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PROBLEMS, METHODS AND RESULTS IN BEHAVIOR[1]

INTRODUCTION

IN every field of endeavor it is from time to time advantageous to pause long enough in the ordinary pursuits of the day to take our bearing, trace the course traveled and adjust plans for the future. I have attempted to do this in the field of behavior and I shall present in brief the result of this attempt.

What I have to offer is in no sense a finished product. It should be looked upon rather as the opening of a discussion, a brief exposition of certain ideas which I hope will be criticized from various points of view.

HISTORICAL REVIEW

Before the renaissance no practical problems in behavior were recognized. All activities in organisms, plants as well as animals, were held to be under the control of souls, agents not amenable to law and not subject to experimental analysis.

Descartes early in the seventeenth century came to the conclusion, partly from the results obtained in observations, partly on the basis of philosophic speculation, "that the bodies of animals and men act wholly like machines and move in accordance with purely mechanical laws." Under the inspiration of this idea, Borelli and others undertook to reduce certain reactions to purely physical and chemical or mechanical principles. Somewhat later Ray, Dodart, Du Hamel and others attempted to account for the movements in plants on the same basis. Thus the science of behavior had its origin, and, strange as it may seem, the fundamental problem before it in its youngest days was to reduce reactions to mechanical principles.

The investigators interested in this en-

[1] An address delivered at the Marine Biological Laboratory, Woods Hole, Mass., July 15, 1918.

deavor were enthusiastic and numerous; so-called mechanical explanations were offered for all sorts of reactions, but these explanations were so extremely simple and crude that they soon came to be looked upon as worthless and the mechanistic concept of behavior fell into disrepute resulting in a period of stagnation.

Not until early in the nineteenth century was interest in the reactions of organism revived. During this period numerous observations and experiments were made on plants, all form a purely mechanistic point of view and very encouraging results were obtained. The work on the behavior of animals consisted, however, almost entirely in superficial observations and the collection of anecdotes, mainly concerning reputed marvelous feats performed. The dog and the fox were favorite subjects, but all sorts of animals were dealt with. We have as a result of this work numerous volumes testifying to the interest in the subject. Menault's "Wonders of Animal Instinct," running through five editions, Jesse's "Anecdotes of Dogs," Swainson's "Habits and Instincts," Cough's "Instincts," etc. These anecdotes are all essentially the same in character. Let me illustrate by quoting one from Menault.

"The following has been related by one of our most eminent naturalists, who heard it from a person worthy of credit:

A young lady was sitting in a room adjoining a poultryyard, where chickens, ducks and geese were disporting themselves. A drake came in, approached the lady, seized the bottom of her dress with his beak, and pulled it vigorously. Feeling startled, she repulsed him with her hand. The bird still persisted. Somewhat astonished, she paid some attention to this unaccountable pantomine, and discovered that the drake wished to drag her out of doors. She got up, he waddled out quickly before her. More and more surprised, she followed him, and he conducted her to the side of a pond where she perceived a duck with its head caught in the opening of the sluice. She hastened to release the poor creature and restored it to the drake, who, by loud quackings and beating of his wings, testified his joy at the deliverance of his companion."

It was generally assumed during this period that animals are endowed with mental faculties similar to those in man and the main incentive in all of this work was the inculcation of humane treatment of animals. The distribution and extent of pleasure and pain in the animal kingdom was the problem of the day. Menault says in his preface: "The marvels of animal intelligence claim now, more than ever, the attention of observers." "We believe that the lower animals possess, in a certain degree, the faculties of man," and Jesse says: "The better the character of the dog is known, the better his treatment is likely to be, and the stronger the sympathy exerted in his behalf."

The "Origin of Species," which as you know appeared in 1859, opened a new field in behavior. Evolution came to dominate every phase of biology, and the evolution of reactions and psychic phenomena came to be the central problem in animal behavior. To the solution of this problem a number of able investigators devoted their energies (Darwin, Lubbock, Bert, Romanes, Preyer, Graber, et al.).

All of these men concluded on the basis of the results obtained that psychic phenomena extend well down in the animal kingdom and some of them even contended that there are indications of such phenomena in plants. Thus they maintained that all organisms are functionally and psychologically interrelated in the same way as they are structurally, and that the mental faculties of man originated in primitive forms.

