment exercises absolutely no control over the mind, for two reasons: first, because neither jaberwocks nor griffins exist; and, secondly, because there is no *réason* for connecting even the bare idea of jaberwock with the idea of griffin-eating. Now take an example of a judgment of the next higher degree of modality. "If there's a mermaid, she lives in the sea." This resembles the preceding judgment in so far as its subject is unreal, and again in so far as there is no rational connection between subject and predicate—no intrinsic reason for a mermaid's not living in a pond; it differs from the previous judgment, however, in so far as in the quasi-real world of fairyland it has actually been learned from experience that mermaids live in the sea and nowhere else. What is actual for a hypothetical world is hypothetical in a real world. We are actually limited by this judgment, *i. e.*, forced to recognize its cogency whenever we choose to talk about fairyland.

These two types of judgment exhaust the realm of the hypothetical, and we have now to enter on that of the actual or assertorial judgments. And just as we found two degrees of hypothetical judgment dealing respectively with unreal connections, and with real connections in an unreal world, so now we shall find two kinds of assertorial judgments—(1) non-rational or contingent relations between real facts, and (2) necessary



relations between unreal facts. The non-rational between real facts is exemplified in such judgments as "The grass is green," "The rain is falling," "The match is two inches long," "To-day is Tuesday, not Wednesday." These judgments are true, and there is no discoverable teleological ground for their truth. They differ from the hypothetical judgments in that they have absolute cogency over the mind for the moment in which their terms are perceived. They are necessary not for the understanding, but for the sensibility. (The hypothetical judgments only had cogency over the mind on the condition of the mind's voluntarily accepting the hypothetical world.)

The other type of assertorial judgment is not a judgment of fact, but a judgment about determined relations between unreal entities. For example: If a mermaid is defined as a being which if it lived would live in the sea,<sup>5</sup> then the judgment that a mermaid would be able to endure salt-water would have assertorial validity, *i. e.*, cogency over the mind of the thinker as a brute fact in the real world. Or, again, whether any three things *A*, *B* and *C* are real or not, it is nevertheless a fact that if  $A = B$  and if  $C = B$  then  $A = C$ , and the validity of this conclusion is absolutely independent of the existence or non-existence of *A*, *B* and *C*. It has, however, assertorial validity and nothing more. The thinker would only have to recall the fact that *A* and *B* were creatures of his fancy and all sense of necessity would cease—his judgment would resolve itself into the mere recognition of his consciousness as having a certain form. He finds this character of unity in his consciousness and recognizes its existence in this particular case, just as he recognizes that the grass is green or that to-day is not yesterday. In short, the final test of the degree of modality possessed by any judgment is the amount of the force which it exerts over our actions. The fact that an unreal thing is identical with itself is equal to no more than a simple recognition of an actual relation, whose terms being unreal is itself nothing more than a fact, and as such only determines our actions in so far as they concern themselves with it.

If we now pass to the third and final type of teleological sequence we shall be able to see better the justness of this serial

arrangement of the first and second degrees of modality. We come to the world of necessity in which judgments are neither hypothetical nor assertorial but apodictic. If it be true that possibility, actuality and necessity really stand in a serial order, we ought to be able to predict the nature of apodictic validity from a consideration of hypothetical and assertorial validity. We found that hypothetical judgments could be of two kinds. We could assert a possible relation between possible facts—"a jaberwock would eat a griffin" or "people in Mars have three arms" (and these judgments, exerting absolutely no cogency, could fitly be called 'problematic' rather than hypothetical); or again, instead of asserting a possible relation between possible facts, we could assert an actual relation between possible facts, *i. e.*, "Mermaids live in the sea." The mermaids are to be sure unreal, but the relation between the concept 'mermaid' and the concept 'sea-dwelling' is real though contingent when taken merely as a relation. The hypothetical judgment or the judgment about possibilities concerns itself with relations and not facts. The assertorial judgment or the judgment about actualities concerns itself with facts apart from relations. The fact was either simple, as in the judgment of the grass being green, or complex, as when formed by the actual coexistence or intersection in one consciousness of two purely hypothetical judgments or assertions of mere relation. If the combination of two possibles makes an actual we should expect that the combination of a possible and an actual, a relation and a fact, would yield a necessary. Let us see if this really happens. A triangle is an actual fact and the relation between the sum of the angles of a triangle and two right angles is a genuine relation, *i. e.*, a relation which is, if not genuinely intelligible, yet more than a mere fact of perception. Hence the judgment, "the sum of the angles of a triangle is equal to two right angles," is a judgment which combines a relation or rule of reason with an actual thing or fact of sense experience, and as such possesses apodictic validity. Now the question arises as to whether there are degrees of necessity. Would there be any meaning in saying that one apodictic judgment was more necessary than another? We have said that the degree of

validity of any judgment was measured by the degree of cogency which it exerted over the mind of the thinker. Hence if we find that several apodictic judgments differ in their cogency, we can admit that there are degrees of necessity. Let us examine the following three judgments: (1) A straight line is the shortest distance between two points; (2)  $7 + 5 = 12$ ; (3) Every event is identical with itself. All these judgments are apodictic, but they possess different degrees of importance. We cannot, indeed, deny the axioms of geometry, but can we not to a large extent neglect them? In so far as our experience is spatial it is dependent upon the laws of space; but a large part of our experience is not spatial and is to that extent independent of the axiom of the straight line. It only possesses cogency over part of the mind. Now the second judgment, like all judgments about particular numbers, involves in itself the whole number series; and as number applies to inner experience as well as to outer, we have in the numerical judgment a greater because a more extensive cogency than in the judgment about the straight line. Finally in the third judgment, the axiom of identity,  $A = A$ , we have a cogency and consequently a validity more nearly universal than that of either spatial or numerical judgments. For if there is a large part of experience that is independent of the laws of space and a smaller surplus of experience which is independent of number, there would at first glance seem to be no experience at all that is not dependent upon the law of identity. We must then admit that there are degrees of necessity, that apodictic validity may vary in its extent. Hence it will not be enough to say that any apodictic judgment can be regarded as the limit of the series of teleological sequences. To find the limit we must find the judgment which is cogent throughout all experience and not merely throughout particular departments. Perhaps the axiom of identity fulfills this demand for an absolute or universal necessity. To prove this we have only to show that there is no case in which a thing changes its identity. But does not the very statement of the task bring out the impossibility of its attainment? Wherever there is change the law of identity is neglected.  $A$  does not remain identical in so far as it gives rise to  $B$ . One



thing becomes another thing. We need not take the fact of change as a violation of the law of identity for there must always remain a constant or identical element throughout every change. We have, however, the right to regard change as an example of the limitation, if not the contradiction, of the axiom that  $A = A$ . Change may imply identity; but identity is not the whole essence of change, and whenever we attend to the changing forms and states of a thing rather than to the thing itself, we are in just so far attending to a phase of experience over which the judgment of identity is not cogent.

Professor Royce in his *Religious Aspect of Philosophy* gives an example of a judgment which would seem to possess the sort of universal validity for which we have just sought in vain in the judgment of identity. The judgment that 'every doubt implies a reference to an objective truth' is absolutely universal throughout the realm of reason. To deny or to doubt the truth of this judgment is to affirm it, because any rational doubt of the truth of a proposition is based upon the belief that it does not conform to the established nature of things nor to objective Truth, and the existence of this objective Truth is all that the judgment asserted. Before submitting this proposition of Professor Royce's to a final test of the universality of its cogency, we should recall the manner in which the limitations of the previous judgments were manifested. When confronted with the axioms of geometry we could say: There is a portion of our experience over which these laws have no sway. Again in the case of the axioms of arithmetic we could point to the concrete differences in the qualities of experience as being outside the laws of number. Finally in the third judgment we could instance the phenomena of time and change as examples of experiences, the whole nature of which could not be expressed by the principle of identity. We have then, if we would show Professor Royce's refutation of scepticism to be limited in its degree of validity, simply to follow the same path as before, *i. e.*, to find some experience over which the law in question does not hold. The topic of the judgment is itself an indication of its limitation. What are truth and error? They are objects of Reason—the law which binds them together is a law

of Reason. Is there any portion of experience that is not purely rational? Feeling and acting may be reasonable but are they not something more? Every sensation and every act of will contains an element or an aspect which is not reducible to the laws which govern our thought. And in so far as we have experience which is not merely rational, just in so far is our experience independent of Professor Royce's proposition about the necessary implication of a rational doubt. For a non-rational being and for any being in so far as he is non-rational, the judgment in question possesses no cogency nor validity.<sup>1</sup>

The question naturally arises here as to whether there *is* any single judgment the consequences of which we cannot escape by changing or extending our point of view. We may remember that when we arrived in the series of mechanical sequences at what seemed to be a purely quantitative and irreversible change, we found that it was possible by changing our origin or center of mass to view the sequence in reverse order. The only ways in which it was possible to transcend this reversibility or relativity of mechanical processes was by extending the material system until it embraced the entire universe for which there is only one center of mass, or by selecting a system (*e. g.*, an organism) which possessed a *unique* center of mass which could not be exchanged for any other. The case is precisely the same in the present series of teleological sequences. All rational sequences, *i. e.*, all apodictic judgments, seem to be permeated with relativity—there is always some other point of view, as it were, some other center of mass, with respect to which our sequence loses its validity. I can think of only one type of judgment from the consequences of which it is impossible to escape. In the moral judgment or judgment of duty there seems to me no relativity whatever. The judgment that  $A = A$  does not forbid us to neglect it in so far as experience is temporal or

<sup>1</sup>If this reasoning be valid, there follows what seems to me to be a rather important result, *viz.*, this: In so far as the arguments used by Professor Royce and Mr. Bradley for the demonstration of the Absolute as a being in whom evil and pain are transcended—depend upon the conclusion that error is transcended—they are baseless. That is to say, there is no inconsistency in regarding God as necessarily rational but by no means either good or happy. The divine transcendence of evil and pain does not follow from the mere transcendence of error.

subject to change. The judgment that every rational doubt implies an absolute truth does not prevent us from neglecting altogether the world of the rational in favor of the world of sense. But the moral judgment that something ought to be not only forbids us to deny it, but it also forbids us to neglect it for anything else. When we stand in the presence of a duty, the moral law does not simply assert its validity or cogency for a single department of experience, it asserts its own apodictic truth and as it were in the same breath it claims our attention and our absolute and peremptory obedience to it. This is the peculiarity of the 'Categorical Imperative,' that in addition to the apodictic validity which it possesses in common with all necessary judgments, it puts in an extra claim to be recognized as more important than anything else. It forbids us to deny its truth, *and it also forbids us to neglect it*. Its cogency is not merely negative but positive, it commands us to vindicate our recognition of its absoluteness by determining our actions in accordance with its maxims. It appeals to us not in so far as we are subject to the laws of space and number, not in so far as we are subject to the Laws of Identity and contradiction, not even in so far as we are rational or sensuous, or social or virtuous. It condescends not at all to justify itself by pointing to any one mode or aspect of our nature. It bids us unconditionally to recognize it and to follow it with all the strength that we have. These attributes of the moral law may be very edifying or they may be the reverse; our interest in the Categorical Imperative is not in the least emotional, we simply cite it as a very peculiar phenomenon and a very significant one for students of epistemology and logic. Its significance is due to the fact that it fulfills the ideal of absolute validity or truth. It is the limit of the series of judgments in which each possessed a greater degree of truth than the one before.

From the problematic judgment which exerted no cogency whatever, and the hypothetical judgment which was valid only if we voluntarily admitted the condition upon which it was based, on through the assertorial judgments whose validity was limited to the moment of perception, up to the apodictic judgment which was absolute through some one department of ex-



perience and finally to the judgment of Practical Reason, which possessed an absolute and unconditional cogency over the entire self—through all this process we have noted the genuinely serial order. Each term of the series, each type of judgment possesses all that the previous terms possessed. And at the end of the process we reach the limiting term, the type of judgment which declares itself as absolute. The moral judgment alone, as the limit of the series, contains in itself the perfect validity to which the speculative judgments could only approximate.

