STUDIES ON THE REACTIONS TO LIGHT INTENSITY PATTERNS OF AMOEBAE OF THE GENUS NAEGLERIA WITH SOME DISCUSSION OF THEIR IMPLICATIONS FOR THE GENERAL PROBLEMS OF ZOO DYNAMICS.

by

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INTRODUCTION

CHAPTER I

A. Nature and Importance of Problem

While the problems of the behavior of organisms with respect to their environment are usually considered to be the domain of psychology, the fact remains that many of their aspects, and certainly their implications, have much significance for the broader field of general zoology.

The distinction would seem to be in the matter of emphasis rather than content. This is especially true regarding the behavior of simple organisms. For psychology, such studies might be of value as a preface to the understanding of the behavior of more complex animals; for zoology, they have significance in so far as they reveal or verify the underlying laws of zoodynamics. It is toward the latter end that this paper shall be directed. Its procedure will be, first to consider briefly the history of explanatory concepts regarding the behavior of simple organisms; secondly, to set forth the procedure and results of experimentation on a simple amoeba in its reaction to an environment controlled within stated limits; thirdly, to evaluate such results in terms of earlier and necessary additional concepts; and fourthly, to point out the implications of the findings for the broader underlying concepts of zoodynamics.
An Historical View of the Concept of Tropisms

To the naive mind life and motion are inseparably linked. That which moves is alive or has the attributes of life. It is only natural that the behavior of living things should have attracted the notice and inspired the speculation of the inquiring mind very early in our civilization. Living things move; plant and animal alike change their position with respect to their surroundings. How and why?

The first question in many cases could be answered by observation and because it probably was, has not held its grip on the interest of students as has the latter question. The history of discovery as to how organisms move, with the possible exception of the simpler forms, is of little interest. But the record of man's attempt to answer the "how" question to the point of its becoming a question of "why" is an absorbing story of the application of logic and

1. The following history was first prepared as a semi-popular report given in seminars before the Departments of Physiology, and later, Zoology, of the University of Kansas. This fact is here noted to explain a certain difference in style from that of the remainder of this paper. Its material was acquired from a number of sources, chiefly those listed as Bibliography no's-1,2,3,4,5,6.
observation to the exploration of a profoundly baffling problem.

Aristotle (384-22 B.C.) assumed that both plants and animals had souls which accounted entirely, so far as he was concerned, for all behavior he and his contemporaries observed. Hence, his problem of "Why?" instead of having to be answered, died at birth and his entire energies could be (and were) turned to the solution of the How?-part of the problem.

Four hundred years after Aristotle, we find Galen (131-200 A.D.) attempting a few experiments more after the manner of modern science. But social pressures and intellectual inertia were great and for nearly 1300 years none followed to build upon the scant foundation laid by Galen.

Early in the 16th century interest in organic behavior again manifested itself and prepared the way for the work of William Harvey. Not only was Harvey's discoveries epochal, but his methodology was that of true science, the inseparable functioning of logic, observation, experiment and reevaluation. A few years following, Borelli conducted extensive experimentation and under
the influences of Cartesian atomism, founded the iatro-mechanical school which looked on all organic functions as that of a simple machine. His point of view has persisted to the present time, in several respects.

The entire 17th and part of the 18th centuries were a period of scattered endeavors, all form in the effort to interpret the behavior of both plant and animal in the simplest of mechanistic terms. Ray (1693) attempted to account for the behavior of the sensitive plant, Mimosa, on the supposition that, normally, its leaves were held in position by liquid pressures in stems and petioles which fell, due to constriction of tubules that occurred when the plant was disturbed. He accounted for the bending of plants toward the light with the idea that the non-illuminated side grew the faster, not because of the light but because the light source (a window) was also a source of chilling air which retarded growth on the exposed side. Sharvoc, a contemporary of Ray, arrived at this conclusion and did much to support this point. Dodart, another contemporary, came to the conclusion that such plant bending was due to a difference in moisture on the exposed and shaded sides. Du Hamel, who during the same period was carrying on experiments with plants and their reaction to
light, temperature and moisture concluded that the actual bending resulted from the effect of light on the moisture in the stalks and that, within the cells of the plant, gases were generated which actually accounted for the energy manifest by the bending. (Staff Richten der Dampfe"

Despite the popularity of such mechanistic explanations as the foregoing even well into the 18th century, their inadequacy was becoming increasingly appreciated. The consequence was a swing toward a vitalistic interpretation of all life processes especially those having to do with the total reactions of organisms. The extent of vitalistic emphasis varied from those who held that the so-called "inner force" was utterly inscrutable to others among whom was the German physiologist, Johonnes Muller and the French Botanist, De Candolle, who merely held that such inner force was material for further analysis.