Whatever view one may take regarding these conclusions, the fact remains that the experimental work of some of the investigators mentioned is of the highest order and the results obtained have been largely confirmed. I should like to refer particularly to Lubbock's ingenious and thorough work on light-reactions in *Daphnia* and color-vision in bees. Critics should always bear in mind that these investigators were interested in the origin and evolution of responses and of psychic phenomena, and not in the mechanics of reactions.

During the latter part of the nineteenth

century students of animal behavior again returned to the problem which dominated behaviorists of the early years of the seventeenth century, namely, the reduction of reactions to mechanical principles. Prominent among these students were Engelmann, Verworn, Loeb and Jennings.

The work of the last three investigators mentioned is in a general way very well known. That of Engelmann, however, seems to have been to a considerable extent overlooked, although it is among the very best that has ever been done in behavior. I should like to refer particularly to his investigations on *Euglena*, published in Pflüger's *Archiv* in 1882, several years before any of the others mentioned began work in this line.

Engelmann finally concluded, after years of searching observations on the relation between physico-chemical phenomena and the reactions in various unicellular forms, that while many of the reactions in these forms are purely mechanical some of them can not be explained without postulating psychic processes. This conclusion may be responsible for the fact that his work has not received the attention that it deserves.

Thus we see that one problem after another has dominated the work in behavior. Reduction of reactions to mechanical principles; distribution of pain and pleasure; the evolution of reactions and psychic phenomena; and again the reduction of reactions to mechanical principles. What has become of these problems? What are the fundamental problems in behavior to-day?

DISTRIBUTION OF PLEASURE AND PAIN

It has often been said that it is impossible to ascertain whether or not animals experience pleasure and pain and that it is consequently useless to attempt to ascertain the distribution of such phenomena in the animal kingdom. In a sense this is true but in this sense it is also true in reference to human beings. Subjective states can be ascertained with certainty by the investigator only as they exist in himself. He can not be certain that your pain is like his pain. All that he can do is to note his actions, including language, during the process of subjective experience, compare these actions with those in other individuals and base his conclusions upon the relation between them.

Precisely the same method is open to him in regard to other organisms, although it is evident that comparison of actions becomes more and more difficult as the difference between the structure of the organisms involved increases. The problem as to the nature and extent of pain and pleasure (feeling or sensation) consequently becomes more and more difficult as one descends in the organic realm.

This problem can, however, not be avoided. The behavior of every individual depends to a large extent upon his conclusion regarding the nature and extent of feelings in the creatures with which he comes in contact. Human society demands a decision of some sort or another regarding the distribution of these phenomena. Witness the work of the anti-vivisection organizations, societies for the prevention of cruelty to animals and charitable institutions everywhere, all built upon and acting upon decisions regarding this matter. The problem then resolves itself into this. Shall we permit human conduct in reference to such an important matter to rest upon judgments based upon evidence casually gained or shall we demand that it rest upon judgments based upon the results obtained in a comprehensive comparative study of the reactions of organisms under experimentally controlled conditions?

Many anti-vivisectionists and members of other anti-organizations who shed copious tears over cats and dogs in our laboratories do not hesitate to sit all day and impale earthworms, crabs and minnows on hooks, and they do not object to the practise in certain tropical regions of turning turtles and cutting steaks from them for a week or more while alive. They assume, of course, that earthworms, crabs, fishes and turtles do not suffer. Are they correct in this assumption or was Brooks correct when, after a lifetime of intimate association with animate beings of all sorts he said: "I try to treat all living things,

plants as well animals, as if they may have some small part of a sensitive life like my own"? Or are those correct who maintain that sensations in all organisms below the upper stratum of human beings are insignificant?

This question can not, at present, be definitely answered and it may never be definitely answered but a comprehensive comparative study of the reactions of organisms bearing directly upon it will unquestionably make it possible to answer it more nearly correctly than can be done to-day.

The field in this line is open. Practically nothing of a thorough going nature has been done in it. Among the best of the works on the lower organisms is that of Norman presented some twenty years ago. Norman showed that the squirming reactions in earthworms to violent stimulation do not constitute conclusive evidence of pain, for the simple reason that when a worm is cut in two the posterior part squirms violently while the anterior part with the brain does not. Reactions in other organisms led him to conclude that there is no satisfactory evidence of pain in any of the invertebrates. But even this work, which, as stated, is among the best, is far from comprehensive and the conclusions are consequently only meagerly supported.