We hear a great deal at present about the judgment of worth as superior to the judgment of truth. Practical Reason is dogmatically asserted to be more real than speculative Reason, and the return to the epistemological dualism of Kant is advocated as 'the only refuge for Theology in its flight from the persecutions of science.' The object of this study in the method of limits is simply an attempt at some sort of justification of Kant's hypothesis of the supremacy of Practical Reason. If one is content with simply asserting that moral truths are superior to the truth of reason, he is helpless against anybody who makes the opposite assertion. If Speculative and Practical Reason are wholly incommensurate the one with the other, there is no criterion for deciding as to the supremacy of either. To assert one as prior to the other is a senseless and arbitrary act. But if we can show that the judgments of the one faculty form a continuous series with the judgments of the other, we vindicate our right to a division into higher and lower. In the light of our analysis it is no longer a paradox to assert the existence of duty as truer than truth. For as we have seen the degree of truth possessed by a judgment is measured by the degree of cogency which it exerts over the mind. The judgment of duty is absolute or unconditional and as such has more cogency than any other possible judgment, hence it possesses a maximum of validity or truth.

Now that we have found the limit of the series of teleological sequences it is necessary to analyze it in order that by observing its inner nature we may be able to see if it is at all commensurate with the limiting type of mechanical sequence. To simplify this analysis I must ask you to assume, without proof,

that the moral law only, or at all events primarily, appears in its own form under certain definite conditions. We use the word 'ought' in a variety of cases in which moral obligation is not felt at all, or if felt, it is only indirectly and by analogy. The three conditions under which the moral law makes its appeal are :

1. The recognition of a good.
2. The immediate possibility of realizing the good.
3. The fact that I and not someone else am the agent for its realization.

We make use of the word ought when any one of these conditions is present ; but we only use it in its true and proper, *i. e.*, in its moral sense when all three conditions are fulfilled.

Omit the first condition, and we have the class of what Kant called Hypothetical Imperatives. "I ought to come in out of the rain"—there is no moral obligation contained in this judgment unless I make the further judgment that it is morally good to keep dry.

Again, omit the second condition, *i. e.*, the possibility of action, and we get that class of judgments which express regret or remorse or distress without obligation to act. "My past actions ought to be different." "I ought not to be in this condition." In these judgments I see the impossibility of realizing the good by any action, and with this recognition the obligatory cogency of the judgment of duty is destroyed. And, thirdly, when I say you ought or he ought to do so and so, I fail to perceive the peculiar strain or compelling force of the Moral Imperative. It is only when I realize that there is, first a *good*, which is, second, immediately *possible*, third, for *me to realize*—it is only then, I say, that the judgment of Practical Reason sets up its claim for absolute and peremptory obedience. And I think that if one takes the trouble to analyze his feelings when he fancies that moral obligation is present independently of the presence of all these three conditions, he will find that either on the one hand the apparent obligation will pass away, or on the other hand, he will insist on projecting himself into a world in which the duty in question is possible and possible for

him alone.<sup>1</sup> Granting this we may state the judgment of duty as a judgment in which a possible good is connected with its realization in the world of fact—by means of the *ego*. The cause or antecedent term of the judgment-sequence is an idea; the effect or consequent term is the idea realized or actualized.

We must here note a further peculiarity of the moral judgment. It is self-transcendent in the sense that it refers to something beyond itself, *i. e.*, to its realization in the world of fact. The judgment, "I ought to do this," is only fulfilled or completed in the process of realizing the judgment in action. To recognize or think a duty as binding is only the most rational of judgments when it is accompanied by the specific realization of the good. The moral judgment is truer than the speculative judgment, but the acting out of the moral judgment is the only way to complete or exemplify this truth. Hence this final type of teleological sequence is the change from an idea of a good deed to a good deed. It is the change from the possible to the actual. *A* is the subject of the judgment, *A'* is the predicate. *A* is the idea of the good, *A'* is the realized fact. *A* is the antecedent or causal term of the sequence, *A'* is the consequent term or the effect. *A* as a possible is the cause of *A'* as an actual. To find the essence of the sequence we have simply to find the difference between the possible and the actual, the idea of the fact and the fact itself. The effect only differs from the cause in possessing existence. What then is the predicate of existence? As Kant expressed it, How does the actual dollar which will pay a debt differ from the idea of the dollar—which will not pay a debt? That there is a difference between a fact and the idea of a fact we cannot doubt. A man is thoroughly good when he turns his ideas of good into facts; a man is thoroughly bad when he possesses the idea of the good, the knowledge of his duties, but refuses to realize that knowledge. The antithesis between the idea and the fact is of the same kind and of the same degree as the antithesis between conscious sin and

<sup>1</sup>This is what happens in the case of remorse: we either recognize the uselessness of regretting what is necessarily the case and the remorse vanishes, or we persist in projecting ourself into the past circumstance in which the regretted action was not a necessary but a freely chosen possibility.



conscious virtue. The existential predicate is something real, but it is not a quality. We cannot point out any quality which the actual dollar possesses that is not also possessed by the possible dollar. How do we detect the presence of this predicate of existence if it is not visible as a quality? We detect it by its effects, it manifests its reality and its nature in its functions in its relations to other things. The real dollar is known to be different from the ideal dollar because the two stand in different relations to a debt. The one produces certain effects which the other does not, and by the principle of sufficient reason we are bound to explain the visible difference in the effects by positing a genuinely real though invisible and non-qualitative difference in the respective causes. There have been suggested various criteria for distinguishing the external world from the sensations about it.

There is Humes' criterion, based upon the difference of vividness between the real and ideal. This criterion is imperfect in so far as it affords no basis for condemning as unreal or merely subjective the remarkably vivid hallucinations of an insane man. We are driven to adopt a second criterion, the opinions of our fellow-men. What our fellows pronounce actual, that is really actual; and what they say is mere idea or possibility is really so, no matter how vivid it may seem to us. There is to-day an increasing body of thinkers who stop here and accept the verdict of the 'social consciousness' as the final and all-sufficient criterion for distinguishing the real from the ideal. What constitutes lunacy?—simply having an experience which your neighbors do not have. If there were ninety-nine men who possessed in common a certain hallucination, and one man who did not possess it, then we are told that that one man would be insane so far as that community was concerned. It is doubtful if any one could ever seriously believe in this theory, if its necessary implications were clearly seen, and yet like all forms of Berkeley's doctrine of the identity of *esse* and *percipi*, it is a pleasant paradox and one that is easy to defend.

For in the first place, this appeal to the verdict of the Social Consciousness as the ultimate test of what is real, involves an endless and vicious regress. "A thing is real because my

neighbors say it is real." Why do my neighbors say it is real; what is the rational ground for their assertion? According to this theory their only ground must lie in the fact that their neighbors assert the experience as real, the opinions of the latter being in turn justified only by the beliefs of *their* neighbors, and so on until finally we should exhaust the number of men and arrive at the individual who acted as bell-wether to the human flock. Whatever he said was real, that also everyone else would pronounce real. But what would be *his* ground for distinguishing between the real and the ideal? Not the rational (?) ground of 'imitation' because in this first case there would be no one to imitate. We must either admit that in the last analysis the distinction of subjective and objective rests upon pure caprice, or else we must seek a criterion beyond the 'Social Consciousness.' Other men happen not to dream your dreams, but that is not the reason that your dreams lack an existential predicate!<sup>1</sup>

There is, however, a third criterion for distinguishing the ideal from the real, viz., the criterion of permanency. When the patient suffering from an illusion refuses to believe his neighbors, he is when possible taken to the apparent cause of his illusion and allowed to test it with other senses than the one affected, upon which the illusion usually vanishes. The part of it which is unaltered by changes in time and in sense remains for him as real. Hence, in general, we may take the common sense view that the *unalterability* of a thing is the final test of its reality.

Now what is the meaning of this word 'Unalterability'? It means what cannot be altered. A man tries to alter an experience and can't succeed. Why? Because the thing resists him; he tries to change it and fails—then he feels safe in pronouncing it real or objective. A baby feels an uneasy sensation

<sup>1</sup>There is nothing mysterious in the seeming plausibility of the Social Consciousness theory, for this plausibility is due to a very simple *hysteron proteron*. A real thing usually shows its reality by being an object for a plurality of subjects, in much the same way and for much the same reason that to a real body in space a number of lines can be drawn. Stand this truth on its head and we get the idealistic doctrine that *because* a body has relations it is real. Relations are at most the *ratio cognoscendi*, but never the *ratio essendi* of existence.

in its finger—finds he can stop it. He also sees the moon and wants it and can't get it. What is the obvious and legitimate induction for the baby to make? Finger sensations, thoughts and gurglings are one kind of thing; moons and cribs and other people's voices are a different sort of thing. The first class belong to me, they are subjective the other class do not belong to me, they are objective. Why even such a very subjective thing as a pain will if it resist long enough be objectified. The first day of a hard toothache the sufferer speaks of 'my toothache'; the third day he speaks of 'that infernal pain,' as though it did not belong to him at all.

A thing which resists our will is actual, a thing which does not is possible. We identify that which resists our will with the realm of external experience, because a purely internal thing, like a train of thought, is wholly identified with and amenable to our wishes. The feeling of resistance or of *continuity with something not ourself* is at once the ground of our belief in objective experience as being caused and in subjective experience as being free. There is no sensation so purely possible as not to contain a slight degree of stubbornness or resistance, nor is there, on the other hand, any sensation so intensely actual as not to be in some degree changeable by our will. The property of resistance is then a relative or quantitative affair. It is recognized as akin to our own effort-feeling because it varies continuously and directly with our effort. As in our feeling of effort we get the intuition of pure quantity freed even from the semi-qualitative attribute of extension, so in our intuition of the objective correlate of effort we are likewise compelled to think of a purely quantitative entity. The moral change from the purely possible or practically unresisting *idea* of the good to the actual or practically unalterable good deed is not only the most thoroughly rational and teleological of sequences, but it is also a change from one quantity to a quantity infinitely greater. Existence is manifested in resistance and resistance is a matter of quantity, hence existence is also a matter of quantity. The change from a possible dollar or a possible good deed to an actual dollar or an actual good deed, is a change which involves the addition of an existential predicate, *i. e.*, the addition of an



infinite quantity. We cannot change the possible dollar into the actual dollar, but we can change the possible good into the actual good. But you may answer, Surely the difference between the idea of the good and the fact of good is too fundamental to be explained by a mere increase of quantity. To this I answer by offering an analogy. If we decrease a surface infinitely we reach a line. Now the difference between the line and the plane is of the same fundamental nature as the difference between fact and idea, and yet the conception of an infinite quantitative increase is all that is required to explain the one case. May it not then explain the other also?

The limit approached by the first or mechanical series was the change from a less to a greater quantity, the amount of change being finite. The limit approached by the second series, *i. e.*, the series of teleological judgments is also a change from a less quantity to a greater, though in a perfect moral act the change would be infinite. *The two attributes of the soul are in their essential or limiting forms homogeneous with each other and imply a common substance.*

But the most cursory examination of the act of duty reveals another and equally important characteristic, *viz.*, its perfectly material physical causality. Our actions are quantitative or mechanical in so far as they follow from the intensity of desire, rather than from the quality of the desire. The moral law is essentially and peculiarly material or physical, in that it bids us seek the greatest possible quantum of Good, the maximum of desirability quite regardless of the quality of the object. This is why the good manifests itself in such a variety of objects, though never completely or adequately in any one. Pleasure *qua* pleasant or Beauty *qua* beautiful can never be moral ends. Only what is most desirable and *because it is most* desirable can be recognized as an object of duty.

The moral action then has as much to do with the sensuous and physical as with the rational and teleological, and the realization of an act of virtue manifests its quantitative and spatio-temporal nature in the feeling of effort, to the same extent and at the same time that it exhibits its non-temporal and universal or ideal validity. This double aspect of moral phe-

nomena is evidenced in the use of the two terms 'right' and 'good.' Every moral act is right or rational and at the same time 'good' or 'desirable.' The ethical rationalists or rigorists attempt to restrict ethics to a study of the law of right, the Categorical Imperative. The opposite school of ethical writers—who are in general Hedonists—regard ethics simply as a study of the *summum bonum*. The former claim that the Good or *end* of conduct is deducible from and secondary to the Right, or *law* of conduct. The latter claim that the Right or categorical imperative is deducible from and secondary to the *summum bonum*. The end must justify means is their watchword, while the watchword of the Rationalists is "Let Justice prevail though the Heavens fall." The means must justify the end. The Hedonists forget that the limiting or perfect type of the Desirable must somehow imply the existence of a perfectly right or rational means by which it is to be attained, the Highest Good must be compatible with Right action. The Rigorists on their side forget that the limiting or perfect form of Right must be something more than a merely rational or formal law, and must lead towards the maximum Desirable. To find a single principle of moral action which should in its own simple nature express and harmonize these two opposite *motifs* of our moral nature, upon which the two methods of ethics are based, would constitute the solution of the moral antinomy, the 'masterknot.'