Immediately following and building upon the work and views of Muller and De Candolle were a line of researchers Wohler, Liebig, Helmboltz, du Bois Raymond, Lotze, Weber, Fechner and many others whose work and interpretations slowly swung the weight of conviction and attack back toward the period of mechanism in which the 19th century was to close. But before passing on to the influence of Darwin and to the
latter decades of the century it may be well to speak a bit further of De Candolle and his ideas. His work was largely on the behavior of plants. He reversed the periodic sleep movements of the leaves of plants by keeping them darkened by day and illuminated artificially by night. In his studies on the turning of plants toward the light he used for the first time a term which has abounded in the nomenclature of behavior ever since. The turning of a plant toward the light he spoke of heliotropism. In using this word he referred to the bending with respect to the sun's energy.

Now, note three things about this term as originally used by De Candolle: (1) He used it to indicate the exciting agency, sunlight; (2) insofar as it referred to the plant it had to do only with its act of bending, a readily observable fact; and (3) in no sense was it advanced as an explanation for the fact of behavior. These points are especially significant in view of the fact that De Candolle, himself, was a vitalist.

Following the first use of the term tropism by De Candolle in 1836, we find it used frequently by others in a variety of ways and with an interesting series of departures from its original meaning. Hoffmeister, 1863, suggested the terms positive and negative tropism; Frank (170)
invented the term geotropism. The researches of Knight '73 on the direction of growth of roots of plants led him to feel that their direction was the direct result of gravity upon the cells concerned in much the same way that De Candolle had explained the light response in terms of the difference in the direct effect of light. In 1876 Sachs, in the preface to a paper by Herman Muller first published his views on the response of organisms to light, views of which were later expanded and published in a volume in 1882. He pointed out the fallacy of De Candolle's view by showing that all plants are not positive to light and yet presumably have the same phenomenon of growth retardation due to light. He further pointed out that geotropic action like that of light seems more one determined by direction than by actual intensity. He advanced a view that has since become known as the "ray direction theory" which, briefly, advocated that what actually determines the direction of movement of the plant is the direction at which the rays of light pass through the tissues of the plant. Now let us stop here for a moment and note already a change in emphasis. The concept of tropism which to De Candolle was a descriptive one concerning a source of stimulation and to his immediate followers was descriptive of the simple
relation of such stimulation, if applied now in terms of the concept of Sachs must concern a stimulation in its relation to the inner tissues of the plant, which tissues must be assumed to be so organized that the fact of direction of a light ray may be transformed into an actual motion of a plant in an appropriate direction. Already the concept is becoming complex and so far we have followed its application only to sessile organisms whose maximum movement is but a bending from their place of anchorage or a direction of growth.

During the time in which we have concerned ourselves with the light-plant-gravity problem, a number of workers were studying the reactions of motile organisms to light. As early as 1860, Nageli showed that certain flagellates and ciliates tend to collect on the side of a vessel nearest a source of light even if the edge of the vessel casts a shadow so that the side of the surface of the liquid nearest a source of light even if the edge of the vessel casts a shadow so that the side of the surface of the liquid nearest the light source is actually least illuminated. It was further shown that this was not a matter of the organisms avoiding the light for the same thing occurs when the vessel is full and no shadow is cast. In 1866, twelve years before the first publication of Sachs, Cohn met this dilemma by suggesting that the organisms were reacting with respect to the direction of the rays of light
passing through them.

