STUDIES IN BEHAVIORISM.

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Studies of the author in animal behavior date back to boyhood days of the farm, when living things afforded no end of delight, and inspired interest and even affection, which in turn led to multiple appeals to parents and others of *why, how, what for,* and the like, to their annoyance and even perplexity. Later when nature books, like that of Gilbert White, served to show how to discover at first hand answers to many similar queries, the fields and woods, brooks and ponds became volumes of prolific wealth of interest and knowledge. Stray bottles filled with rain-water into which horse hairs were to become snakes, decorated window sills of his bed-chamber, and literally solved that problem—negatively—forever. Various objects of curiosity were investigated, such as frog spawn, eggs of snakes and tortoise which his plow share had turned up. And from the pugnacious snakelet just emerging from its shell which struck viciously he learned of instinct and heredity, the latter however meaning but little just then but not forgotten in later years.

In college, Dana's geology, Gray's botany and Agassiz and Gould's zoölogy led still further afield, keeping alive, despite Greek and Latin and Philosophy, the love of nature and life. Later he was made aware of Agassiz and Penikese, but the untimely death of the great naturalist ended the work of that earliest marine laboratory for many years. But in 1887 he found his way to a similar laboratory at Martha's Vineyard, and visited the Fish Commission Station and the Marine Biological Laboratory at Woods Hole, all of which opened new vistas of life hitherto wholly unknown. Here he learned at first hand the life history of that rare and erratic frog, *Scaphiopus holbrookii* (American Naturalist, June 1888); and began what became a life-work, the life histories of Hydrozoa.

Such in brief may be said to comprise in outline, including membership in the Indiana Academy of Science, the first chapter of my work as a naturalist, which was fundamentally studies in behaviorism *in nature;* that is, in the haunts and homes of the actors!

A second chapter in behavorism began at the Marine Biological Laboratory, an inspiring factor of which was its first Director, Prof. C. O. Whitman, whose personal friendship and helpfulness is an abiding heritage of example and stimulus, and an insight into the methods of a master workman in this great field.

Here also I came into intimate contact with another of the great students and teachers of behaviorism, Dr. Jacques Loeb, to whom it is a pleasure to acknowledge obligations. It was he who introduced me to new methods of study in behaviorism; namely, so controlling organisms to be studied under rigid artificial conditions as to enable one to critically analyze every factor involved therein and properly evaluate its part. His classic experiments with *Euproctis chrysorrhoea,*

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the caterpillar of the brown-tail moth, I saw and was captivated by their significance and apparent simplicity of control.

Under these stimuli and associations several lines of experimental work were begun and continued for many years, the results of which need not be rehearsed here, literature of the time being easily accessible to those concerned. As some of these papers will show, I found certain of my results differing more or less sharply from Loeb's axiomatic principles, that is, the tropism formulae. An example will make this more specific. According to Loeb:—

"The larvae of *Euproctis chrysorrhoea* creep out of the eggs in autumn, and winter in colonies in a nest on trees or shrubs. The warm spring sun drives them out of the nest and they crawl up on the branches of the tree or shrub to the tip where they find their first food. After having eaten the tips they crawl about until they find new buds or leaves which, in the meantime have come out in great numbers. It is evident this instinct of the caterpillars to crawl upwards as soon as they awake from the winter sleep saves their lives. Were they not guided by such an instinct those that crawl downward would die of starvation. I have found that young caterpillars are oriented by the light. Until they have taken food they are positively heliotropic. This positive heliotropism leads them to the tops of the branches where they find their food. The direction of their movements is determined by the light. * * * Hence the animals are forced as a result of their positive heliotropism to crawl upward until they reach the tip of a branch. They are held there by the light. The chemical stimuli which are transmitted to the animal by the young buds produce the eating movements. In this instinct, which is necessary for the preservation of life, we have another instance of simple, positive heliotropism, and the central nervous system plays only the role of a protoplasmic connection between the skin and the connective tissue, which in plants is performed just as successfully by undifferentiated protoplasm * * * Why does not the light hold them on the highest point permanently? My experiments showed that these caterpillars are only positively heliotropic as long as they remain unfed; after having eaten, they lose their positive heliotropism." (Comp. Physiol. of the Brain, p. 188.)