ORIGIN AND EVOLUTION OF REACTIONS AND PSYCHIC PHENOMENA

It has become the fashion among certain ultra-modern psychologists to solve the problem of consciousness by contending that it does not exist. This contention is no doubt largely verbal. The term consciousness is not very specifically defined. It is used loosely by many, and the controversy as to the existence of consciousness is rooted in this fact. What is denied by some is, as I understand it, the existence of an entity capable of action and experience independent of matter. Regarding this I have nothing to say.

Practically every one who is sane, even the modern psychologist, admits that he is aware; he admits that phenomena may have a subjective as well as an objective reference or existence. Whatever else the term consciousness may imply it always implies awareness (subjective experience). As to the actuality of this phenomenon, we are, I believe, more certain than we are about anything else. The origin, the evolution and the nature of awareness, the processes associated with it and its relations to objective reality constitute, in my opinion, the most fundamental problems that confront the human mind, and all available methods of attack should be brought to bear upon them.

The introspection method has been extensively used in the investigation of some of the problems mentioned. This method is, at present, in disrepute and many have abandoned it altogether in favor of the so-called behavior-method. I do not believe that the tendency to entirely abandon introspection is wholesome, although it is of but little importance in reference to the question before us, the origin and evolution of reactions and consciousness, awareness or subjective phenomena. In the investigation of these questions two methods are promising. One might be called the comparative behavior method, the other the method of genetics.

The method of comparative behavior has been and is still being extensively employed. It consists in the comparison under given conditions of reactions in various organisms including man. It is anthropomorphic in its tendencies and owing to this it has been severely criticized both justly and unjustly. This is doubtless due largely, if not entirely, to misapprehensions as to the import of the method.

The method of comparative behavior was used almost exclusively by Lubbock, Graber, Romanes, Darwin and others interested primarily in the evolution of psychic phenomena. These investigators tried to ascertain whether or not this or that animal sees, hears, smells, tastes and feels.

The results obtained led them, as previously stated, to conclude that various animals, besides man, have subjective sensation. And since it was generally assumed that human behavior is, at least to some extent, controlled by subjective states, it was thought that the be-

havior of other animals is also thus controlled. This resulted in the anthropomorphic explanations of reactions current at the time. For example, it was maintained that organisms which are photo-positive go toward the light because they hate darkness or love light, that the moth flies toward the candle-flame to satisfy its curiosity, etc. These explanations have justly been severely criticized, and yet the method is not necessarily at fault.

If human conduct is dependent upon subjective states, and if other animals have such states, is it not altogether probable that their reactions are also dependent upon subjective states? If this is true it is possible to explain in a certain sense reactions in animals on the basis of psychic phenomena. It is maintained, however, that this is putting the cart before the horse, that it consists in attempting to explain the unknown in terms of something still more unknown. With this contention I do not agree, for I hold that every individual knows his subjective sensations better than anything else. The question, then, resolves itself primarily into this. Does conduct depend upon subjective sensations? If it does then it is evident that in the study of behavior it is of the greatest importance to ascertain the distribution of such sensations. But whether conduct is dependent upon subjective phenomena or not, knowledge regarding the distribution of such phenomena is fundamental; for it seems to be the only knowledge that bears upon the problems of the origin and evolution of consciousness.

We judge as to the presence and nature of such phenomena in others almost wholly by comparing their behavior with ours. We know that conscious states in ourselves are accompanied by certain reactions and when we see these reactions in others we conclude that their subjective experience is the same as ours, and by comparing the conclusions thus reached regarding subjective experience throughout the animate kingdom, we formulate conclusions as to the origin and evolution of these phenomena. I realize full well that conclusions based upon such evidence are precarious, but this method is the only method available in the investigations of subjective states in others, and precarious as the conclusions may be they are far more likely to be correct than those formulated without such investigations. We must consequently either abandon this profound problem altogether or proceed along the line indicated.