During the years from 1878 to 1883 Strassburger and Engelmann in studying aggregations of organisms found that, in even motile spores, a sudden change in illumination has a profound effect upon behavior. The former suggested that one explanation of aggregation of organisms in light areas rested in this fact. He assumed that organisms wandered into light areas by chance and were trapped there by their reaction to the change of intensity which occurred when they attempted to leave. We need not point the fallacy of such view, resting in the fact that if such organisms react to changes of intensity and if the idea of Strassberger were correct the organisms would never have entered the light area in the first place. But leaving this and other inadequacies of his explanation, we must not omit the actual contribution of his work. Organisms not only react to intensity, to differences of intensity and to the direction of illumination but also to gradients of intensity in point of time. This fact was carried further and more of its true complexity pointed out by E. S. Jennings some 30 years later in his studies on what he called "avoidance reactions." But we are not yet ready to consider the contributions of Jennings.

Now a new involvement must be incorporated into the
simple concept of heliotropism, the factor of time or the
duration of intensity change. Englemann showed that it was
not the degree of intensity change which provoked reaction
but the rapidity with which such change occurred. He show-
ed that the same degree of change which provoked pronounced
reaction if rapid, produced no evident response if the change
were gradual. Thus the simplest organism reacts to the fourth
dimension. What wonder that some one has not advanced a
"chronotropism" to explain this fact. Fortunately, they
haven't!

In 1880 a significant work by Charles Darwin and his
son, Francis Darwin, entitled the "Power of Movement in Plants"
advanced a new aspect of the problem. They showed that (1)
there is movement within plant cells that are apparently not
stimulated from any outward source, the so-called "streaming
movements" and (2) that plumules of plants with either tips
or sides either shielded or even destroyed can and do re-
act to light quite as do normal structures. These findings
would seem to have much bearing upon the theories of inten-
sity difference and ray direction but to a large degree were
not noticed or quoted by succeeding investigation for many
years. Nevertheless they do complicate the problem of be-
havior and the meaning of the term tropism.
In 1892 Olmstead attempted to settle the ray direction intensity problem by studying the reaction of colonies of Volvox to light which had been passed through a hollow prism filled with India ink and a gelatin mixture and found that colonies chose a region at neither extreme. The region so chosen he arbitrarily called their optimum intensity. There is no record of his having varied the intensity of the source to see whether or not there would be a compassating shift of position of the organisms. Had this been done, the result might have saved much of the confusion which follows and which still shrouds the term today.

The published work of Davenport in 1897, covering several years of careful experiment and culling of the works of others, favored both the direct influence and the ray direction views. For these he coined the terms phototaxis: movement with respect to ray direction, and photopathy: movement toward or away from regions of greater or less intensity. He later complicates his position, however, by speaking of the earthworm as negatively phototactic. Obviously phototaxis according to his own definition could be neither positive nor negative since an organism if reacting in the direction of light rays might be traveling in either of two directions and still meet the requirement of his definition. This point seems never met in subsequent
writings although he does reiterate in a number of places his belief that reactions may be of either type.

In 1887 Jacques Loeb began his researches and publications in Wurzburg in a time and influence strongly pervaded by the work and views of Sachs. Perhaps no one published more nor was more influential in shaping the thought of those last decades of the 1800's than was Loeb. He began his work to the avowed end of explaining simple organic behavior in the terms of equally simple mechanistic principles. To this end he held religiously despite repeated illustrations of the inadequacy of his explanations and the fallacies of his logic. He began as a stout supporter of the ray direction theory but later, without any consciousness on his part, seemingly, of a shift in camp, devoted his papers to proving that organisms orient entirely by the difference of light intensity on opposite sides of their longitudinal axes. His view was always that of simple, inflexible automatism. Seemingly he refused to see any order of response that did not yield itself to such explanation. He did little to either advance or complicate the tropistic concept. He used the term in several different senses during his productive years, chiefly lending to it an entirely mechanistic meaning.

