

in this way because it is the Will of the Absolute that they should so behave" (p. 515). The impression left upon the reader is that the seemingly independent will of personality is in reality none other than the 'quasi-independence' which may be attributed to things. Surely this is a loss of the self as regards the integrity of its free personality. The relation of the Absolute Will to the wills of persons must certainly differ from the relation of the Absolute Will to the manifold forces of Nature.

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Einleitung in die Vergleichende Gehirnphysiologie und Vergleichende Psychologie mit Besonderer Berücksichtigung der Wirbellosen Thiere. By JACQUES LOEB. Leipzig, J. A. Barth, 1899. Pp. 207 and 39 illustrations.

This work from the hands of Professor Jacques Loeb of the Department of Physiology of the University of Chicago is of considerable significance for psychologists. It embodies not only the results of many original researches, but is also a valuable digest of recent literature bearing on the subject. Its importance is enhanced by its negative criticism of current neurological conceptions of cerebral localization and its psychological corollaries. As the author says in the preface, this work grew out of an examination of the assumption made in his previous work¹ that all the reflexes of the animal life are under the specific direction of ganglion cells. This assumption was rendered assailable by the fact discovered by him of the identity in nature of many of the animal and plant tropisms. He was led to one of two conclusions. Either we must attribute consciousness to plant forms, or we must develop a new criterion for the presence of consciousness in lower animals. As we shall see, he adopts the latter alternative.

He takes up first the consideration of the fundamental facts and general principles of comparative brain physiology. The physiological unit has hitherto been regarded as the reflex, while ganglion cells have been regarded as the structural element which makes the reflex possible. Even writers who have seen nothing in the reflex but a mechanically determined activity have regarded ganglion cells as the essential organs of the complicated movements involved therein. Says Professor Loeb, "I never would have doubted the correctness of the older physiology, which gives to the ganglion cell this important rôle, had not my own discovery of the essential identity of the animal and

¹ Especially his 'Der Heliotropismus der Thiere und seine Ubereinstimmung mit dem Heliotropismus der Pflanzen,' Würzburg, 1890.

plant heliotropisms shown the inadequacy of this point of view, and at the same time given me another conception of the real nature of the reflex." This new point of view led him to further experimentation in which these animal heliotropisms are shown to remain even when the central ganglia supposed to be concerned have been removed. In other words, there is a direct relation between the peripheral sense organs and the different musculatures, not mediated by the central nervous system. The general conclusion from his experiments, therefore, is this, that stimulability and conductivity, only, are necessary for reflexes, and that these are universal properties of all protoplasm. Ganglion cells possess no mysterious property, not common to all protoplasmic nervous tissue, for the production of the reflex. The ordinary conception of instincts as inherited reflexes perpetuated by a sort of organic memory in these ganglion cells comes in for its share of criticism. Such a theory can furnish neither a mechanic of instincts nor a natural explanation of their transmission. Their real explanation is found in the phenomena of the various tropisms (heliotropism, chemotropism, geotropism, stereotropism, galvanotropism, etc.). If, then, the mechanics of a number of instincts can be traced back to tropisms common to plants and animals, and if the significance of ganglion cells is limited to the function of conductivity, then we must expect a new statement of the factors which condition these complicated reflexes. Researches on the galvanotropism of animals leads Professor Loeb to conclude that a simple relation exists between the orientation of the nervous elements in the central nervous system (along the line of the chief axis of the body) and the direction of the movement of the body called out through these elements. Tropism reveals itself in two general conditions (1) in the specific irritability or excitability of definite peripheral elements, and (2) in the symmetry of the body. Symmetrical elements of the organism have the same irritability; unsymmetrical elements have different irritabilities. The elements near the oral pole have a greater stimulability than the aboral, or the reverse. The same argument applies to the so-called spontaneous movements, as well as to reflexes. There are two groups of these, (1) simple spontaneous movements, and (2) rhythmical spontaneous movements. That simple spontaneous movements do not require mediation by ganglion cells is apparent, *e. g.*, from the spontaneity of the swarm-spores of algæ. The rhythmical spontaneous movements are more important in this connection. Respiration and heart-beat belong in this category. These are ordinarily attributed to certain ganglia. But the researches of Gaskell, Engel-

mann, Loeb, and others, show that these processes can take place independently of the ganglia to which they have been referred. They do not require a higher coördinating center, but are purely segmental reflexes. This segmental conception is then extended to the whole animal series.