Aside from its bearing on consciousness the method of comparative behavior has an important bearing on the problems concerning the evolution of reactions themselves and their interrelation, their sequence. In its bearing on this problem comparative behavior is similar to comparative morphology. As comparative morphology yields results concerning the relation between structures in different organisms, so comparative behavior yields results concerning the relation between reactions. It is not primarily concerned in the relation between the environment and the reactions. Its primary interest lies in the relation between the reactions themselves as manifested in various organisms.

In regard to the evolution of reactions, the comparative method in behavior must, however, give way to genetics just as the comparative method in morphology has. In this field we have as yet scarcely made a beginning. It is a virgin field of great promise. I should like to refer to Yerkes's work on mice and McEwen's on Drosophila.

I have pointed out a number of important problems which are dependent for their solution upon the *relations between reactions* and not primarily, if at all, upon the *nature or the mechanics* of the reactions. There are many other problems which can be greatly illuminated by a study of such relations. I shall refer to but one of these, modifiability in behavior including habit formation and learning in general.

Much of the recent work on the behavior of the higher animals centers about this problem, the work of Thorndike, Morgan, Yerkes, Watson, Carr and others. The results of this work have been of inestimable value, practical as well as theoretical, and yet it is based almost entirely upon the relation between reactions. Practically nothing is known regard-

ing the mechanics of the reactions involved, and owing to their extreme complexity little is likely to be known for years to come. I should like to emphasize this point, for there are those who appear to hold that a study in behavior which does not deal with the reduction of reactions to physico-chemical principles has no practical value.

The study of modifiability in behavior should be much extended, especially in the investigation of the lower forms where it has as yet received but little attention, and closely associated with this is the problem of regulation, so clearly set forth by Jennings in the closing chapters of his book on the behavior of lower organisms.

Comparative behavior then, in spite of its anthropomorphic tendencies is valuable in certain lines of investigation, and I hope that what I have said may counteract the strong opposition that has developed against it. However, no matter what may be the immediate object of behaviorists, practically all of them desire to see reactions reduced, as far as possible, to mechanical principles. What has been accomplished in regard to this, and what are the prospects in reference to it?

THE MECHANICS OF REACTIONS

One of the foremost physiologists says in substance: Many reactions have already been reduced to physical and chemical or mechanical principles and all reactions together with all life-phenomena can be thus reduced. Another equally prominent physiologist says: "The attempt to analyze living organisms into physical and chemical mechanisms is probably the most colossal failure in the whole history of science."

How is it that the results obtained by two eminent and practical investigators in the same general field have led them to conclusions so diametrically opposed, the one maintaining that many vital phenomena have been and that all vital phenomena can be reduced to mechanics, the other apparently maintaining that no vital phenomena have been and that no vital phenomena can be thus reduced? The difference in these conclusions is in part, if not entirely, due to different conceptions as to what a reduction to mechanics involves.

Fundamentally all scientific knowledge is the same. It concerns the order of phenomena not the cause of the order. It is rooted in experience and founded upon the conviction that Nature is orderly, that a phenomenon that occurs under a given set of conditions will occur again whenever this set of conditions obtains. All of the scientific laws that have been formulated are merely expressions summarizing the results of experience, and their validity depends upon the extent of the experience. They are in no sense absolute; any and all of them may have to be modified as more experience is gained. To ascertain and to regulate the order of phenomena in nature is the purpose of science.

Mechanics deals with the relation between events or phenomena and changes in the configuration of material systems associated with such events. The reduction of behavior to mechanical principles consists in ascertaining the relation between reactions in animated systems and changes in material configurations within and outside of such systems. In other words, it consists in ascertaining the sequence in series of changes in material configurations ending in reactions. For example, suppose we have an alkaline medium containing paramecia and add a bit of acid, thus inducing avoiding reactions. The substance or material in the alkaline medium has a certain arrangement or configuration. When the acid is added this configuration is changed and this sets up changes in the material configuration within the paramecia which result in a response. That is, we have a series of changes in material configuration ending in a reaction, and similar series of changes precede all reactions.

Now, when the mechanist says that reactions have been reduced to mechanical principles, he probably means merely that some of the changes in material configuration in the series ending in reactions have been ascertained. And when the anti-mechanist says that the attempt to reduce reactions to mechanical principles has been a colossal failure,

he probably means merely that in no such series have all of the changes in material configuration been ascertained. If this is true then both views are doubtless corect; for it can not be disputed that some of the changes in material configuration in series ending in reactions have been discovered in numerous instances, and it can not be demonstrated that all such changes have been discovered in any instance.