Interesting as were these experiments, and suggestive as are the interpretations, the writer was not satisfied as to their conclusiveness. Subsequent observation and investigation showed that these larvae do not come to their first feeding in the early spring, for they were hatched in July of the previous summer, and hence lived and fed for several weeks before entering the hibernating stage. In emerging from the hibernating nest which was made during the previous summer they have no need of external stimulus in matter of food, for these nests are always on the tips of branches just where the first leaves appear, and any external tropic urge would of necessity take them away from, rather than to this food. Dr. E. P. Felt states that "The winter is passed by partly grown caterpillars in the peculiar webs on the terminal twigs. They begin work in the spring, feeding downward from the tips of the branches, leaving the naked twigs and the gray apex

at the extremities conspicuous evidence of their presence," (Bulletin New York State Museum, No. 103, 1906, p. 17).

It should not be overlooked that another important fact must be considered; namely, that the eggs of this insect are laid on the under side of leaves in this summer season, from which larvae hatch in immediate contact with the leaves upon which they are to feed, a condition which obviously calls for no special tropic impulse of any sort. Again it must be borne in mind that in the recurring vicissitudes of temperature of early spring there will of necessity be similar recurrences of hibernation and awakening and renewed feeding. But the citation shows that there is no recurring tropic stimulus even though the hunger may be no less urgent than at first.

Loeb's abounding enthusiasm at times led him into serious error of fact, as in the above cases cited. Again, he seems also to confuse definitions, for example, instinct and tropism. "It is evident that the instinct of the caterpillars to crawl upward as soon as they awake from the winter sleep saves their lives. Were they not guided by such an instinct, those that crawl downward would die of starvation." But he has above declared that heliotropism is the all powerful stimulus which prevents this disaster! Either this is a confusion of terms, and hence of meaning, or we shall be compelled to hold that instinct and tropism are but different terms for one and the same thing. But this again would compel a usage entirely different from that usually understood.

For several seasons the writer had especially fortunate opportunity for critical study of two species of caterpillars whose habits were similar to those above described, namely, *Malacosoma americana,* the common tent-caterpillar; and *Malacosoma disstria,* the forest tent-caterpillar. But unlike the former foreign species, these native species deposit the eggs late in the summer or fall and they do not hatch until the following spring. In these the young larvae do not begin at once to feed upon young buds or leaves, but devour the remains of the hardened mucilaginous egg-case within which the embryos have passed the winter. Soon after escape from the egg-capsule the larvae of the common tent-caterpillar weave a delicate silk-web, usually in the crotch of a branch, which serves as a protection, and is enlarged as the inmates grow in size and age. The other species spins no definite web, but spins threads along the branch which they frequent, or on which the eggs were originally laid. Both species may be found together in orchards or in the forest, though seldom found on the same tree or shrub. They differ also, in a further point, namely, that the common tent-caterpillar feeds exclusively during the day, while the forest tent-caterpillar is a night feeder, congregating in masses during the day on the rough bark of the limb or trunk, and at evening faring forth to feed, the operation being distinguishable by the cutting processes of feeding. The striking differences of habit as given show that, though the larvae are very similar in size and markings, they differ diametrically in their habits; one diurnal, the other nocturnal. Is this to be designated as positive and negative phototropism? If so, is it in any sense related to the feeding instinct, or to that of safety from enemies which might prey on them? So far as discernible the answer would be negative in each instance. So also

as to the feeding instinct. There is at no time evidence of the directive action of any external factor whatever. Individuals are found traveling at the same time in opposite directions on the same branch or leaf. I have repeatedly found two individuals, or occasionally several, feeding on the same leaf, but without the slightest aspects of similar orientation; that is, two specimens feeding on a given leaf, the one headed upward, the other exactly the reverse. Members of a given colony which feed during the day may be readily seen traveling in all directions, scattered over a large area of foliage; some will be found greedily feeding, others traveling in search of edible leaves. This is especially noticeable when a tree has suffered heavy defoliation.