Naturally there arises the question of the psychological application of the foregoing conclusions. It is one of the distinctive problems of brain physiology, in the opinion of Professor Loeb, to develop a theory of consciousness. Accordingly he makes a statement of what he regards as the criterion for the determination of the presence of consciousness in the lower animals. This he finds in what he calls 'associative memory.' By this he means that arrangement through which not only the immediate reactions take place which correspond to the stimulus and the sense organ, but also other effects or reactions which are the normal responses of other stimuli acting either previously or simultaneously on the organism. This had been worked out previously by Professor Loeb in his 'Beiträge zur Gehirnphysiologie der Würmer.'¹ The detailed experimental application of this criterion to all the different classes of animals constitutes the future problem and task of comparative psychology.

On the side of the comparative physiology of the brain the chief object of this work is, thus, to establish the segmental theory of the central nervous system. On the side of comparative psychology, its principal object is to establish 'associate memory' as the criterion for the presence of consciousness in the lower animals. With this general view of his spirit and aim we may pass to a brief survey of some of the facts upon which these conclusions are based.

The first type that we have presented to us is *Medusa*. In those forms of Medusa which possess a more highly developed nervous system in the marginal ring (such as *Hydromedusa*) the rhythmic spontaneity is localized exclusively in that part of the animal containing the nerve ring, though in types possessing a less highly developed nervous system (such as *Aurelia aurita*) any portion of the umbrella is capable of exhibiting these contractions. It is fundamental that the continuity of the connections of the nerve ring is requisite for the production of the synchronous movements which are necessary for the contraction of the umbrella. An analogous example is found in the case of the frog's heart, in which it has been found by experimenting on the contractility of different excised portions, that its different parts contract in rhythmic unison with the sinus venosus, which

¹ Pflüger's Archiv. Band 56, 1894.

when isolated, has the greatest rate of contraction. There is no necessity here for a special coördinating center. The stimulation of one part of the organ results in the instantaneous communication of the stimulus to the remaining parts, and the organ practically contracts as a unit. This is confirmed by Dr. Hargitt's researches (under Professor Loeb's direction) on *Gonionemus* in which two individuals were artificially united, and two days after the operation showed perfectly synchronous contractions. In opposition to Romanes, Professor Loeb denies that these contractions are dependent upon the presence of ganglia, either in the case of *Medusa* or in the case of the frog's heart, and this denial is supported by Engelmann's conclusions that the seat of the automatic activities even of the adult heart is the muscle tissue rather than the ganglia. Professor Loeb cites further the case of the Infusoria in which we have rhythmic contractions of the vacuole without the presence of distinct nerve elements. Such reactions as that of the frog in attempting to remove the acid irritant have been attributed to the complex coördinations made possibly by its relatively highly differentiated nervous system. But Professor Loeb shows that entirely analogous reactions may be secured in the case of *Hydromedusa* in which the nerve ring has been excised.

The next researches are on the central nervous system of *Ascidians*. The peculiar 'purposive' reflexes of *Ciona intestinalis*, ordinarily explained as due to the coördinating functions of its single ganglion, are here shown to remain after its excision. The ganglion has simply the function of conductivity, rendering such reactions quicker, but is not essential to the possibility of such reaction.¹ Similarly, in such reflexes in the higher animals as the contraction of the iris, the function of the central nervous apparatus is demonstrated to be simply the accelerating of the reaction, since contractions take place not only in the decerebrated animal, but in the excised iris. That other tissues beside nervous tissue are capable of such contractions is, again, substantiated by the acknowledged phenomena of contractility under similar conditions in insectivorous plants and in protozoans, where there is no suggestion of a clearly differentiated nervous tissue.