Take for example, one of the very simplest, if not the simplest of all responses, changes in ameboid movement. It has been maintained that this reaction is due to changes in surface tension. Similar movements can be induced in inanimate systems. If a bit of potassium bichromate is brought near a drop of mercury in ten per cent. nitric acid the mercury will flow toward the bichromate. This is due to a local reduction in surface tension. This and numerous similar experiments, it is maintained, show that movements in Ameba are due to changes in surface tension. It has, however, recently been demonstrated that changes in surface tension can not produce the force required in certain ameboid reactions. Other factors have consequently been postulated to supply this deficiency. Now this is a perfectly legitimate procedure in scientific investigation. All that I wish to emphasize here is the fact that ameboid movement has not yet been completely reduced to mechanics. Even if it were conclusively demonstrated that every movement and every change in movement in Ameba is directly the result of changes in surface tension, it could still be maintained that the series of changes in material configurations associated with these phenomena is not completely known for such a demonstration would have no bearing upon the problem of the regulation of the movements.

Ameba can move in a homogeneous environment. Consequently, if its movements in such an environment are due to changes in surface tension, such changes are the result of *internal factors* concerning which practically nothing is as yet known. These factors may be purely physical and chemical, but it certainly can not be maintained that it has been demonstrated that they are. For all that is known to the contrary there may be non-material factors, entelechies and psychoids, involved in this regulation. Do not misunderstand me, I do not maintain that there are such factors involved, I merely hold that it has not been demonstrated that such factors are not involved.

In reference to regulation which constitutes the very essence of vital phenomena, we have indeed as yet traveled but a short way on the road toward reduction to mechanical principles, and it is mainly in this region that the anti-mechanist operates.

If we are correct in our analysis thus far, the essential difference between the mechanist and the antimechanist or vitalist is found in the fact that the former maintains that all reactions are completely determined by material configurations, and that all of the changes in such configurations can be ascertained, while the latter maintains that the reactions are not thus completely determined and that the changes in material configurations ending in reactions can be ascertained only in part. Which of these views is correct will be known, if it is ever known, only after every possible sequence associated with reactions has been ascertained. Thus it is evident that the mechanistic and vitalistic programs are, in so far as they pertain to experiment and observation, precisely the same. The mechanist holds that all reactions can be reduced to mechanical principles. Consequently he proceeds to ascertain by experimental methods every possible sequence of phenomena ending in reactions. The vitalists hold that some reactions or certain phases in some reactions can not be reduced to mechanical principles. He also must proceed to ascertain by experimental method every possible sequence of phenomena ending in reactions. For this is the only way he can be certain as to where mechanism breaks down and non-material factors begin to act.

But mechanists frequently maintain that faith in vitalism tends to inhibit experimentation, and that it inculcates superficiality. They maintain that when the vitalist gets into

difficulty he merely calls in some one of his numerous entelechies, psychoids, vital elans and what-nots to settle the matter in place of exerting himself to trace the source of the difficulty to changes in material configuration. There probably is some truth in this contention; but one thing is certain: that is, that not all vitalists are superficial experimentalists and slovenly observers, *e. g.*, Mendel, Müller, De Candolle, Driesch, Haldane.[2] Nor are all who profess to be mechanists proficient investigators.

Personally I do not believe that there is necessarily any essential difference between the mechanistic and the vitalistic doctrines in so far as they may affect investigation in behavior. I do not see how any one, no matter what doctrines he may hold, can fail to believe that some sequence in changes in material configurations ending in reactions have been ascertained, and that more such sequences can be ascertained, and if one believes this and holds that such knowledge is important, he has the same incentive to investigate regardless as to whether he is a mechanist, a vitalist or an agnostic.

The fact that many series of phenomena have in part been ascertained strongly favors the mechanistic view. This doctrine rests on positive experimental results, while the antimechanistic doctrine is founded largely, if not entirely, upon negative results. But there are, nevertheless, some difficulties involved in accepting without limitation the mechanistic doctrine. These difficulties, it seems to me, should be more fully comprehended than they appear to be. I shall, therefore, briefly refer to some of them.