The publication by Radl, in 1903, of his work on the
reactions of insects and crustaceas to light has an interesting point to be here considered. Radl favored the direct-effect-of-light view but unlike its many supporters preceding him, he did not share the view that differences of intensity on different sides of the organism set up differences in chemical activity which accounted for both movement and its direction. Radl postulated an even simpler explanation. It was known then that light striking an object actually exerts a pressure on the illuminated surface. Radl suggested that this pressure was the force which explained orientation; that an organism, whether reacting positively or negatively to a light source, turned until such pressure was equal and then moved in the appropriate direction either toward or away from such source. The first objection to such view of course lies in the fact of the very slight pressure that would be exerted by the normal illumination in habitat conditions. But a far more significant fact lies in this view of Radl's. If phototropism can be so explained and the fact of orientation can be accounted for on the basis of this assumed pressure, the tropism ceases to be the problem of turning and becomes the far graver problem of explaining the movement toward or away from the source. Thus the concept ceases to be the observable act of turning and becomes the explanation for the true dynamic which produces
movement in a direction, once that direction has been determined by equalization of pressure. Thus the concept of tropism becomes very different from the simple description of behavior first so named by De Candolle.

A series of observations, begun in 1897 and discussed in numerous papers by H. S. Jennings were brought together, edited and expanded and published in 1906 in the well known volume entitled "Behavior of Lower Organisms". This is by far the most thorough and understanding work ever produced by one observer up to that time or for that matter even to the present. Jennings observations on orientations and on the formation of aggregations of ciliates and flagellates, especially, were carried on with a patience of dispassion that yielded results unlike any before him, with the possible exception of those of Engleman. Organisms as individuals were Jennings special study. He observed what they did in its minutest detail, free from any preconceived system of explanation. His accumulated data lead to two conclusions of importance to this paper: He showed (1) that the simplest acts of the simplest organisms cannot be adequately explained by the simple mechanistic systems so popular with many of his predecessors. He showed repeatedly that in the most carefully controlled situation any one of several responses by the organism might occur
the selection of the response not being a result of the external situation but, seemingly of the organization of the individual in relation to his situation. (2) Jennings showed that organisms constantly behave in a wide variety of ways when wholly unstimulated in an experimental sense. Behavior, he showed, to be a dynamic ongoing thing in no way seemingly initiated by the experimental situation and only controlled or modified by it within a narrow and often unpredictable extent. His work to a large extent made the concept of tropisms meaningless except as a descriptive term or as a sort of "blanket term" to include, as he supposed, all the uncontrolled "inner factors" which find expression in the simplest response. His work not only yielded much actual information regarding behavior but placed the whole problem on a plane where the concept of tropisms must either be used so simply as a description or to be useless or so complexly as to become almost vitalistic in its emphasis.

Since the work of Jennings, research in the field of protozoan behavior has followed largely two general trends: the first, a relation to mechanistic analyses, the second and more recent, the so-called organismic or holistic approach. The first, the mechanistic approach, has been the more prolific from an experimental angle but practically lacking in
originality with regard to the evaluation of either their own or their predecessors' work. It is easy to see why this direction of emphasis has been most active. It is but a continuation of the systems of thinking of past workers. Analysis is always easier than synthesis. Analytic thought in chemistry antedated synthetic thought by 50 to 100 years; it is only natural that the same should be true of studies in the field of behavior.

Following Jennings' publication little significance was done in the general field of the behavior of animals until a series of papers by S. O. Mast which were compiled and edited in a volume published in 1911 entitled "Light and the Behavior of Organisms." While very original and one of the most thorough works ever published in English on the subject, it shows plainly the influence of the work and thought of Jennings. In fact the work is chiefly a review of the subject and of its trends which lead up to the conclusion of Jennings. Excellent as the work is, the fact remains that Mast makes in it (nor since, either, for that matter) little contribution not to be found in the earlier work of Jennings. It is interesting further to note that Mast from that time to the present has become more and more analytic in his efforts and likewise more and more mechanistic. His students, in large degree, have accomplished about as much and in the same direction with one
possible exception of which we shall speak shortly.

In speaking of recent trends in this field, some work must be noted which lies, in a sense, midway between these two lines of emphasis. For a number of years now Chapman and his students have been carrying on ecological research upon the effects of what may be called multiple stimuli upon insects. They have amassed much evidence to show that insects react very differently to mosaics of stimuli, when such stimuli are controlled, from what is the case when a single stimulus is studied and all others are either neglected or excluded.

Von Frisch of Munich in his studies of the sense discrimination of bees has contributed a vast number of data to our appreciation of the actual complexity of organic behavior.