In the *Actinarians* (*Actina equina*) the following experiment was performed. A bit of thread with a piece of meat attached to one end and a bit of paper to the other was laid across its tentacles. In every case, no matter what the relative position of the two ends of the thread, the meat was drawn inwards through the mouth opening while

¹ This is reproduced from his previous work entitled 'Untersuchung zur physiologischen Morphologie der Thiere,' II., Würzburg, 1892.

the paper was allowed to remain inertly hanging outside. Here the explanation is similar to that of the movements of carnivorous plants. There is no need for the assumption of the coördinating function of a ganglionic center. The chemical affinity between the constituent elements of the food material and the tentacular structure is sufficient explanation. That there is no purposive consciousness or intelligence here is proved by further investigations of heteromorphosis, in which, for example in *Cerianthus membranaceus*, a new ring of tentacles became artificially developed in connection with a lateral incision. Here the tentacles attempted, as in the case of the true mouth, to force the morsel of the meat into the digestive cavity at the point where the mouth should have been. This occurred as often as the meat was presented to it. There was no suggestion of associative memory, no learning by experience. It was altogether as mechanical as any purely physical or chemical reaction. Indeed, in the case where the new head was developed near the old one, the two sets of tentacles actually entered into a contest each striving to force the morsel of food into its own mouth cavity. In addition to these chemotropic reflexes, the foot of the actinian is shown to be stereotropic, and the general bodily position determined geotropically. An actinian placed in an inverted position in a test-tube, with its head end down, will regain its normal upright position within an hour by the gradual contracting of its body and tentacles. That this is due to the sheer force of gravitation is shown by Professor Loeb's experiments with a horizontally placed coarse screen through which the actinian which was laid upon it slipped slowly into a suspended vertical position in a purely passive way. When the screen was inverted, instead of slipping out, the foot end began to bend and finally slipped through another mesh of the screen, and so on, until the animal lay coiled among the meshes of the screen.

The reactions of *Echinoderms* are shown to belong to the same general category. The ventral surface of the starfish is stereotropic, while in some types there is combined with this a negative geotropism or positive heliotropism which causes these animals to climb up the slant surfaces of rocks or up the vertical walls of the aquarium. It is necessary for the nerve ring to be intact in order that the animal may regain its normal position after having been laid upon its back; but this is not because of any disturbance of a ganglionic center since a single amputated arm will succeed in regaining its original position as well as the normal starfish.

In his researches on *Planarians* Professor Loeb emphasizes in

the first place the absence of any reactions such as in the higher animals are called reactions to pain. If the head end of *Thysanozoon Brocchii* be snipped off as it floats upon the surface of the water, the tail end drops inertly to the bottom, while the head end (containing the only ganglion of its simple nervous system) proceeds on its way unhindered and without any signs of pain from the operation. The peculiar feature about the orientation of these animals is that on the ventral side we have positive stereotropism and on the dorsal side negative stereotropism. Here again the reactions take place quite as well in the absence of the ganglion as when it is present, except that they are slower. If a cross-section be made which is not quite complete, the head end which contains the ganglion will attempt to creep off (spontaneous movements) while the tail end fastens itself to the nearest object (positive stereotropism). In this type the head end only shows the spontaneous movements. But in a fresh water Planarian, *Planaria torva*, though morphologically very closely related, the tail end as well as the head end creeps off in a very lively fashion in spite of the absence of the ganglion. The spontaneity of progressive movement is evidently here again not a function of the 'brain' or cerebral ganglion. The reactions of *Planaria torva* to light are also of especial interest. They take place as well in the decapitated as in the normal animal.

The same question arises in the experiments on *Annelids*. Has the cerebral ganglion any function of coördination in connection with the movements involved in locomotion? This is shown to be not the case. Even in the dog it is shown by Goltz that coördinations for locomotion are explicable in a manner similar to that in worms, *i. e.*, without calling upon the inexplicable functioning of some ganglion. As in *Annelids* the progressive movements of locomotion take place not only in the absence of the cerebral ganglion, but even when the chain of segmental ganglia is itself broken, so in the dog certain locomotor reflexes occur in the hind legs even after cross-section of the spinal cord. In both cases these reactions are due to reflected stimulation through the peripheral organs (skin, muscles, etc.). The difference between the dog and the earthworm, so far as locomotion is concerned, is not so much the difference in the central nervous apparatus as in the development of the peripheral organs. Had the dog, instead of its long lithe limbs, mere stumps instead, we would have the same phenomena and the same explanation as in the progressive movements of the earthworm. Other functions of the earthworm, as in the case of Planarians are explained as due to heliotropism or chemotropism.