It has often been maintained that the object of all scientific endeavor is the control of nature. While many hold that this statement is too strong, that the control of nature is not the only scientific problem worth while, it is quite generally conceded that it is among the most important problems. If the sequence of natural phenomena is known and if the probable sequence can be predicted, human activities, it is maintained, can be so adjusted as to fit in with the sequence of environmental phenomena, so as to reap pleasure and avoid disaster and, moreover, it is maintained that the sequence can be altered at will, and that nature can be made to obey the commands of man. "Truth makes us free," says Brooks in substance, because it teaches us how to adapt our responses to the order of nature and how to alter the order of nature to meet our demands. Man has harnessed the waters and chained the lightning, he has bridged the oceans and conquered the air. Who can say that he has not gained control over nature? But does this not imply freedom and is freedom not absolutely opposed to mechanism? Are we really free or do we merely think we are free?

Mechanism implies,[3] as previously pointed out, that every phenomenon is specifically associated with changes in the special interrelationship of material particles, masses or systems, changes in or states in material configurations, which are absolutely determined by preceding changes or states in material configuration. Consequently, if mechanism holds, every phenomenon, every act of every organism that ever existed, exists now, or ever will exist, is absolutely determined with reference to character, time and place and has been thus absolutely determined from the very beginning. If you can in reality, at any given instant, move your hand either to the right or to the left, mechanism breaks down, for according to the laws of mechanics, if you move your hand to the right, that movement is by the material configuration within and about you absolutely determined with reference to place, extent, duration and time and you could not possibly have moved it to the left at that time.

In discussing the question of design with Gray, Darwin implies that it is nonesense to believe, that when a swallow snaps up a gnat, it was designed that that particular swallow should snap up that particular gnat at that particular instant. It may be nonsense to be-

[2] Haldane should be classified as an anti-mechanist rather than as a vitalist.

[3] The following statements, of course, apply, in essence, to all deterministic doctrines, antimechanistic as well as mechanistic, as for example Driesch's vitalism.

lieve that all this was designed, but if the doctrine of mechanism holds, and Darwin is usually supposed to have been a mechanist, it has been, by series of material configurations which extend back to the beginning of time, absolutely determined for every swallow that ever existed or ever will exist, precisely which gnats he shall snap up and precisely when he shall snap up each particular gnat. If mechanism holds, chance does not exist in the commonly accepted sense of the term, and the statement so often made that this or that structure or phenomenon originated by chance or the fortuitous concourse of atoms indicates that the import of the mechanistic doctrine is not fully comprehended, for according to this doctrine every movement of every atom, every atomic configuration is absolutely determined by preceding movements or configurations and no other movements or configurations are possible.

If mechanism holds without limit, it is idle nonsense to talk about what might have been. The great calamity that has befallen the world, spreading misery as far as east is from west, threatening to ruin civilization, was scheduled before the world was. And if this is true, is it not sheer folly to hold this or that individual. whose every act is absolutely determined, responsible for the calamity? If mechchanism holds, we are merely cogs in a machine, nothing more, and freedom is a sort of epiphenomenon that exists only in the ethereal realms of philosophical speculation. Do not misunderstand me, I do not maintain that no one is responsible for the war; *far from it!*

I do not know to what extent the mechanistic doctrine is valid; all reaction may possibly be absolutely determined by material configurations; but I do know that I act as though I could, in a measure, regulate the order of phenomena about me, and my actions so as to harmonize with this order in such a way as to receive pleasure and avoid pain and disaster. And I believe that in view of the difficulties involved and in the present state of our knowledge, the general acceptance of this doctrine, without restrictions, is not advisable, because I think that, like all fatalistic doctrines, it would perniciously affect human conduct.[4]

Has then the attempt to reduce animate reactions to mechanical principles been a failure? It has been a failure in the sense that all science has been a failure. The purpose of science is to ascertain and to regulate the order of phenomena in nature; to ascertain in series the sequence of changes in material configurations. In no such series have all the changes yet been ascertained and they probably never will be. We know in part and we prophesy in part. Engelmann proved that sudden reduction in illumination causes reactions in *Euglena,* which result in the formation of dense aggregations in strong light. Lubbock discovered certain definite relations between wave-length of light and reactions in *Daphnia* and various other organisms. Verworn found that various animals orient definitely in electric currents. Loeb showed that photic orientation in Eudendrium bears a certain relation to the quantity of light energy received. Jennings demonstrated that weak acids and various other substances induce reactions in *Paramecium,* resulting in the formation of aggregations by the so-called trial and error method. Parker discovered certain definite relations between the rate of vibration in the surrounding medium and reactions in fishes. All of these responses and innumerable others have been to a certain extent reduced to mechanics, for in all of them series of changes in material configuration ending in reactions have been ascertained.