For our purposes it is sufficient to recognize the significance of the moral act as the unique embodiment of a perfectly pure or limiting type of efficient causality, and a perfectly pure or limiting type of teleological causality. When Kant pointed out the Practical Reason as the only clue to the nature of Reality, he discovered a veritable mine of metaphysical wealth,<sup>1</sup> none the less rich from the fact of its all but universal neglect at the hands of his disciples.

From the nature of the moral act as containing in its own

<sup>1</sup> Not least among the many curious and beautiful phenomena of the moral consciousness is the fact which we have foreborne to mention that although the moral act is the only example of perfect efficient causality and of perfect final causality, it is also the only act whose causality is genuinely indeterminate or free. The extent to which a man realizes any given ideal of good is a wholly independent variable governed by nothing in the past or present except itself.

simple and irreducible nature, both efficient and final causality, we can and must infer the nature of the moral agent as a being or substance, which is at once as particular and as material as the *atom* of the physicist, and as universal and intelligible as the *concept* of the logician.

Such then is the nature of the soul. We may call it a 'substance' because it fulfills the three conditions mentioned above as the requisites for a valid concept of substance. It differs from a mere Ding an Sich in that it is known to exist under a form of its own, viz., the moral form; and it stands to its attributes in that peculiar double relation (due to its nature as *limiting type or essence*) whereby it is at once the common genus of both and a distinct species of each. That the soul exists as a substance distinct from matter and mind, yet commensurate with each therein, furnishing the only possible explanation of their interaction, we have seen. We have seen, too, that it is a simple and not a composite substance, for the act of duty in which the soul manifests itself under its own form is a simple act, and not a mere complex of elements. May we ask, in conclusion, as to the destiny of the soul? Does the soul enjoy a genuine immortality, *i. e.*, an individual continuance of consciousness under conditions to some extent analogous to those of time and space? The outlook is extremely sobering and dark. The evidence, when candidly scrutinized, seems to reduce itself to a few ghost stories and a mighty yearning. What indeed does the universe want of an individual's consciousness after death? The world of the physicist certainly does not need it. The dead body in its mere decomposition fulfills satisfactorily all the laws of conservation of matter, motion and energy. Not only is there no need for a 'loose consciousness,' but if there were anything left beside the dead body the symmetry and unity of the physicist's world would seem to be threatened. Nor is the case different with the world as viewed by the transcendental philosopher. An individual consciousness is as far from harmonizing with the Platonic Ideas or pure forms of the panlogist as with the atoms and energy of the materialist. For the individual consciousness, just so long and just in so far as it is individual is permeated with a particularity and contingency



which absolutely defies and sets at naught the attempt to define it in terms of universals. In short, consciousness is in very much the same position as the classical bat, there is no place for it in the empire of the earth, neither in the empire of the air, and condemned to flit helplessly between the two realms, it will ever be as a thorn in the side of the consistent empiricist and materialist and the consistent rationalist and idealist. For what indeed is the individual consciousness but the hybrid product of the union of 'matter' and 'meaning,' of 'facts' and 'values,' of brain cells and 'pure forms'—a thing incommensurate with and wholly different in its nature and processes from the two orders of being with which it deals? Truly an 'epiphenomenon' with respect to either of the two factors from whose union it arises, what right can it possibly have to continue to exist when that union is annulled? And, indeed, modern philosophy when true to itself must answer the question as to a genuine immortality in the negative. We have, it is true, several substitutes for genuine immortality. The transcendentalist doctrine that the *ego* is a timeless fact, and hence not mortal, but possessing non-mortality of the same type as that possessed by the Pythagorean theorem or any other eternal verity. Again, we have the other type of panlogistic 'immortality'—that advocated by Dr. Paul Carus, according to whom, as I understand it, we may hold man immortal in so far as the form or meaning of his life is preserved in the memory of his successors and is influential in moulding history. And, finally, we have the materialist's 'immortality,' which assures us that our real self is the matter of our body and will continue forever.

It seems to me that if we regard the real man as consisting in the matter of his body or the sensational modifications of that matter, or in the timelessly valid ideas with which his intellect deals, or finally and most of all in the mere Hegelistic unity of these two sides of his nature, that we have no right to hope for genuine immortality. But in truth the real man, the *man himself*, is neither matter nor idea, nor both together; the real man is the 'something I know not (thoroughly) what,' which makes possible the extraordinary phenomenon of consciousness, *i. e.*, of the union of the two apparently

incommensurate orders of existence. Nature makes no leaps—there is no action at a distance; and it is simply unbelievable and unthinkable that a bundle of Platonic Ideas and a bundle of brain cells could on their own initiative and without any third thing or medium commune together in the violation of all laws of logic and of physics. And yet they do so communicate. All consciousness bears witness to the fact, and the moral consciousness testifies to the additional fact, that these two phases of being have their true reality, their essential nature in something which is more real than either, viz., the substantial soul. And when consciousness goes out and the universal truths and ideals which swayed the life of the living man return again to their own place, leaving the brain cells again free to follow the laws of inorganic matter—when that event takes place, something will remain, something more real and more precious than what has gone, something that being the condition of consciousness, and having under certain circumstances manifested itself in consciousness, may, under new circumstances, once more feel and think and act.

## DISCUSSION AND REPORTS.

### THE GROWTH OF VOLUNTARY CONTROL.

Some time ago a series of experiments was conducted by Professor Ladd connected with the voluntary control of the 'Eigenlicht.' Little attention was paid to the results, which were briefly embodied in an article published at the time in the *PSYCHOLOGICAL REVIEW*. This was somewhat surprising considering the importance of the general principles involved. There can be no doubt that the dominance of the physical explanation of phenomena has reacted to the detriment of our naïve faith in the all-powerfulness of the will. Mechanism has the floor just now. We should be entirely unwarranted, however, in drawing the conclusion that the will and its old-time spontaneity are 'for sale cheap.' The experiments conducted by Professor Ladd showed that the common conclusion is, to say the least, hasty. For the averages obtained, based on an extended series of experiments and conducted, under his direction, by some twenty special students of psychology, revealed the fact that voluntary control, though varying in degree in the particular function in question, as was to be expected, was nevertheless in every case more or less superior to the physical conditions which surrounded the experimenters. We are not, then (for I was one of the experimenters), wholly submerged in the meshes of mechanism.

I propose to trace, briefly, the growth of this fact of voluntary control, especially in connection with the function, illustrated in Professor Ladd's experiments, which the will serves as a mediating term between mechanism and so-called freedom. For I take it that the two statements, 'the will is limited' and 'the will is free,' cannot be reconciled except through the study of the evolution of the mind's progressive self-mastery. The real significance of the will, as an element of psychic life, is to be found, I think, in the way it is occupied in adjusting means and ends, mechanism to freedom. This aspect of volition has not, it seems to me, received the attention it deserves. To present some of the facts connected with this phase of mental life, taken from the psychology of volition, may serve the double purpose of calling attention again to the facts contended for by Professor Ladd



in the experiments above referred to; and also to suggest a new way of approaching the problem of freedom as a psychological factor of noetics and ethics. We may present the subject under the following heads: spontaneous control, or tact; immediate control, or conscious adjustment; teleological control, or self-control.

1. The subject of tact is one of the most mysterious in the whole range of psychology. Here we can only follow out the suggestions given by nature; for the key to the mystery lies in the organic and instinctive activities, the preëxistent factors of which may be taken as affording the clues to the various concrete types of spontaneous control. These are mainly three: (1) One kind of spontaneous control results from a peculiar facility of the will to isolate itself in the developing organism in certain directions to the neglect or indifference of others. We may call this the tact that isolates. Ultimately, this form of spontaneous control rests upon the relationship of the chemical and physiological elements of the vegetative life and the resultant differentiation of organ and function. The will, in some cases, and at some periods always, follows the index finger of nature, and the tact manifested, for example, in the control of the bodily functions, in the progress from infancy to youth, is a concrete illustration of this general fact. Where this species of tact is pronounced, the tendency to control by isolation is continued. The phenomena of genius, in all its forms, depend upon this fact of organic tactfulness for isolation; on this side of it, genius is merely the spontaneous ability to ignore certain directions of possible control for the sake of those which are more spontaneous. Isolation is the physiological condition of self-limitation and it is largely a matter of spontaneity, the will tactfully taking the line of least resistance as the 'rational' line of self-realization. In support of this, it is a fact, well vouched for by physiological students, that certain organs and functions develop more quickly than others and this fact has its corresponding feature in control. In abnormal cases, *e. g.*, abnormal and neurotic children, and children born of parents married late in life, it is frequently observed that the rudimentary organs of the mind, the head and brain in particular, attain to a quicker relative development and are thus isolated for spontaneous voluntary control for the rest of life. This fact, however, if associated with relative stability among the elements, leads to marked character and greatly increased facility of control. (2) The will spontaneously and instinctively controls the changes introduced by *growth* and *experience*. This is another species of tact, *viz.*, the tact for variation. The body, at first in absolute isolation from the world, always shows this tendency

when introduced into its larger environment. But it may also become a specialized form of activity, just as the tendency to isolation does; both isolation and variation are organic; but either may, under appropriate and opportune influences, become voluntary and automatic. In early life, we have doubtless noticed the tendency of the will to make departures in the matter of control. Abnormal and criminal children are cases when this tendency has run to excess. It rests upon the relative instability of the elements of organic life, as well as upon the failure of training. Tact for change, for variation, is a positive gift of the normal individual, however, as these abnormal cases show. The control is spontaneous in this case. No teaching or training seems to be necessary. The child suckles the breast without any previous education and this is a type of the tactful control of variation in all its phases. Further illustrations occur in the voluntary control of the *means* of conscious and teleological control. (3) Isolation and variation are conditions of natural selection; natural selection itself, however, is conditioned on the law of heredity. It is matter of general observation that many of the spontaneous acts of the will are hereditary, *i. e.*, reproduce the features of tact based on preëxistent determination: *e. g.*, the kind of spontaneous control shown by the children of musicians, artists, etc., resembles in kind, though not in degree, the peculiarities of their originals. For all species of tact, and therefore all kinds of spontaneous control, depend on conscious and teleological control, subject to the laws whereby acts are mechanized in habits and temperaments. The notion that the voluntary control itself is a matter of heredity is still unproved. Reflex movements are partially determined by heredity; but tact is more than reflex movement plus heredity; there is a residuum not contained in the chain of organic conditions which is the self-activity of the will itself. Thus, in certain of the arts and crafts, aptitude for the control of tools has become considerably facilitated by the operation of heredity laws. In the Middle Ages, the guilds of workmen exhibited this fact in a very concrete way: generations of the same families continued in the line of service marked out by their ancestry. So to-day in older countries, where the tendency to variation has not entirely overcome the other tendencies, a great part of domestic and industrial life rests upon the spontaneous control of hereditary instincts.

These three kinds of spontaneous control condition all other kinds of voluntary control. Tact constantly broadens as life unfolds; but the development of individuality and character is unfailingly faithful to the type discovered in the earliest spontaneous reactions of the will.

In other words, voluntary control, in its spontaneous form has a modicum of "freedom," and a maximum of "mechanism." The point we make is that freedom and mechanism could not be mediated in any case, not even in the form of tact, without the control of the will, at least in the assenting and instinctive manner peculiar to it. Tact itself is the result of contact, *i. e.*, of the intercourse and inter-play of spontaneity and mechanism. It is, in short, the will that gives to our earliest exertions at control the aspect of experience.