The researches on the foregoing forms have taught us, says Professor Loeb, that the characteristic reactions of these animals are determined (1) by the different forms of stimulability and arrangement of the peripheral elements, and (2) by the ordering of the motor organs, the muscles. The central nervous system has no other part to play than that of furnishing a means of quicker and easier conduction, and the fact of the greater complexity of the cerebral ganglion is simply due to the massing of the peripheral sense organs in the head. He then goes on to show how the segmental conception holds also for *Arthropods*. His first example is *Limulus polyphemus*, in which all of the nervous system was extirpated with the exception of a portion of the circumoesophageal ring and the abdominal (respiratory) ganglia. The portions of the nervous system retained enabled the animal to eat and to breathe, and the animal was actually kept alive for some time by food being furnished it. This completely overthrows the conclusions of previous investigators who asserted the existence of a special control by the cerebral ganglion. The relations in higher animals are essentially the same as those in *Limulus*. The usual reference of the coördinating center for the respiratory movements to the medulla, to the point called by Flourens the 'vital node' is altogether erroneous. This view has rested upon the observation that an injury at this point or section of the cord between this point and the point of origin of the Phrenicus leads to the arresting of the respiratory movements. But Professor Loeb shows that this is a temporary inhibition only like that occurring in *Limulus* immediately after the removal of the suboesophageal ganglion. Langendorff,¹ indeed, has made the important discovery that even the decapitated vertebrate, in which the 'vital node' is removed, is capable of independent respiratory movements. In fact, if in the new-born vertebrate the respiration be kept up artificially until the shock effects pass off, the animal will then continue to breathe in a normal manner. To sum up, the common idea, as represented by Faivre's work, that the centers for the reflexes of locomotion, breathing, etc., are located in the suboesophageal or prothoracic ganglia, in arthropods, is wholly rejected, and the conclusion of Bethe adopted, that these reflexes actually have, as they appear to have, their centers, in the corresponding segmental or abdominal ganglia. Or, to state the principle in general terms, each individual segment of a segmental animal may be viewed as a simple reflex animal like the *Ascidian*. The assumption of special higher coördinating centers is wholly superfluous.

¹Studien über die Innervation der Athmenbewegungen, I., Mittheilung, Archiv für Physiologie, 1880.

The researches on *Mollusks* are wholly in line with what proceeds.

The probability is, then, that the segmental theory holds also for *Vertebrates*. It is Professor Loeb's opinion that the brain represents more, rather than less, segmental ganglia than even investigators sympathetic with this point of view have been inclined to assign to it. Experimentation shows that after recovery from the shock effects of operation, the caudal part of the spinal cord is capable of all the reflexes that are possible when it possesses its normal connection with the cephalic portion (brain). Such reflexes as reaction to irritation of the skin, erection of the penis, secretion of urine, dejection from the rectum and bladder, and the vasomotor reflexes are all retained. Loeb has shown the same to be true in the case of the Crawfish for the respiratory movements, and Goltz has shown the same for the arm reflexes in the frog, while Schrader has shown the same for the movements of locomotion. The cerebral cortex is found to be unnecessary for vision in frogs and fishes: the optic thalamus only is necessary. The theory of a single ruling center or even of a group of cortical controlling centers is thus negatived, and the importance of the independent functioning of the spinal segmental ganglia is emphasized. The chief reasons why the segmental theory has not been accepted hitherto, according to Professor Loeb, are (1) because in vertebrates the brain shows the segmental character only in its earlier embryological stages; consequently only a few brain physiologists have seen that this is the key to the understanding of its function, (2) because the shock effects of operation on the higher animals has so obscured the real effects of extirpation, and (3) because of the influence of a metaphysical psychology with its doctrine of separate functions and corresponding centers.