The attempt to reduce animate responses to physico-chemical principles has resulted in evidence which proves conclusively that a great majority of such responsess if not all of them, are at least in a measure mechanically determined. To ascertain the extent of

[4] Many who read this article will doubtless conclude that the author is a vitalist. Such, however, is not the case. He believes that the evidence at hand does not, as yet, warrant a definite conclusion regarding the extent of the validity of either vitalism or mechanism, and he holds with Huxley that "assertion which outstrips evidence is not only a blunder, but a crime."

this determination is, I believe, one of the foremost, if not the foremost problem that confronts the behaviorist of to-day, regardless as to whether he is vitalistically, mechanistically or agnostically inclined. This problem may never be solved. The extent to which reactions are determined by material configurations will probably never be precisely ascertained. But our knowledge concerning this can certainly be greatly extended. Our observations and experiments have thus far been largely qualitative. Indeed, we have as yet scarcely begun to apply quantitative methods. In this direction there stretches out before us a vast unknown region full of great promises and enticing possibilities.

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GEORGE JENNINGS HINDE

On the eighteenth of last March Dr. George Jennings Hinde, F.R.S., F.G.S., died, at the age of seventy-nine. No doubt in time some friend or associate of his will write a personal tribute to the man and his work; I can but speak as one who knew him through his published researches, and it is to these that I would call attention in this brief appreciation.

Hinde was a man who saw nature through a microscope. His life was devoted to the study of minute organic remains so that, as he said of himself, when other paleontologists went into the field armed with hammer and chisel to collect large specimens, he took with him only a magnifying lens.

In the early seventies Hinde, having completed his preliminary education in England, came to Canada where he spent seven years at the University of Toronto under Professor H. A. Nicholson. During that period his researches were more primarily geological than paleontological, his most important contributions dealing with glacial phenomena in western Canada, the glacial and inter-glacial strata of Scarboro Heights and other localities near Toronto, and the terraces of Lake Ontario. But while studying these broader geological problems he was already turning his attention to the well-nigh invisible contents of the rocks. Near Toronto he discovered in the Ordovicic strata conodonts and annelid jaws, while in the Devonic of Eighteen Mile Creek, near Buffalo, New York, he discovered the now famous Conodont bed. Later he found similar annelid remains in the Siluric of western England and in the Lower Carboniferous rocks of Scotland.

Shortly after his return to England, he set out for what was then the paleontological Mecca of Europe—the University of Munich. Karl A. von Zittel had been called to the chair of paleontology at Munich in the early seventies and in less than a decade his fame as a teacher and original investigator had spread throughout the world. His first major contribution to science was his monograph on fossil sponges which appeared between the years 1878 and 1880. In this he laid the foundations of the science of paleospongiology, for he introduced the method of microscopic study of the spicules and skeletal structure, a method which had previously been deemed of no value for fossil forms although it was used for recent sponges. Furthermore, on the basis of these microscopic observations, he made a new classification of the whole phylum, redefined the old genera and described a large number of new ones, covering in this way the whole field of fossil sponges, and, finally, he gave an excellent series of illustrations of the spicules, a thing which had not been done before.

It was during this very period, when Zittel was annually publishing contributions on the structure and classification of sponges, that Hinde went to Munich where he rather naturally undertook, for his doctorate dissertation, a piece of work along the lines then being pursued so eagerly at that university. He had brought with him from England a small nodule from the Chalk of Horstead, in Norfolk, and this supplied him with material for his thesis, for, although the nodule measured only about a foot in diameter, it was found to contain thirty-eight species of sponges, all represented by spicules. These species, many of them new, were described and figured by Hinde in a paper published in 1880, entitled "Fossil Sponge Spicules from the Upper Chalk."