2. There is no marked line between spontaneous and conscious control; the one develops out of the other; the latter bears all the characters of the former. The new factor introduced is the influence of training; for as soon as we leave the phenomena of instinctive and tactful control, we see the necessity of the will 'to take a hand' in all its experience. Now this conscious exertion of will is the main characteristic of the mind of the child. Dim and inchoate are its ideas and but for a rough-and-ready equipment of bodily organs and functions, together with tact in the progressive control of them, everything has to be learned. But a great deal of this conscious control rests upon tact: *e. g.*, the formation of the various areas of reaction in the brain rests upon an organic adaptation in the organs concerned for their particular functions. Take the visual area. This is, mechanically, easily explained; but from the standpoint of voluntary control it is a very complex process, involving adjustment of eye balls, control of muscles and the unique fact of development in the visual area. The same is true of the other so-called 'ideal' sense, hearing. Mechanically organ and function are beautifully adapted; but the will, only after long processes, learns to control this source of perceptions and sensations. To a large extent, tact again explains the difference, *e. g.*, between the organic response of a child to sound stimulation and that of a trained musician. The large interpretative factor in the 'ear' of a Beethoven points to the relatively larger control of the sound impressions. The ear of both, other things being equal and presupposing a normal organ and auditory area, records equally well the stimuli; but only conscious and immediate voluntary control can explain the fact that Beethoven wrote his grandest music after losing control of the mechanism of hearing altogether. But the conscious and progressive control of our organs of sensation is, nevertheless, largely a matter of spontaneity, of tact, dependent upon the large amount of mechanical process involved. And facts go to show that our conscious efforts follow the features of tact already mentioned.

The control of the senses *in combination* is a matter of conscious



voluntary effort, *i. e.*, it is the result of immediate forth-putting of conation. Indeed, when sufficient strength has been obtained, the voluntary control becomes competent to inhibit and even suspend the activities of the bodily mechanism. It is now generally understood that the feelings and the will are intimately related, and that the feelings are closely allied to certain visceral and sympathetic nervous centers. These are under the control of the will, so that, in states of fear, anger, or remorse, or similar more or less complex affective states, the constitutional arrangements of the body may be interfered with. Functional activity, indeed, we regard as subject not only to immediate control, but to teleological also. We train children upon this assumption at any rate: that the spontaneous will to indulge these functions *must* give place to a higher and immediate control. Dirtiness is not only a matter of functional defect; it is a moral affair.

The control of intellection shows, in like manner, a growth of voluntary activity. Language is the greatest achievement of man and to a large extent language is a matter of tact in the form of the so-called imitative will. The conquest of vocabulary, beginning as it does in single words, and extending as intelligence extends, to verbs and relative parts of speech, is based upon experience of the self in its action on the environment. No emergence of intelligence is easy, still less necessary, when the motor centers are undeveloped. Activity again expresses the normal feature of the formation of speech. 'Willie *do* this,' 'Willie *do* that,' shows the mode of self-activity. Always 'do' something. The will must be appealed to and aroused. No word is truer of children and grown men than that they learn by doing. The *order* of volitional control in speech is first the noun. 'Willie' stands for certain associated images of actions; and this is based on previous experience with other concrete objects. Next verbs: 'do' implies the impulsive and imitative will; and this is securely founded upon experience with self in the past and instinctive tactfulness. 'This' and 'that' show the related yet the discriminated thought of purposive action. The complicated phenomena of speech are often amusingly illustrated in the voluntary control of the aspirate. The 'h' is a very active part of speech, and failure to control it is an evidence of failure in motor control, as connected with the imitative use of the will. This is a serious matter in self-consciousness; for it points to volitional instability in muscular and functional reaction. No one can doubt the volitional effort required to control this refractory member, involving breathing, muscular contraction, intellection and conscious and immediate will-power. Some children never control the aspirate. Some nations make havoc of it even.

The control of thought is another and higher step in voluntary activity. Through speech thought is coördinated and knowledge extended. Whether thought exists apart from speech cannot be determined from the introspective standpoint. What we know is that thought develops and comes under the control of the will as speech is mastered; thinking and speaking are the same things on different sides, speech being the volitional expression of thought. *Now no thought bears the aspect of reality which lacks will.* Even the comparatively passive process of sense-perception would be blind without the active presence of the laws of thought; for an object, thoroughly perceived, observed in all its elements, is a thought-object; even the infant's perceptions are, potentially, of this nature. Strictly speaking, of course, thought cannot be controlled, but only followed; but thought is not a matter of experience and knowledge without will, and in so far as the nature of thought becomes a matter for speech, it is entirely under voluntary control, *i. e.*, it is an adaptation of mechanism and spontaneity.

In both these processes, speech and thought, the tact of spontaneous control betrays itself. Generically, this fact may be expressed by the manifestation of peculiar combinations of instincts, feelings and motions in the back ground of thought so to speak. These subjective aspects of consciousness are, however, forms of willing and involve intellection. For we are never *merely* receptive. A feeling, even an organic impulse, is an active state, whether viewed from the aspect of pleasure or pain. The volitional control of feeling is therefore obviously possible, either through the spontaneous activities, or through intellection. There is no affective state which cannot be, to some extent, controlled through these channels. We may thus summon the intellect and emotions to support the will against any attack upon 'freedom.' It is true the intensity of the stimulus, say an intense feeling of pain, cannot be controlled by the will, *i. e.*, cannot be got rid of, or displaced by indifferent mental contents, since the relation of stimulus and reaction is permanent; but all our higher life, all teleological control, rests upon the assumption that the will is not bound by this mechanical relation. For the will's significance in mental life is just this: it is endowed with the function of standing between the mechanical relation of stimulus and reaction and the ends involved in consciousness as a progressive and self-conscious reality.

This voluntary control of intellection can be illustrated in the activity of attention. Attention is a complex operation involving both voluntary and involuntary elements. In a loose sense, primary intel-

lection is a species of spontaneous voluntary control based on reflex-activity; in the simplest act of attention, however, there is involved, something more than reflex-activity, though it is difficult to draw the line between what is mechanical and what is 'free.' The bridge over the chasm between mechanism and freedom is again to be found in the phenomena of progressive control. This control proceeds along two lines. Along one of these we observe a growing power of the will to subordinate the character, intensity, and duration of the sensuous content, upon which so much of what requires attention depends, to ends. This power, primarily, rests, as all else in voluntary control, upon the organic and affective life. We adjust ourselves easily to what greatly excites us. The clearness and degree of absorption in our attentive states, in other words, depend on motor control of the subconscious sort. What is called mind wandering is simply the inability of the will to control the motor side of our mental associations in an immediate and conscious manner: the will flows spontaneously along the stream of suggestion. But training, *i. e.*, the practice to which our organic powers are submitted in contact with suitable environments, soon gives the cure for this state. That cure consists, essentially, in introducing into the stream of sensuous and mental stimulation, the deeper principles of suggestibility involved in control of the teleological sort. Along another line, control is secured in attention through interest. A certain school of thinking to-day, following the isolated suggestions of thinkers as far back as Comenius, says that interest is the alpha and omega of attention. We think not. It is undoubtedly a strong influence: we easily attend to what interests us; but the 'prick' of *sensuous*, organic, excitement is just as strong. The strength of interest lies in the large amount of the self-referring activity involved. We are concerned when 'our interests' appeal to our wills. The ego, in the form of feeling, is what we mean by interest; but, for the reason that the ego *is* involved, interest expresses the complex unity of thoughts and volitions which go to make up the total man considered as a person. Interest is thus the total man 'bulging' in the curve of feeling; it is self in the intensest form, self-interest. As such it is almost entirely under the voluntary control of the will. Attention, at any rate, either as sensuously determined or as determined by self-interest, is a growth in which we discover a progressive adjustment of reflex-action to higher and higher modes of self-activity. The phenomena of religious experience, in conviction of sin, repentance and new life, show, as Höffding says, a relative failure of control; but he is entirely wrong in his analysis of humility.<sup>1</sup> The interests which

<sup>1</sup> Outlines of Psychology, p. 244.



we may deem at any time desirable as favoring our private ends are not so high and noble as those which may be contrary to our personal prospects. Egoistic influences, exclusively followed, do not make the demand on voluntary control that those of the more sympathetic type make. So far as attention is concerned, the voluntary control grows, in very few cases, normally, and the types of tact maintain themselves in their concreteness, just as the forms of feeling and temperament remain relatively permanent. The point we make is this: the attempt to resolve interest into exclusive states of feeling, purely self-regarding, and to maintain that it is interest, in this sense, that determines attention, is not a complete analysis of the phenomena of attention. Interest involves a certain instinctive and conscious exertion of the will and so far is not merely a state of feeling. That it also involves representation, in intellection and thought, is obvious when the object of attention is considered. It is nothing against the will that in any particular act of attention it adjusts itself to the prevailing forms of feeling at hand; this is no disgrace, no compromise of freedom; it is the type of all forms of voluntary control. At any rate, the whole of the religious and ethical life proceeds upon the assumption that the will is responsible and free, and what this can mean, if attention is absolutely conditioned on the affective experiences involved in interest, is more than can be understood, at any rate, by the writer.

Immediate and conscious control, then, is a belief which is supported by a considerable array of facts. It will be observed that it starts and abides in instinctive control, or tact; but presents this marked character: the gradual subordination of the mechanism of mental life to its ends. Step by step, the will assumes the power as it is disclosed, and maintains its self-activity by practice until the authority of reason has become possible. In short, we meet with the same phenomenon here as we meet in spontaneous volition, viz., the constant mediating of reflex action and 'freedom.'

3. The same fact is presented in what I have described as teleological control, or self-control in the ultimate sense. The life of man, says Hartley, is a journey from self-interest to self-annihilation. This thought, which is sadly neglected in both psychology and pedagogy, owing to the dominion of physical theories of conduct, plainly implies a progress from a certain aspect of the self to another, with the full consciousness of ends. Not to wrangle on the meaning of the terms, 'self-interest,' and 'self-annihilation,' all will finally admit that the most distinctive characteristic of mind is *activity directed towards ends*. The will, in other words, is teleological. In the first place, it

is so, spontaneously. Tact is a species of semi-conscious adaptation of means to ends. Mechanism is not opposed to purposive selection; it is itself an example of selection, and therefore rests finally on voluntary control. In the case of spontaneous control, the end sought is so largely involved in the operation of the reflex-activities, that the apparent automatic response given bears the outward marks of being purely involuntary. But in so far as ends are proposed will is involved. If the phenomena of tact be resolved entirely into mechanism, it ceases to be a state of the finite consciousness except as re-presented.

The presence of ends in immediate voluntary activity is more readily verified. Synthetic activity is now denied by none but materialists, and so far, therefore, the will in seeking the control of the operations of consciousness is teleological. In specific cases, multitudes of which can be gathered in the class room of any school in the land, the conscious adjustment involved in this fact can be seen. The presence of ends is the light of all mind.

But it is more particularly when the ideals of reason are considered that the full swing of voluntary activity is made known. The will is never 'freer' than when it brings itself under the laws immanent in reason. A lawless will is an abnormality: spiritual principles lie back of all mechanism, and it is one of the glories of man that he can be appealed to on grounds higher than those of self-interest. Now the stable condition of voluntary control which is reached as a result of 'self-denial' for the sake of higher objects and ideals, is the result of conscious adjustment: 'The self' as an immediate object of direct cultivation is brought under higher rational ideals, through the unifying activity involved in all our teleological self-activity. What we call self-control, which expresses both spontaneity and final purpose, is thus the most concrete case of voluntary control. It includes the so-called bodily self, with its mechanical arrangement of organs and functions; it includes the empirical self, and developing intellection; it includes the ideal self, that is, the spiritual self which furthers or hinders all the other processes. In the construction of this self, it is will that plays the controlling part. The feeling about the neck and head, into which some<sup>1</sup> would resolve the consciousness of self, is purely an organic matter, not directly connected with self-consciousness. But 'nature' could appear a unity only for the reason that the will teleologically synthesizes the complex activities of the self; nature has no meaning apart from unifying intelligence, and therefore knowledge would be impossible, even knowledge of the neck- and

<sup>1</sup> Cf. Professor James' *Principles of Psychology*, Vol. I., 300 p. ff.

head-feelings, apart from voluntary control. The essence of selfhood is this voluntary activity directed by ideals.

In ethics and religion these phenomena are matters of obvious experience.<sup>1</sup> The point we make is that the phenomena of voluntary control are obedient to a general relation, which obtains between reflex-activity and *all* the forms of 'freedom'; that the real question involved is this mystery of control, and not freedom *or* mechanism, as the alternative is usually put.