Professor Loeb's theory of animal instincts grows out of this segmental conception of the nervous system. He looks upon instincts as made up of a series or chain of segmental reflexes (*Kettenreflexe*). By instincts are generally understood unconscious actions of animals directed toward a definite end. Such are the habits of insects which lay their eggs in material which serves later as food for the hatching young. Such also are the periodic migrations of birds and aquatic animals, instincts of self-protection, protection of the young, etc. But many of these so-called 'purposive' instincts are clearly shown by Professor Loeb to be explicable as animal tropisms. His whole chapter on Instincts is very interesting and instructive as throwing light on the segmental theory. Of course, his theory of instincts militates against any theory which suggests that instincts are degraded habits,

the inheritance of fixed modes of action of preceding generations of ancestors. In connection with the question of instincts arises the problem of the transmission of characters. That there is a transmission through the sexual or reproductive cells is not doubted, but the question that has puzzled all investigators is how so simple a structure as a reproductive cell can be the bearer of so complicated an apparatus as that necessary for reflexes and instincts. There are two possibilities. Either the egg is a much more complicated structure than has been supposed, containing all the elements necessary for the fully developed animal, or the complexity of the mechanism of reflexes and instincts can be reduced to the terms of simple elements and processes which can be easily transmitted by the egg without the latter possessing such a highly complicated structure. Weismann represents the former standpoint. The results of Professor Loeb's investigations lead him to adopt the latter alternative. Growth processes are first of all processes of chemical differentiation, with which the particular morphological characters are secondarily associated. The egg is the bearer primarily simply of certain determinate chemical substances, and hence the transmission of characters is possible only in the form of the transmission of these substances, and the nervous system can influence this transmission only as it may be instrumental in changing their nature. Chemical theories of heredity are not new, but in the form in which Professor Loeb presents his theory it has a place quite its own.

This brings us to Professor Loeb's conception of the criterion of consciousness. He finds this, as has been said, in 'associative memory.' Following Mach he identifies what we call self-consciousness or the I-consciousness with the memory factor of our experience, and finds the cerebral cortex to be the medium of the associations involved in the more complex reactions of the higher animals. It is no more remarkable that in certain animals we should have a special apparatus for associative memory than that we should have in certain animals a special apparatus for bringing rays of light to a focus for visual perception. In applying this criterion it is found that consciousness is present in most mammals and some lower forms. The dog responds to its name and greets its master joyously, the parrot learns to speak, the dove to return to its cote or to the place of feeding. Even the tree-frog is granted to have consciousness, though the common frog is denied it. To some fishes it is granted, though in the shark its existence seems doubtful. For the presence of associative memory in invertebrates Professor Loeb thinks there is very little evidence. It

may be granted to spiders, certain crabs, and cephalopods; but is denied absolutely to coelenterates and worms. Especially does Professor Loeb oppose what he calls the anthromorphism of such writers as Romanes, Eimer, Nagel, etc. He finds almost all previous work on ants and bees an overdrawn account of their supposed intelligence, and cites the work of Bethe in support of his own. Many experiments are cited in support of the final conclusion that 'associative memory,' and hence consciousness, is to be regarded as confined to a relatively small part of the animal kingdom. It is not possible to go into the details of these experiments within the limits of this review, but the *facts* seem to be essentially as represented by Professor Loeb. The only query is, whether Professor Loeb's interpretation on the psychological side is the only *explanation of these facts*. Shall we deny a lower form of consciousnesses to these simpler types of animal life simply because we do not find the cerebral apparatus present which conditions our highly differentiated mode of consciousness? May it not be granted that in these lower types associative memory plays a very small part in the life of the animal, and that when it does come it comes in flashes only, in connection with certain exigencies of life, without denying to them either the possibility or the actual presence, at times, of the beginnings of this function? Such researches as those of Professor Loeb no more prove the absence of lower modes of consciousness than the facts of habit in human experience militate against a doctrine of the freedom of the will. A man is no less free because his volitions are regular and rationally systematic. The same is true of the lower animals. Because we have found a statement for the activities of an eel's tail in terms of physics and chemistry, this does not prove that those activities are not also interpretable in another way, provided we can have the chain of psychological inferences unbroken, as in our attribution of consciousness to a fellow human. Are there any greater difficulties connected with the interpretation of consciousness as between man and the dog, than, for example, between the dog and the eel? What we call the voluntary or relatively free acts of the higher type of consciousness, whether it be dog or man, are in no way different psychologically as acts than the tropisms of the lower animal and plant forms, only we have a chemico-physical statement for the latter, whereas we have not as yet, because of the relative incompleteness of our science, an analogous statement for these highly complicated reactions of the higher type of animal. We should be inclined to meet Professor Loeb's encroachments into the realm of the higher animals by not alone granting him what he claims of animal activities (all the invertebrates and