Observations upon the nocturnal species is not so easy, though on cloudy and dark days they will be found feeding, but less freely than at night. As in the former group there is no discernible evidence of any orienting tendency. One might expect that if gravity had any influence it would show itself in the position of the specimens in their aggregations on the trunks of the tree during the day; but critical study of hundreds of such colonies failed to show any such evidence. Massed together on the bark and especially the chinks or valleys of such trees as ash, their bodies will generally be more or less parallel, but with heads up or down indiscriminately.

Finally, with the approach of pupation members scatter promiscuously, seeking a fit place at which to spin the cocoon; but aside from the selection of a sheltered place no evidence was noted as to anything like uniformity, or the operation of any distinguishable tropic factor of determination.

In still other studies of behavior, both of insects and vertebrates, similar conclusions have been forced upon me. In all sorts and grades of animals one encounters unmistakeable evidence of autonomy and individuality. Just as no two animals, even of the same species, are exactly alike, so no two of these behave in precisely the same manner. The mechanistic conception of life proceeds upon the assumption that animals are automata, mere puppets, acting only as acted upon by external stimuli. And one may find examples which apparently illustrate aspects of this view; but they are relatively few, and, when subjected to adequate observation and experiment are far from the automata of theory.

This the writer has had occasion to emphasize repeatedly. For example (Jour. Exp. Zool. Vol. 7. p. 184), it is stated: "If we may regard these organisms, not as mere machines, automata, but as individual beings endowed with an organization, both physical and physiological, capable of self-coordination and direction, whether from external or internal stimulation, or from pure spontaneity, then these variable phenomena of behavior are only such as conform to natural expectation. They form integral parts of that living world, from monad to man, whose correlated behavior * * * differs relatively according to the complexity of the organism concerned."

Touching the same general problem as related to widely differing organisms it is stated as follows: "What right has one to assume that the actions of an animal roughly taken from its natural habitat and

as rudely imprisoned in some improvised cage are in any scientific sense an expression of its normal behavior? Is it probable that conclusions drawn from observations made upon an animal in the shallow confines of a finger-bowl, but whose normal habitat has been the open sea, are wholly trustworthy? It is no part of my purpose to discredit the laboratory methods as related to such investigations. They are indispensable. But at the same time let it be recognized that they are at best but artificial makeshifts whose values, unless checked up by constant appeal to nature, must be taken at some discount. This must be especially the case with higher organisms. Some of these may, of course, be readily domesticated, or made more or less at home in aquaria or vivaria, but not a few absolutely fret their lives out, are never at ease, and probably never give expression to a natural reaction under such conditions. Until one has been able to place his specimens under conditions approximating the natural, where in food-taking, health, etc., they are at ease, he has small right to dogmatize as to conclusions, or presume to make such conclusions the basis of so-called *laws of behavior*." (Jour. Animal Behavior, 1912, Vol, 2, p. 51).

As an eminent experimentalist has recently stated: "The properties of protoplasm are too manifold for description. They are those properties whereby living protoplasm acts otherwise then its chemical constituents do. They are as diverse as are the kinds of protoplasm." Again in distinguishing between mechanism and vitalism this author goes on to say: "In addition to vitalism and mechanism there may be a tertium quid, possibly a quartum, or even a quintum quid." (Parker, Science, June 13, 1924).

Another equally eminent authority declares: "As a physiologist I can see no use for the hypothesis that life as a whole is a mechanical process. Mechanical theories have served as temporary working hypotheses round which experimental investigation has centered in physiology. * * * The main outstanding fact is that the mechanistic account of the universe breaks down entirely in connection with the phenomena of life." (J. S. Haldane. Mechanism, Life and Personality.)