From the Zoological and Physiological Laboratory of Harvard in recent years have come several significant contributions though in no sense evaluative. The extensive work of Cozier on the effect of gravitation and electrical currents on organisms is worthy of note even in a paper primarily devoted to light responses. Castle of the same institution has carried on investigations for the past 10 years on the reactions to light of the sporangiophores of the fungus, Phycomyces, and out of it all has brought forth a mechanistic interpretation quite worthy of the 16th century! Walsh, of the Zoological
laboratory of Harvard has done much physiologically interesting work on the stimulating effect of light as shown by muscular contractions in certain of the Entomostraca. Also might be mentioned the work on Cambarus by Knapp and Enzmann of that institution, their effort being chiefly to show the bilateral synchronization existing under conditions of unequal bilateral illumination. Clark and Wolf have recently conducted some interesting studies on "Tropistic reversals" by the effect of drugs. Since drugs can scarcely be considered as affecting the sources of stimulation and must be assumed to bring their changes deep within the organism, it seems plain in conclusion, of this part of our paper, that the modern, mechanistic, analytical approach is not clarifying nor simplifying the problem of tropisms.

And now a few words in conclusion as to the second approach, the organismic view of the problem. It is true that little work has yet been done on interpreting the behavior of simple organisms in terms of the organismic point of view. Its progress has been chiefly in the explanations of the behavior of the higher vertebrates. The response of organisms to patterns of stimuli have been carried on down to the level of the fishes by Perkins. Clark, working at J. H. U. under Mast has results on the reactions of the Gyrinid beetle, Dinentis, that can be
explained in no other way. It is also true that much of the confusion in the results of past experimenters ceases to exist when their data are considered from this more recent point of view. Just as it, as a point of view, is doing much to clarify the problems of histology in the writings of Dontchokof, Studnicka and others; those of cytology in the works of Carrel, Sharp and others and has in embryology since the days of C. O. Whitman and is in the neurology of present in the works of Coghill and a host of others, so as a concluding hope, it may be said, will the problem of behavior of simple organisms find meaning in this point of view. Then and only then will the tropism return to its original, descriptive meaning.
CHAPTER I

C. The experimental approach defined with regard to aims and limitations.

One thing seems certain from the preceding history of the tropistic concept: it has arisen out of observations on behavior and the attempt to first describe and, later, to explain them.

Therefore it may be well to consider briefly the assumptions that seem to underlie the concept and to see, whether or not the evident limitations of it may not arise from the very nature of those assumptions.

The most obvious thing about an organism is its discreteness from its environment. Quite as apparent however is the fact that its behavior is not independent of environmental changes. These two aspects of observation and later of experimentation contain the problem of behavior, and the question to be answered becomes: How do environmental forces bring about observable behavior?

Having accepted the view that the organism is discrete and that the environmental forces do modify its behavior, the next logical step has been to assume that such forces impinge upon the organism in a physical sense. Thus its behavior becomes the results of such physical factors of
the environment as light, temperature, etc. Assuming further the validity of these assumptions it seems reasonable that a quantitative factor should enter; that a given physical influence of determined scalar magnitude should produce a definite extent of behavior change is difficult to measure in a scalar-sense and from this fact has arisen through experimentation the concepts, first, of threshold values, the quantity above which a stimulus must mount before any behavior change becomes apparent, and, secondly, optima of stimulation, the stimulation at which the most normal, natural behavior of the organism seems to be carried on, that intensity, which the organism seeks when otherwise uninfluenced.

Having gone this far successfully, a final problem arises. How do simple physical forces in their impingement upon an assumedly discrete organism bring about the complex, observable facts of behavior? From the fact that the forces of the environment can be analysed and controlled. The factors in question must be assumed to be within the organism. What, then, is the nature of these "inner factors," this "inner something," and how can they, or "it," be approached. Obviously we are at the parting of the ways of the mechanist and vitalist. We do not need follow, further, either one
of them. Rather it is important here to see that the problems of both arise out of common assumptions. Let us focus our attention on what might seem to be a series of dilemmas created by those assumptions themselves.