The relation of these facts to the problem of noetics and ethics is obvious, but too large to be explained in this connection. It is plain, however, that the claim for free intelligence as a constitutive element of knowledge and conduct; the claim that knowledge is impossible without synthetic activity, turns on the implicit acknowledgment or denial of the phenomena briefly presented above. In the experiments conducted by Professor Ladd, briefly referred to, the claim made was that the will not only can, but does, control the physical conditions of intellection. It is true the isolation involved in all experimentation required special conditions, and the purposive choice of means and ends, in the class of facts brought out by him; but the general result was to establish a far greater degree of control than was commonly, or academically, supposed possible. Even making all due allowance for the influence a distinguished teacher is almost always able to exert on the pupils he teaches; making all allowance for the fact that we are liable to see what we *want* to see; the simple fact is (and it is borne out by the psychology of suggestion),<sup>2</sup> that the will has more control and is a more prominent factor in our life-history than current psychology is in the habit of admitting. Whether we look at the spontaneous, the conscious, or the teleological form, we make our own character and destiny; I would go further and say, in the light of the few facts we know, that unknown possibilities of voluntary control are the necessary corollary of the known, and that the future life and human immortality are (in any worthy sense), dependent upon the 'free' adjustment of our souls, in the society of being (the ultimate nature of which cannot exclude intelligent purpose) to God, freedom and immortality.

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<sup>1</sup> Cf. Bosanquet, *Psychology of the Moral Self*.

<sup>2</sup> Cf. Sidis, *Psychology of Suggestion*.



## ETHOLOGICAL PSYCHOLOGY.

In the *PSYCHOLOGICAL REVIEW* for September, 1899, Mr. C. B. Bliss reviews my pamphlets on Ethology in sympathetic and appreciative fashion. These pamphlets were prepared primarily at the suggestion of one of my colleagues at the University of California and for the purpose of giving the University people some idea of the work I was trying to do. New work is always in great need of both criticism and sympathy. I hungered and do hunger for both. Knowing that my colleagues could find out more about the work if they wanted to, I ventured to pack into a very few pages an amount of material far too great to be clearly set forth in anything less than a good-sized book. As some of the readers of this *REVIEW* may agree with Mr. Bliss that the work is important, perhaps I may be pardoned if I comment on the impression my work has produced on—may I say—my fellow-psychologists. The history of the terms 'Education as Related to Character' and 'Ethology' cannot well be discussed here; suffice it to say, they have a history and are largely due to local conditions. I certainly have no desire to give new names when they can be avoided.

In a sense, all the sciences that have to do with consciousness may be called psychology. I do not plead guilty to the charge of holding unworthy views of psychology. Unless, however, the social sciences, philosophy, and the study of education must all be called psychology, I cannot agree that the science of the development of concrete character ought to be called a chapter in psychology. If we agree that all the sciences concerning themselves with consciousness should be called psychology, I see no reason why the term 'ethology' should not be changed to 'ethological psychology.' In one sense, geology may be called 'geological physics.'

I am exceedingly sorry if I have given the impression that I regard psychology as 'unsympathetic, mechanical and lifeless.' Such an assertion certainly does not occur in my writings. For instance, I regard the teaching of psychology by my colleague, Professor Stratton, as sympathetic, organic and full of life. Perhaps Mr. Bliss will agree with me that what is *ordinarily* spoken of as 'empirical psychology' does not deal with concrete character, however much it may concern itself with interesting and concrete psychical experience. The other psychological sciences deal with various aspects of our complex character-life; ethological psychology deals with these aspects in their interrelation as functions of actual characters. Hence, for instance,

ethology is particularly interested in the study of scientific biography. Most of my advanced students are working on biography. 'Child-study' is regarded by us as a phase of biography. We try to keep the characters we study 'all of a piece' as far as we can. Hence the necessity of using diagrams. "A science of character must make the whole man significant, must show his development in all its aspects, must integrate the ethological aspects of biological, psychological, social and historical sciences, as well as relate itself to the various philosophical disciplines." In the sentence just quoted I am willing to strike out all of the adjectives qualifying the word 'sciences' except the term 'the psychological.' Ethology would still remain as a chapter of psychology very different in its method and standpoint from all the other chapters.

The 'cone' diagram in my pamphlet is not intended to show all aspects of the subject. Diagrams are like parables; they must not be taken too literally or pushed too far. Not only are we careful in our work not to put too much dependence on mechanical devices; we are also careful to provide ourselves with diagrams that show to some extent the varying value of the different aspects of character. To illustrate: we use a diagram showing the spiral movement in the development of character *from* the predominance of self-assertion, *through* the predominance of religious instincts, *to* the primacy of logical insight. In another diagram we indicate the connection of self-assertion with the instincts or tendencies for play, art and ideality.

As my work at present is in connection with a department of pedagogy, it is natural that I should seem to put too much stress on the school-studies and too little on the influence of authority and personality. Mr. Bliss would not find that fault with the actual ethological work. I meant what I said when the following words were written in the pamphlet on ethology: "Each one of us reflects the universe from his own peculiar standpoint. Each is himself and not another. Each character is unique; particular and universal; social, individualistic and personal. The universe's interests are ours and ours are the universe's. We seek to bring about the society of which each one of us is a member. We seek not the society apart from ourselves, or ourselves apart from the society. So far as we interact with others we are simply natural agents, products and not creators; so far as we *really* coöperate with others, we are creators, and are members of the Kingdom of which God is the Integrator." Indeed, the 'studies' are partial results of character-life, and cannot take the place or even share the place of real living. My 'ethology' would have a poor out-

come if it made me exalt the machinery of education. In an essay recently published (*Love and Law*, San Francisco, 1899) I take strong ground against the dispensation of the 'Hoe with the Man.'

It is only fair to say that our study of ethology is being applied to the school-work. With the aid of Mrs. Frances Bracken Gould, a graduate of the University of California, and a very clear-headed kindergartner, we have been able to see many of our ideas put to the test of practice. Work is also being done on history in the schools, and in other directions.

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### SENSATIONAL ATTRIBUTES AND SENSATION.

Professor Calkins, of Wellesley, in the last number of the *REVIEW*, brings up a topic still much in need of similar discussion by psychologists; it, however, seems to me that she makes her conclusions, demonstrated by excellent arguments, tend in rather the wrong scientific direction, her logic assisting, moreover, the contention of the present writer, namely, that these conclusions do not go far enough.

The very fact that introspection at once belies the common assertion of the best text-books that sensations do have attributes, and this despite the circumstance that by definition they should not have them, would seem to argue that the definition itself is useless, or worse. That the term is, indeed, worse than useless is, in short, the contention of the present writer. No one term in current psychology seems to be more misleading or, as Miss Calkins shows, more illogically used than that of this very concept.

The expression sensation seems to be one that indicates little, if anything, more than a somewhat which if it did exist might serve as a basis for the better understanding of something else, namely, the term feeling. It is as if one precise about technical terms, in teaching psychology should say: You all know what a feeling is—well, imagine all the attributes taken away from feeling and you have a notion of a sensation. Indeed, to current usage, a sensation is nothing more than the unnecessary Ding-an-sich of a feeling, or its logical substance in the Spinozistic sense.

It is not here the place to sketch a history of affective terms as used in mental science, nor is it needful to do so clearly to suggest



that at present this term is ordinarily devoid of meaning. It is hoped that in the forthcoming philosophical dictionary all these terms will be fixed as is best for future scientific usage. It surely is not necessary to keep the term in use in its common sense as defined by Wundt, James, Ladd, Titchener and the rest, for the sake of denoting the inseparable periods of special consciousness which an infant is supposed to have during the first days of his life, nor yet to signify certain rare and almost abnormal experiences had by adults in the so-called 'anæsthesia,' or on awakening from coma. Yet these are almost the only occasions on which a 'sensation' has any objective existence. If we examine into the connotative properties of these periods of consciousness, we find but one, and that deficiency—deficiency of 'quality, intensity, extent and duration,' the so-called attributes of sensation. This Professor Calkins aptly shows. Science has no proper use for terms thus purely negative.

It is, in part, the presence in psychology and in allied branches of knowledge of such concepts as this of sensation that makes the subject so often difficult and confusing to the beginner and frequently so uncertain of expression to the more advanced psychologist. The term sensation is not, like the purely abstract notions common enough in other sciences, denotative of something fundamentally important, but its proper use, as noted above, is very infrequent and relatively insignificant. The terms atom and ether, for example, have for chemistry and physics fundamental importance, for on them, at present, is reared in part the noble structures of these sciences. 'Sensation,' on the contrary, is a relic of a now quite outworn psychology of mind-stuff, and positively misleads therefore in this important regard, while indicating nothing of value as amends. Sensation, as defined and in use to-day, is not the substance out of which is characteristically carved, so to say, a feeling; nor is the sensation buried beneath the feeling, forming its base; but in general the sensation simply is not concerned in the feeling at all: the feeling is feeling all the way through and it is nothing else.

Modern advance toward demonstration of the doctrine of parallelism has done away with any usefulness the term sensation might have (as defined by J. S. Mill, for example), as the immediate concomitant of a bodily change, for feelings as certainly as sensations are now considered to be direct correlates of somatic conditions. For popular use the term still has, of course, reason to exist; our present strictures apply only to the technical usage, when it is important to be exact.

My attack then, is not, like that of Professor Calkins, on the attributes of sensation, but upon the term sensation itself as its most frequent application defines it.

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#### AFTER-IMAGES.

In his recently published monograph on 'After-Images,' Mr. S. I. Franz makes with reference to some work of mine a slightly misleading statement. The article referred to appeared in *Mind* for last January, and Mr. Franz implies that its writer while noting the existence of individual variations in the color changes of the image, arbitrarily eliminated them, and that 'her subjects had to be drilled to see a normal image (*i. e.*, like her own).' This way of putting the matter would lead one to suppose that the uniformity of results attained was an artificial one, produced by suggestion: that is, that the subjects were told what I saw, and drilled until they could see the same thing. On the contrary, as the object of the study was to observe the effect of suggestion on the image, I was careful to give no hint to my subjects of my own experiences, during the experiments to determine the normal course of the image. I simply found that while great individual differences existed at first, they tended to disappear in large measure with practice. I quite agree with Mr. Franz that the causes of these variations deserve thorough investigation, but I am inclined to think that one important cause is simply lack of practice in discriminating the image from subjective or other retinal phenomena.

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## PSYCHOLOGICAL LITERATURE.

### NEUROLOGY AND PATHOLOGY.

*Nevroses et idées fixes.* Études expérimentales sur les troubles de la volonté, de l'attention, de la mémoire; sur les émotions, les idées obsédantes et leur traitement. DR. PIERRE JANET. Félix Alcan. 1898. Pp. 492.

In this work Dr. Janet has brought together a number of papers of psychological interest read before various societies, and has added to them a series of chapters based upon studies made upon invalids suffering from various forms of mental disease.

In the introduction he calls attention to the fact that much valuable information for psychologists is to be obtained from the study of abnormal minds, and that no psychological system can be considered adequate which does not take into consideration the disturbances of will, of attention, of memory and of emotion so commonly manifested by the insane.

At the Salpêtrière Dr. Janet had the opportunity of studying many such cases of mental disease especially among the hysterical patients of Professor Raymond, to whom the volume is dedicated. The first fifty pages are devoted to a careful analysis of the mental processes in a young hysterical girl, who manifested a number of fixed ideas. These led in her case to apparent defects of will power, to imperfect power of attention and memory and to abnormal acts of many kinds. The attempt is made to trace these acts and defects to the existence of ideas, present to the subconscious self rather than existing in consciousness. And the proof of this is given in the fact that hypnotic suggestions succeeded in combatting the ideas and thus changing the character, acts and conduct. These fixed ideas seemed to vary in duration and intensity, a fact which Janet implies by the expression *idées fixes stratifiées*, and the most permanent were found more difficult to reach by suggestion than others. A true hysterical explosion seemed to be followed by a clearing away of the ideas and a normal train of thought, and eventually the patient returned to a state of complete sanity after a number of such attacks.

This chapter with its painstaking analysis of this girl's character illustrates well the position of Janet that "experimental psychology



consists above all in mastering the particular subject studied, his life, his temperament, his character, his ideas; and in being convinced that one can never learn enough. It is necessary to place this subject in various given circumstances and notice exactly what he says and does. This method enables us to discover many things which are not without interest for pathological psychology" (67).

In chapter second Janet describes certain results obtained in an attempt to measure the degree of attention and the reaction time of certain individuals, normal and abnormal; his results being about the same as those of other observers.