most of the lower vertebrates) for such mechanical or chemico-physical explanation, but we should grant him the whole field, all the activities of the higher animals including man—only, we should insist that after he had covered the whole ground, his explanation of the facts would still be partial only, requiring to be supplemented by what we may call the psychological and teleological interpretation of these same facts. It does not follow, in other words, that there is no conscious side to these phenomena of the activity of the lower animals and plants simply because these activities have been discovered to be mechanically uniform and necessary. On page 162 he cites the facts of the various kinds of tropism in plants and animals as belonging to the same general category as the chemical reaction, say, of the photographic plate, and suggests that since we attribute no consciousness to the latter, there is just as little reason to attribute consciousness to the former. The reply is this: There is not only just as little reason, but there is also just as great reason for attributing consciousness, not alone to the photographic plate, but to all nature as well. This is just the contention of a current idealism in philosophy, that the whole of nature is susceptible of a psychological and teleological interpretation, as well as of a physical or purely mechanical explanation. This whole argument which is employed with such telling effect to prove the utter absence of consciousness may be turned against itself. The error lies, not in the accuracy with which the experimental observations have been made, nor, in general, in the valuable array of facts set forth, but in the *point of view* from which these facts are interpreted, and in the implicit assumption that the physical and mechanical explanation (the 'scientific' explanation, in the narrow sense) is the whole of the matter. There certainly is no reason for assuming the presence of that type of consciousness which is marked by 'associative memory,' in the absence of the cerebral mechanism which is its invariable condition in the higher animals. On the other hand, it is quite as unjustifiable to deny altogether the possibility of any psychological interpretation for these lower forms of activity. Proceeding upon the lines of Professor Loeb's argument, when we have found a physico-chemical statement of the conditions of all human action, 'associative memory' will go out of the same door that has already, in his mind, closed behind consciousness in the lower animals. The steadily increasing indebtedness of biology to physics and chemistry does, indeed, as Professor Loeb contends, cut at the root of unfounded psychological myths such as the theory of a spinal soul, crude pan-psychism, and hasty generalizations

on hylozoism. But this relation of dependency on the physical sciences is to be supplemented by an equally great and increasing dependency, not alone of biology, but also of chemistry and physics, on the psychological point of view. If psychology seems to be losing its position as a distinct science, it is not because the psychological mode of viewing phenomena is decadent; rather, it is because this psychological mode of interpretation is becoming applied to all spheres of activity, supplementing, and, when rightly understood, reinforcing the mechanical or physical interpretation in every department of scientific procedure. Psychology is losing the narrow footing which it once held among the sciences, not to disappear as the superstition or myth of an unscientific age, but to become universalized as the indispensable correlate of all the so-called natural or physical sciences. But after all this has been said, Professor Loeb has certainly rendered a real service, not alone to physiology, but also to psychology, in overhauling in so thorough a manner the unintelligent lumping of all psychical functions in the brain, which has marked so much of the previous work in this field.

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VISION.

On After-images. SHEPHERD IVORY FRANZ. Monograph Supplement to the PSYCHOLOGICAL REVIEW, No. XII. New York, Macmillan. June, 1898. Pp. iv+61. Price, 75 cents.

The present volume is, in the main, an attempt to deduce the factors influencing the perception of after-images. This empirical portion comprises sections dealing with the threshold, the latent-period, the duration, the fluctuations, the space-relations and the retinal transfer.

Experiments upon four subjects show that to have an after-image appear seventy-five per cent. of the times the eye is stimulated it is necessary to have as a stimulus (*a*) for $\frac{1}{100}$ second and 64 sq. mm. surface of light, an intensity of $\frac{2}{3}$ candle power; or (*b*) for one second exposure, an area of 4 sq. mm. and an intensity of $\frac{2}{3}$ candle power; or (*c*) for one second exposure, an area of 64 sq. mm. and a light of $\frac{1}{100}$ candle power.

The latent period (*i. e.*, the time between the end of the stimulus and the first appearance of the after-image) was found to vary correspondingly with changes in the objective stimulus. The lesser times, the greater areas and lesser intensities seem to make this period short,