Chapter third contains a careful study of a patient whose memory of events was suddenly arrested by a shock at a given date and who for nearly nine months appeared to have no memory of things occurring subsequently to that shock, *i. e.*, she lost her power of acquiring new memories. Janet distinguishes this condition from the more common one in which certain memories, as of language, are obliterated by disease; and he shows what a different influence it has on the character. This form of defective memory he names continued amnesia. The subject Mme. D. seemed to see persons and objects, but a moment after failed to recognize them or to have any memory of having seen them. All impressions rolled away and left no trace, even events of importance to her were entirely forgotten at once. An individual may after an accident forget a few weeks of his life but up to the time of the accident his memory was good. An individual of two personalities under suggestion may forget in state II the events of state I, but when again in state I the memory becomes continuous. But in Mme. D. there was a disappearance of the power of acquiring new memories for this entire period. When, however, she was hypnotized and questioned, she related accurately all the events which had occurred during this period showing the existence of an unconscious memory which was not available on the conscious state. Hence though apparently without memory Mme. D. really was not deprived of memory and her talk during sleep revealed the existence of memories of things about her. She also had a clear memory of things suggested in a hypnotic state, would execute post-hypnotic suggestions. Automatic writing also showed the existence of memory of current events. Janet attempts to explain this condition by separating the power of acquiring memories from the power of reproducing memories acquired. In the case of Mme. D. the latter was defective. It is not enough that a simple isolated sensation should be produced in the mind that it thereby should be perceived. There is needed for com-

plete consciousness of a sensation which is expressed by the words *I perceive*, a second mental operation in addition to the first—not only a personal perception of memories but a psychological assimilation of images (p. 135). And any distraction of the mind is capable of interfering with this process. Hence Janet explains this defect of memory as he does the anæsthesia of hysterical subjects as a limitation of the field of consciousness, a feebleness of the personality incapable of synthetizing all the sensations. The origin of this defect of memory in Mme. D. was a sudden shock, and Janet, believing that this shock was really a fixed idea before the mind causing a distraction, succeeded in curing Mme. D. by modifying her emotional state during hypnosis by suggestions directed to the emotion. Thus all suggestions of return of memory having failed, he suggested certain modifications of the original emotion and thus changed the distraction of the mind by the emotion into a more normal state. As a result the distraction ceased and then memory returned.

Another case was that of a young woman who became possessed of the fixed idea that she was a victim of cholera. All the symptoms followed in a series of attacks, but from each she recovered, remaining, however, completely unequal to any mental or physical effort on account of this prevailing fear. Various attempts in a hypnotic state succeeded in modifying the fixed idea but not in abolishing it. Finally Janet tried to decompose the idea and thus destroy it. "The fixed idea consists of a synthesis of many images, and instead of attacking it as a whole we attempted to transform its elements, substituting one for another and thus to destroy the idea as a whole" (p. 164). The patient had a mental picture of cadavers ready for burial. Janet suggested a certain Chinese general with his robes in place of the cadaver, and then suggested that this figure was alive and walking about, thus removing the fear of the cadaver and of the cholera causing death. But this was not sufficient, and then the attempt was made to substitute for the word *cholera* other words by analyzing its syllables and suggesting others in their place, until finally the word cholera appeared to lose its significance for the patient. She could not recollect it, and it no longer caused alarm. In this way the fixed idea being removed the fear ceased and recovery ensued. There were in this case other secondary fixed ideas which are most interestingly discussed in this chapter, and which in turn finally disappeared.

On the basis of these cases Janet gives, in Chapter V., a *résumé* of the great influence such fixed ideas may play on the mental character of hysterical patients; the idea being sometimes subconscious,

not recognized or known by the patient, yet nevertheless determining the acts and feelings and conduct. The essential feature of hysteria is the existence of these subconscious ideas or representations (*Vorstellungen*). They are perfectly comparable to post-hypnotic suggestion, controlling action without being conscious. In some cases such ideas may be, however, conscious, develop in spite of the will, and are not under voluntary control. In the majority of cases it is some emotional shock which gives rise to the idea either conscious or unconscious, and hence emotional shocks are the frequent cause of hysterical symptoms.

In the succeeding chapters Janet studies various forms of hysterical manifestations, hemianopsia, *alloecheiria*; contractures and spasms of the trunk with disturbances of respiration, in all of which he shows that the subconscious idea determined the effect, and its removal by hypnotic suggestion resulted in a cure of the symptom. Such cases as he presents in detail with much interesting psychological analysis are familiar to many physicians, and are not at all peculiar to the French. It is, however, unfortunate that in this country patients are far less easily hypnotized in the ordinary manner, although the success of the admonitions of mental healers and Christian scientists, so-called, demonstrate that auto-suggestion has over many minds a controlling influence. It is quite certain that the mental state of calm induced by certain methods advocated by these misguided and ignorant individuals may act as hypnosis acts to counteract states of emotional excitement of a subconscious kind and thus benefit the individual.

Chapter IX. contains a study of a case of insomnia, the remarkable case of a young woman who for two and a half years did not sleep at all, being wakened within a minute of falling asleep by a terrifying idea, the memory of the death of her child. The actual character of this idea she had no recollection of during her waking state; and it was only in a state of somnambulism induced by hypnotic suggestion that Janet succeeded in eliciting from her the memory which caused the terror. By suggestion during hypnotism this memory was disintegrated and she was cured. Janet emphasizes the great influence which subconscious or semi-conscious ideas have upon sleep and its disturbances, a fact only too well known to almost every one practically who has suffered from insomnia. The existence of a fixed idea on the subconscious level is enough to prevent or disturb the condition of the mind necessary to the obliteration of consciousness occurring in normal sleep. Many persons in sleep are really in a state in which the subconscious self is quite awake. Thus a mother may watch her child while asleep, waking at the least movement or cry of



the child yet remaining undisturbed by other far louder noises. Many persons can waken at a given hour. Thus it is evident that subconscious processes may go on during sleep and may modify or prevent it. Such disturbances of sleep as are thus produced must be treated as Janet shows rather by suggestion than by drugs, by removing the fixed idea which dominates the subconscious self and reacts upon consciousness.

Chapter X. contains a study of demoniacal possession in a lunatic and of Janet's success in exorcising the demon by hypnotic suggestion. The analysis of the condition of the patient is most interesting and will well repay the study of every alienist as there are doubtless in every asylum similar cases easily curable if one had the patience to examine the mental characteristics, the method of the development of the delusion, and to obtain control of the individual. The clue to these cases according to Janet lies in discovering the fixed idea, almost always subconscious, which gives rise to their insane acts, and by removing it by hypnotic suggestion. There is incidentally introduced into the chapter an interesting hint regarding the explanation of the acts and revelations of spirit mediums.

Janet ascribes their automatic writing and unconscious statements to the subconscious self open to the suggestion of the individual consulting them and hence responding to him as he may desire.

In Chapter XI. crystal vision is described, and the position taken is that the visions are unexpected involuntary visual memories of the unconscious self which becoming conscious cause surprise, and seem like revelations.

Chapter XII. is of particular interest, as it is devoted to a study of the effect of hypnotism upon the person hypnotized. Janet affirms that after each séance there is a period of exhaustion during which suggestions made become fixed, and then a latent period in which suggestions are active but are gradually fading in intensity, and at the end of this there comes a period of desire to be re-hypnotized with a state of mind in which the patient is very dependent upon the hypnotizer. If not re-hypnotized the original ideas, paralysis, etc., recur. Hence, Janet emphasizes the need of a course of hypnotization in any case of disease rather than the expectation of cure from a single or a few séances. He then proceeds to study the state of mind of dependence which those who have been hypnotized feel, and shows that this state of mind is characteristic of many individuals in the community who need direction by others, and who are thus influenced without being hypnotized. It is the subconscious self which is really reached

and directed by a strong will in the case of these weak persons, and unless it is directed they are unhappy and incapable.

As a study in psychology few recent works can approach this book of Janet's for interest and profit. The one fact which it emphasizes is that character and conduct are the result as much of unconscious as of conscious mental activities, and that no study of individual action, either sane or insane, can be considered complete which neglects both these factors.

M. ALLEN STARR.

*Nervous and Mental Diseases.* H. CHURCH and FREDERICK PETERSON. Philadelphia, Saunders. 1899.

It is not an easy task to write a text-book upon mental disease. The writer should possess considerable knowledge of psychology both of its subjective side and also of the many recent advances in the physiology of the brain. He should, furthermore, have such a practical knowledge of insanity as can only be acquired by its clinical study in an asylum. And lastly it is of no little moment that his literary style should combine clearness and accuracy of thought with a felicitous use of language. Among recent text-books upon Mental Diseases, that of Dr. Peterson seems more nearly to fulfill these conditions than any with which we are acquainted. He has a facility in stating his facts which renders his views easily understood by the student or by the ordinary reader. He has a thorough knowledge of insanity from long residence in a large institution where his time was not devoted to the petty detail of management but to the study of his patients—and his familiarity with psychology is easily detected on every page.

The book falls naturally into two portions—the study of the symptoms of insanity and the study of its various forms as they are clinically manifest. In the first part the physiology of the brain plays a prominent part, and the unqualified materialistic position is, of course, assumed. This seems inevitable from the medical standpoint, and we cannot but feel that too little importance is given to the subjective side. There are many mental processes which wholly defy physiological explanation such as the formation of delusions, or the permanence of fixed ideas which either subconsciously or consciously control thought and action, and it is the tendency of the materialist to devote his attention to these rather than to the hallucinations or defects of memory which are so much more easily explained. It seems to the writer that too little attention has been given by the author to the writings of the French school, especially to such works as those of Feré and Janet;

and that their theories in regard to the existence of fixed ideas below the level of consciousness which certainly play a large part in the development of certain symptoms of insanity might have been utilized in this section.

Dr. Peterson has avoided very nicely the abyss into which many writers on mental diseases have fallen by refraining from any discussion of the classification of the insanities. The time has not yet come for a classification, as no basis—pathological, clinical, or theoretical—has yet been found. He merely takes pains to give certain classifications of other writers, and then takes up the well-recognized forms of insanity, melancholia, mania, demencia, paranoia and paresis. These are carefully discussed and well described, and will give a good clinical picture to the reader.

The work can be recommended to the student of psychology and of medicine as a concise and satisfactory text-book upon a difficult subject.

M. A. S.

#### THE EMOTIONS.

*La peur et le mécanisme des émotions.* DR. PAUL HARTENBERG. Rev. Phil., XLVIII. Pp. 113-134. Aug., 1899.

*Observations sur le pouls radial pendant les émotions.* N. VASCHIDE. Rev. Phil., XLVIII. Pp. 276-316. Sept., 1899.

Hartenberg's article is written from the point of view which regards an emotion as an interior synthesis of motions. Hence the organic changes which, on the James-Lange theory, are the cause of the emotion, are here considered to be the emotion itself and only the cause of the consciousness of the emotion. This is clearly a pure difference of definition. The distinguishing feature of the author's analysis of the emotional process is the insertion at both its initial and its final stage of a central process of association. The efferent discharge is held to be controlled by a motor image having its center in the prefrontal convolutions, an image whose associative function is to coördinate the various discharges and to mediate between the 'psychic representations' and the 'emotion.' And on the completion of the circuit there is held to be a similar sensory image, situated in the same area, whose function is to combine the impressions received and to connect them with other images. There is here a recognition at least of the coördination of elements in the emotional process, though no distinct recognition of the important problem of their coördination relatively to the so-called 'object.' Unfortunately, so many of the



bodily 'expressions' appear to fall outside of this last coördination altogether and to be mere accidental accompaniments. Moreover, we may at present perhaps still hesitate to accept the views of association centers, on which the author builds his theory, as final. As to the emotion of fear, the only thing noticeable in the incidental treatment here given of it is the testimony against Lange's view of the importance of the vaso-motor phenomena, the fact being pointed out that, while the most constant phenomena in fear are arrested respiration, constriction of the thorax and the feeling of stifling, there is a great deal of individual variation.

Vaschide's investigations have also reference to Lange's theory, in that they deal with one aspect of the relation of emotion to circulation. The particular question studied is the relative frequency during the course of an emotion of the radial pulse. This subject had been already experimented on by Binet and Courtier in the laboratory; the present study deals with it in the case of emotions spontaneously aroused by the experiences of common life. The investigation under these circumstances was naturally one of great difficulty and delicacy, and opinion will probably differ, not only as to the value of the results obtained in any given case, but also as to the conditions under which investigations of this sort are even admirable. The proverbial savant who botanizes on his mother's grave is certainly not more shocking in his devotion to the sacred cause of science than our psychologist who takes his mother's pulse on their first meeting after the death of his father (p. 300), and again at his father's grave (p. 301), and who examines and records his own pulse when alone at the grave, after the emotion had seized him 'avec une fureur et puissance énorme' (p. 307). However, the results are not a little interesting. They go to confirm the experiences of Binet and Courtier, viz., that in all emotions, whatever their quality and tone, there is usually first an acceleration and then a slowing-down of the movement of the heart. Vaschide found this result uniform in all his observations and under all circumstances. The only important difference was that in the strongest emotions of grief, the movement both rose higher and fell less evenly and to a lower point than in the intensest emotions of joy. In emotions of moderate strength there was almost no difference. The conclusion, therefore, is that the pulse alone is no criterion of the quality of an emotion.

H. N. GARDINER.

*Ueber den Begriff der Gemüthsbewegung.* C. STUMPF. Zeitschrift f. Psych. u. Phys. d. Sinnersorgane. Bd. XI. Pp. 47-99.

The interest of the author in this article is to get the adequate and defensible definition of emotion, rather than to point out the conditions of its origin and development. Though implying the entire affective process, the considerations are limited to those phenomena which are especially regarded as emotions (*Affect*), viz., joy, sorrow, hope, fear, wonder, etc. The positive outcome is rather speedily reached in the preliminary analysis which concludes thus. The peculiar quality of a definite emotion, which constitutes its inner nature for our consciousness, cannot be defined in any manner. The most exact and complete definition can mention only certain rather uniformly recurring marks; but to one, who has never lived through the state, the definition cannot make plain what would transpire in his breast. This need of immediate experience does not forestall an analytical account of the emotional content of consciousness, which admittedly presents nothing new, but confirms the older intellectual accounts given of the emotions. The popular equivalency of emotion and affective process does not aid scientific psychology. There is a recognizable difference between emotion and other feelings, as to intensity, time-rate, ideas, and judgment involved. Emotions do not arise from sensations directly. The sphere of emotion is greatly widened when judgment is integrated in the emotive state. This factor adds no difficulty to the definition. Emotion is also distinguishable from desire. The latter is related to the actual, the former to that which ought to be. In this respect, emotion may be defined as a passive condition of feeling, which relates itself to a judged content. S. insists that a real emotion presupposes a certain amount of mental development, rather than being innate, or given with the biological structure.

The scientific need of pointing out the inner nature of emotion more conceptually, so to speak, has given occasion for the formation of the more modern sensualistic theories. It is supposed that referring to blood and muscle makes the phenomena 'clearer,' because we are more familiar with these in life, than with the intellectual principles of the older theory. It is also supposed that the problems of classification are hereby simplified. The critical portion of the article takes up the theories advocated by Ribot, and by James and Lange. The former regards emotion as only complicated states of the sensuous feelings of pleasure and pain. This theory might have a show of acceptability if judgment, as affecting feeling, could be reduced to mere idea or sensation. Most psychologists to-day reply to this demand in the negative.

The James-Lange theory is not to be identified with that of Ribot, though there is much in common. This view finds the essence of emotion in the peripheral corporeal processes, L. selecting vaso-motor changes, J. the vegetative processes, or visceral sensations. The obscurity of the theory lies, in part, in its general inability to locate the emotion in its exact relation to the sensory stimulation and the attendant organic reflexes. It is shown how the real conception of the theory depends finally upon the psycho-physical principle adopted, since it becomes a question of the casual direction. The state of the discussion of the theory, after fifteen years of defense, is not encouraging. Opponents are more numerous than adherents. The advocates forget that, in spite of all objective and physiological psychology, an opportunity of self-observation must be essential to any attempted definition. The question of the theory cannot be referred to facts, since new facts of emotive reactions are not forthcoming. It remains a question of the power of the arguments. The proof rests on two considerations: Nothing of the emotion remains when we think away all the so-called accompanying phenomena and the corresponding organic sensations; secondly, emotions are produced by purely physical means, even when the representation of objects is entirely wanting, the latter being the chief feature of the older theory. In the first instance, the pathological proofs fail, since the argument supports either the old or the new theory. The proof is robbed of all power by the fact that in anæsthesia, *e. g.*, there is necessarily a reduction and impairment of the intellectual functions. The hypnotic cases tried by Sollier fail in a similar manner.

Besides the particular grounds of proof offered, the two projectors of this theory appeal to certain general principles to support their views. We are presented with the strange spectacle of a physiologist, L., invoking the aid of philosophical monism, and J., a philosopher, putting forth the physiological law of ideo-motor effects, to support the theory. Monistic hypotheses are rejected, however, where psychological observation alone must be admitted in determining a definition. Emotions are something *psychical*, whether the monism becomes physical or spiritual in its logical formulation. The law of dynamogenesis, as interpreted by J., is not fully proven. The factor of the stimulus threshold is omitted. Ideo-motor effects are apparent only when the stimulus has reached a certain intensity. Féré's results on sensation and movement are not conclusive, since they contain too many defects of method. The brain is not a mere tube, through which every drop of the stimulus flows immediately to the



periphery; it is more like a catch-basin. Every sensation does not, fortunately, as shown by Sommer and Herschlaflf, necessitate reaction. J's contention must be regarded as a gross exaggeration. Thus the critic proceeds in pointing out the gaps in the progress of the arguments, until it is maintained that there is not a shadow of proof from the standpoint of the theoretical principles dragged into the debate.

Finally, it is shown with some detail, that there are positive grounds of proof against the sensualistic definition. It stands in direct contradiction with the facts of consciousness. According to the theory, all organic sensations should be emotions, which is manifestly untrue. Again, in case the theory were a real interpretation, emotions must be identical in intensity, quality, and time-rate with the sensations through which they become defined. Nothing of these relations are found. J's classification into 'coarser' and 'subtler' implies, by comparison, a common, but unnamed, factor which should have been taken as the defining mark of emotive states. The article closes with a recognition of certain justifiable points in the physiological doctrine, and a brief discussion of the nature of apathy as an emotive state.

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*A Study of Anger.* By G. STANLEY HALL. The American Journal of Psychology, July, 1899.

President Hall contributes a very suggestive inductive study of a much neglected subject. A good list of words in the English language bearing on states of anger is given in the beginning. Medical literature and anthropological lore are ransacked for hints and suggestions bearing on the anger psychosis. An empirical, inductive study is then pursued based on the questionnaire method. Cases of spontaneous anger are cited. They may be due to the necessities of growth or over lability of nerve cells or centers. The satisfaction and real physical pleasure that sometimes follow anger suggest that it has its place in normal development. A long summation of petty vexations culminating in a form of erethic inflammation may reach its fulminating stage without any cause assignable by the subject or observable by others. Opposition to the dogmatic habits of a rutty specialization is one of the most frequent of chologenic agencies. Education is defined, in part, as learning to be most angry with those things that most deserve it and maintaining a true perspective down the scale. Chologenic agencies are, of course, numerous, such as

personal antipathies based on physical forms and features, aversion to particular acts or automatisms, dress, ornaments, habits, thwarting of expectation or purpose, contradiction, limitations of freedom, pride, justice, etc. Play and mock fights often contain a little repressed anger and are good vents.

The physical manifestations of anger include the vaso-motor disturbances, glandular secretions, salivation, swallowing, nausea, spitting, disturbances of the respiration, various involuntary movements, attitudes and postures, biting, scratching, kicking, etc., etc. The vaso-motor disturbances present a very alluring field for investigation. The very painful cardiac sensations are quite prominent. Letting of blood seems to modify considerably the strength of the anger attack. Menstruation is sometimes arrested. Erethism of the breasts or sexual parts occurs at times. A glandular psychology is mooted. The effect of anger upon the mammary secretions of women is noted. Constipation and diarrhœa are at times the result of irascibility. In two cases a rash, once said to be all over the body, follows every fit of anger in the child. Swallowing, gagging, etc., preliminaries of a fit of anger, are referred to as possible residua of the actions of carnivora as they are about to attack and slaughter their prey. As swallowing is the act of appropriating life-giving food, so the nausea and the antiperistaltic movements of anger mean the repulsion or even the regurgitation of food. By a process of short circuiting and transference of associated kindred meaning the same physical movements accompany a similar mental action or state. Some good hard common sense remarks are made in matters prophylactic and therapeutical which might be taken to heart by those, who by reason of their sentimentality have outgrown their age.

The present reviewer cannot but suggest a few theoretical points of view in reference to the study of anger. The emotions appear to represent the inchoate, uncivilized elements of our lives. They are the Saturnalia of the animal and slave parts of our inherited constitution. As reversions they are subject to some possible explanation. An extension of Hughlings-Jackson's nervous level theory may be of assistance here. With progressive automatization of the various levels in ascending order, consciousness normally accompanies the latest evolved levels. There is apparently maximum of consciousness with maximum of nervous hindrance and nervous expenditure and a minimum of consciousness with a maximum of automatism. In the normal intellectual life the lower levels with their vascular, glandular and muscular subordinated attachments act more or less automatically. At

one time we can easily imagine the whole mental life of our early ancestors was almost wholly absorbed in the reports from these 'serving organs of our nether world.' To-day in unusual circumstances and unwonted contingencies some peculiar stimulus or combination of stimuli, as for instance a physical injury, may set that same nether world of the lower levels in unwonted excitement and the vascular, glandular and muscular combinations of another age may be set in motion and reports thereof, confused and tumultuous, be sent to the present seat of consciousness, the cortex. I can discover in the rougher emotions at least nothing else but disordered masses of sensations from the central and peripheral organs of the muscular, glandular and vascular apparatus accompanied generally by joy or depression. In the emotion of fright, for instance, at an umbrella being opened at my side, I can discover nothing but a mass of disordered sensations arising from the convulsive movements of the muscular mechanism accompanied by vaso-motor sensations arising from cardiac congestion.

Sutherland has well shown the growth of vaso-motor adaptations in the presence of sudden emergencies and unwonted stimuli. Now some of these older adaptations, some of these older coördinations and combinations existing between the nervous system and the motor mechanism of the body may lie relatively dormant or may act automatically and unconsciously. In the civilized life of to-day there may be no need for them to report to consciousness excepting in cases of emergencies. Moreover when they do function they may result in disordered masses of muscular, glandular and vascular sensations owing to the disorder produced by the superimposition of newer and more modern coördinations due to the newer adaptations and to changes made necessary by correlation of growth. Remove the later coördinations, that is, remove the inhibition and control of the later formed associations of ideas and movements and the result is the emotional phenomena of actual warfare, the struggle in all its forms, strikes, holidays, disease, etc.

With the report of every new investigation on the nature of the emotions it is becoming increasingly clear that visceral functioning furnishes the organic algedonic basis of the personality. Some one has said that consumptives generally die happy, but no matter how certain a man's convictions are of a happy immortality, he will never die a triumphant death with disease below the diaphragm.

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## EXPERIMENTAL.

*Ueber die 'Verschmelzung' von Empfindungen, besonders bei Klang-eindrucken.* EJNAR BUCH. *Philos. Stud.*, XV., 1-66; 183-278.

These articles were first published in Danish as the habilitation address of the author in the University of Denmark. The first is theoretical, containing a criticism of current theories of fusion, especially those of Stumpf, Cornelius, Helmholtz, Wundt, Külpe, and James. He takes up in order the subjects of attention, apprehension or perception (*Auffassung*), analysis, and fusion proper. The rôle of interest in voluntary attention is illustrated by examples from genetic psychology. The perception of a complex sense presentation is shown to depend upon interest and knowledge of the fusing elements. Then interest and previous experience, as determining the observer's attitude toward the presentation, are found to be important factors in determining the limits of analysis or fusion. His definition of fusion is essentially expressed in the following: We speak of fusion when we encounter a number of stimuli which, in place of each arousing its own sensation as clearly and distinctly as if it appeared alone, produce a combination-presentation or presentation-mass in which a change would take place upon the elimination of one of the stimuli. The avowed object of the research is to determine by experiment whether this fusion is a mental process by itself or is simply the general designation for known influences upon perception.

The second article contains the report of the experiments. The principal apparatus consisted of a series of twenty-three organ pipes with a manometric contrivance by means of which the pipes could be energized with equal force. A variety of combinations of tones were produced by sounding the pipes simultaneously in pairs. The principal tests were made upon nine observers who were simply required to state whether they heard one or two tones. The observers fall into two classes according to the attitude they take toward the presentation. One class of observers attempt to analyze the tone and consider failure to perform the analysis a criterion of fusion. The other observers seem to judge merely by the general effect. This difference in method brings about radical differences in the results, each class however, presenting some common characteristics. Tones at some intervals apart have a greater tendency to fuse than at other intervals. The intervals may be arranged in a series according to the number of times the respective tones fused in these experiments. The author shows by an elaborate analysis of the results that these differ-

ences may be accounted for by the variation of such known factors as consonance, beats between partials, familiarity with certain intervals, etc., and therefore concludes that there is no ground for assuming the existence of degrees of fusion aside from the variation in such factors. In order to check the results, he performed the same experiments using an Appunn 'Tonmesser,' and obtained results that virtually agreed with those obtained with the pipes. Notwithstanding the keen criticism and the careful and elaborate experiments, the author does not arrive at anything essentially new, and the contribution has its chief value in the exposition of his own theory of fusion.

C. E. SEASHORE.

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*Neue Untersuchungen über die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel.* CHR. D. PFLAUM. Philos. Stud., XV., 139-148.

This is a repetition of Wundt's experiments to determine the direction and extent of disparity in time in the perception of two simultaneous impressions through different senses. The 'Complicationspendel' described in Wundt's *Physiological Psychology* is employed. A pendulum moves a pointer over a circular scale and rings a gong as the pointer passes any desired number. The observer is required to state at what number on the scale the pointer was when the sound occurred. The author finds that the amount and direction of the displacement vary with different individuals and depend upon the speed of the pointer. He thus reconciles the contradictory results previously obtained by Wundt and von Tschisch by showing that there are individual differences just as in the personal equation in the eye and ear method of astronomers. The maximum difference between individuals in this test is about 0.01 sec. which is much less than the differences found in the corresponding personal equations of different astronomers.

C. E. SEASHORE.

*Die Präcision der Blickbewegung und der Localisation an der Netzhautperipherie.* CHAS. B. MORREY. Ztsch. f. Psych. u. Physiol. der Sinnesorgane. XX., 317-325.

The author measures the error in the eye-movement, by which we seek to fixate a momentary peripheral stimulus, by the discrepancy between the actual position of an electric spark and the position of a

pointer, placed by the observer at that point of a dimly lighted background, where the spark appeared to be.

After correcting for inaccuracy in placing the pointer, the error of movement is found to increase directly with the distance of the peripheral stimulus from the primary point of regard, and to consist of a constant tendency to underestimate the distance.

The error in peripheral localization is assumed to be identical with the error in the eye-movement.

The results published are based upon single experiments, each for more than 700 different positions of the peripheral stimulus. No measurements are made for movements of less than  $8^\circ$ . The apparent error in the method of designating the terminus of the eye-movement averages half the total error, while for  $8^\circ$  the two are equal. These facts undoubtedly account for the extreme irregularity of the author's curves.

The problem is interesting and important, but the method used is full of complications and probably incapable of giving accurate results. It is doubtful if anything will be entirely successful, except some means of photographic registration.

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*Die Form des Himmelsgewölbes und das Grösser-Erscheinen der Gestirne am Horizont.* W. VON ZEHENDER. *Zeitsch. f. Psych. und Phys.* XX., pp. 353-357.

This paper is supplementary to the earlier article which has already been criticized by the present reviewer in the September number of the *PSYCHOLOGICAL REVIEW*, page 547. The same errors are committed here as in the first article. The author speaks of '*die Volkmann'sche scheinbare Divergenz zweier vertical stehender Parallellinien*' (p. 356), and states on the next page:—'*nur solche Verticallinien parallel erscheinen, die in Wirklichkeit nicht ganz genau parallel sind, sondern \* \* \* nach oben ein wenig convergiren.*' As has been pointed out these statements are the exact reverse of the truth.

The other part of the paper is devoted to an effort to show that the apparent flatness of the heavens is a result of '*Tradition*' rather than of connate ideas or of experience. Finally, the apparent variation of the size of the sun and moon is explained by contrast with the apparent angular extension of the sky which is overestimated at the zenith and underestimated at the horizon (again an incorrect statement), although no reason is given why the sun and moon should not



suffer exactly the same sort of false estimation as the sky in these two positions.

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### PHILOSOPHICAL.

*Through Nature to God.* JOHN FISKE. Boston and New York, Houghton, Mifflin & Co., 1899. 16mo, pp. xv + 195. Price, \$1.00.

Mr. Fiske's latest book constitutes the third part of the trilogy initiated in 'Man's Destiny in the Light of his Origin' (1884), and continued in 'The Idea of God as affected by Modern Knowledge' (1885). The earliest work, as it may be apposite to recall, offers a summary account of evolution designed specially to lead up to an avowal of belief in the soul's immortality, not as a provable fact, but as an essential implication of 'the' reasonableness of the universe.' The theistic continuation, still basing upon evolution, contains a profession of faith to the effect that "the Infinite Power of which the universe is the multiform manifestation is psychical, although it is impossible to ascribe to Him any of the limited psychical attributes which we know, or to argue from the ways of man to the ways of God." The last little monograph proceeds with the discussion of problems in Philosophy of Religion, and contains illuminating chapters on (1) The Mystery of Evil; (2) The Cosmic Roots of Love and Self-Sacrifice; (3) The Everlasting Reality of Religion.

The first is mainly remarkable for the outspoken way in which it accepts the conclusion so paradoxically put by a younger American thinker—that God is the Devil. In other words, God must be viewed as the author of evil, as well as of what we call good; and the problem is to throw such light upon this unavoidable inference as modern investigation may bestow. The second repeats, with great force and freshness, Mr. Fiske's well-known doctrine as to the part played in the evolving series by the lengthened infancy of the human species of ape, and concludes with an earnest, though dogmatic statement, that the universe exists for moral ends, if any at all. The third defends the reasonableness of the three chief conceptions incident to all religion—the quasi-Human God; the Undying Human Soul, and the value, as a scientific fact (that is, as a *cause* in the process of evolution), of the postulate of the Ethical Significance of the Unseen World. Needless to say, the argument is presented with all the charm that

this author has accustomed us to look for; and, although exceedingly brief, and therefore wearing a certain air of dogmatism, anyone who reads between the lines can note the wonderful range of knowledge it presupposes. It seems to me that, without doubt, similar, if not identical, ideas will be enunciated by some expert in this subject, when the new systematic philosophy of religion, that so many await, makes its appearance.

I have but two criticisms to pass. In the hands of those who know, books of this type can be productive of nothing but good. But, most unfortunately, in the hands of theological reactionaries, whose unconscious hypocrisy is their besetting sin, a sin that takes form in a persistent defence of that for which there is no evidence, I feel sure that Mr. Fiske's outspoken opinions will work widespread harm. It is, possibly, a pity, too, that he has almost repeated the title of Dr. Edwin A. Abbott's striking work (Macmillan, 1877).

R. M. WENLEY.

UNIVERSITY OF MICHIGAN.

The seventeenth volume of the *Bibliothèque Sociologique Internationale* is *Des Religions Comparées au Point de Vue Sociologique*, by M. Raoul de la Grasserie, who is a judge at Rennes. The subject is one of the first order of importance, but M. de la Grasserie is evidently too much of an amateur in the field of comparative religion for his conclusions to have much weight. His chief authority is a certain M. de Milloné who is not known to fame on this side of the Atlantic at least. 'Cosmosociology' is the name that the author would give to the 'society of God and man' which is religion; he discusses also the 'inter-divine society' of the gods themselves on the lines of the *Iliad*; mortuary religion is not neglected; and the 'organic nature' of religion is brought to the front. The chief value of M. de la Grasserie's work is in occasional classifications like that of 'religious diseases' (pp. 194+) and such occasional touches as calling the monks 'the specialists of Christianity.' As a treatment of religion as a social force, the book is quite inadequate.—Paris, V. Giard and E. Brière.

N. P. GILMAN.

MEADVILLE, PA.

*Spinoza und Schopenhauer.* Von Dr. SAMUEL RAPPAPORT. Berlin, R. Gaertners Verlagsbuchhandlung. 1899.

This work is not, as its title might suggest, merely a comparative study of the two systems, but an attempt to determine the character and extent of the influence of Spinoza upon Schopenhauer.

It is a recognized fact that Schopenhauer, like Schelling, Hegel, Schleiermacher and all of the important thinkers of the time, was affected by the pantheist. But the influence might be direct or indirect—either the result of an immediate acquaintance with the works of the pantheist or merely a product of the Spinozistical ideas which were ‘in the air’ during that period. Our author attempts the task, which is not as simple as it might seem to be, of showing that Schopenhauer was not only early subject to a distinct mediate influence from Spinoza but actually acquired a first-hand knowledge of his works before and during the formation of his (Schopenhauer’s) system. This proof is made valuable, and for that matter possible, by reference to the still unpublished MSS. of the pessimist.

The writer also endeavors, in an exhaustive manner, to determine Schopenhauer’s opinion of the doctrines and personality of Spinoza.

The work seems to be conscientious and thorough, and as a detail out of the history of philosophy it is valuable. However, it is of interest only to the technical student.

F. KENNEDY.

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## NEW BOOKS.

*A Manual of Psychology.* G. F. STOUT. University Correspondence College Press. London, W. B. Clive; New York, Hinds & Noble. 1899. Pp. xvi + 643.

*History of Ancient Philosophy.* W. WINDELBAND. Authorized Translator, H. E. Cushman. New York, Charles Scribner’s Sons. 1899. Pp. xv + 393. \$2.00.

*The Evolution of General Ideas.* TH. RIBOT. Authorized translation by Frances A. Welby. Chicago, The Open Court Publishing Co.; London, Kegan Paul, Trench, Trübner & Co., Ltd. 1899. Pp. xi + 231. \$2.25.

*Social Laws, an Outline of Sociology.* G. TARDE. Translated by Howard C. Warren. New York and London, The Macmillan Company. 1899. Pp. xi + 213. \$1.25.

*Discourse on Method.* René Descartes; Veitch’s Translation. Chicago, Open Court Publishing Co. 1899. Pp. vi + 87.



## NOTES.

WE regret to record the death of the well-known French philosopher, M. Paul Janet, member of the Paris Academy of Political Science and formerly professor at the Sorbonne.

A. KIRSCHMANN, Ph.D., lecturer in philosophy at the University of Toronto since 1894, has been appointed professor of philosophy and director of the psychological laboratory.

DR. CHARLES G. SHAW has been appointed to the position in the department of philosophy in New York University made vacant by the resignation of Dr. J. H. McCracken, to accept the Presidency of Westminster College, at Fulton, Mo.

The Regents of the University of Texas have provided a psychological laboratory which has been placed under the charge of Professor Caswell Ellis, of the department of pedagogy.

MR. CLARK WISSLER, of the Ohio State University, has been appointed Assistant in Psychology in Columbia University.

S. I. FRANZ, Ph.D. (Columbia), and G. V. N. Dearborn, M.D., Ph.D. (Columbia), have been appointed Assistants in Physiology in the Harvard Medical School.

R. S. WOODWORTH, Ph.D. (Columbia), has been appointed Assistant in Physiology in University and Bellevue Hospital and Medical College.

PROFESSOR W. H. SQUIRES, who holds the chair of psychology and pedagogics in Hamilton College has been given a two years' leave of absence, which he will spend in study in Germany. W. B. Elkin, Ph.D. (Cornell), Teachers College, Columbia University, has been appointed acting professor.

DAVID R. MAJOR, Ph.D. (Cornell), Teachers College, Columbia University, has been appointed Acting Professor of Pedagogy in the University of Nebraska, Professor G. W. A. Luckey, who holds the Chair of Pedagogy, having been given leave of absence to carry on advanced work at Columbia University.

PROFESSOR G. S. FULLERTON has returned to the University of Pennsylvania after a year's absence abroad.

PROFESSOR J. MARK BALDWIN is at present at Oxford (3 Museum Road), where he is revising for the press the MS. of his Dictionary of Philosophy and Psychology. Professor A. C. Armstrong, Jr., is also at Oxford, and Professor G. H. Howison is expected there. Professor William James may also spend part of the winter at Oxford.

DR. JAMES H. LEUBA, of Bryn Mawr College, has compiled a card catalogue of psychology, containing about 10,000 titles. The catalogue consists of the contents of periodicals from 1860-1899. The periodicals selected are not confined to those devoted to psychology, but include many journals such as *Nature*, *The American Journal of Science*, etc., in which psychological articles might be readily overlooked. There are indeed many journals omitted, such as the German physiological archives, but it is hoped that these may be indexed at some future time. Dr. Leuba offers to supply mimeographed copies of the catalogue on standard cards at a price not to exceed \$50.00.

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