First, why assume the organism as discrete from its environment? Obviously the assumption rests upon the empiricism of observation. Yet the intimate metabolic dependence of the organism upon the environment has long been known and, it would seem, should have led to the questioning of such assumption regarding its total behavior. But another thing must be considered. Such assumption allows for the entrance of a purely mechanistic explanation of behavior in terms of physical forces. Here is an supposedly discrete organism, here are physical forces playing upon it; hence the behavior must be explicable in terms of such forces. The whole problem, then, should be the relatively simple one of experimental analysis. And to this end has proceeded the bulk of investigation. That much of the behavior of even the simplest forms, does not yield itself to such explanation has only stimulated further analysis without a "right-about-face" questioning of the first assumptions.

2. For modern discussion of this intimate dependence see Bibliography no. 8.
And so the problem stands today, with at least three logical dilemmas inherent in the very assumptions upon which it rests. First, how can that which is assumed to be unrelated be related logically? The mechanistic answer is: That unrelatedness is bridged by the stimulus of external physical forces to which the organism is sensitive. Very well. That answer merely raises a second question: How account in terms of physical forces for the complexity of behavioral response and for the fact of sensitivity itself? Here, as we have already seen, the vitalist and mechanist part company and since the latter position is the only one which leaves the problem open for further experimental answer we shall follow only his approach. And from that approach much work has been done. But none of it has thrown much light upon either the question of the nature of sensitivity or the increasingly larger question of those responses in organisms in which (1st) choice of response occurs in experimentally identical conditions: (2nd) in cases of progressively modified behavior to identical situations: learning4; thirdly organisms seem to react to relations or patterns of stimuli rather than to definite scalar values,

of such intensities.

The aim of this paper was first to throw light upon the last of these observations (e.g., reaction to patterns) but it seems now that the answers to the first two cases (learning and choice) may be of exactly the same order.

In April, 1932, while observing the reactions of large amoebae (Chaos diffluens) to the boundaries of strongly illuminated areas in fields of lesser intensity it was noticed that, after periods of 30 minutes to one hour, in undisturbed fields organisms tended to seek regions of medium rather than least intensity. The same was found to hold true when the actual intensity of such areas was varied over a considerable range by changing the distance of the light from the microscope. Such behavior was observed repeatedly, but rarely upon more than one organism at any one time. These observations seemed explicable only in terms of the organisms reacting to patterns of intensity rather than to one of scalar magnitude. Several questions grew out of these chance observations: (1) Would several organisms react similarly to the same situation if they could be placed in it simultaneously? (2) Could pattern be so arranged as to make possible a prediction of the nature of such reactions? and (3) Could such reactions be brought about, to sudden change in values of
patterns, so as to eliminate the possibility that these reactions were either chance observations of atypical behavior or that the cases observed were actually those of an organism becoming adjusted to an intensity to such point as to account for its remaining in a certain zone rather than experience the change involved in passing to one of greater or lesser intensity? These questions seemed possible of answer by experiment.
CHAPTER I

D. Equipment and procedure.

From the foregoing observations two things seemed obvious; the amoeba was an ideal organism to use, not only because observation of it first stimulated the idea of studying the reactions of a simple animal to environmental patterns, but because of its slow rate of movement and its position of simplicity in the evolutionary scale. Further it seemed that light as a source of stimulation was an ideal medium since it might be used in simple "setups" could be readily controlled, and was an almost universal factor in environmental situations. The further fact that so far as known amoebae have evolved no special structure for the reception of light stimulation seemed to recommend this combination of organism and environmental influence for such experimentation.

The organism chosen, Naegleria histodiabia Colkins, seemed ideal for several reasons. It is doubtless the simplest of the naked lobase Rhizopoda in an evolutionary sense. Its small size 20 ms. approximately in length.

5. See the excellent reorganization of the lobase Rhizopoda by Shaeffer. Bibliography No. 10.
in its usual trophic form, made possible the observation of
a number in any one field of a compound microscope at a
magnification of 100 diameters. A simple method of culturing
it in large numbers was found and has been elsewhere de-
cribed. It was further recommended for such study by the
fact that very little seems to be known about it and the
possibility that working with it in this experimental way
might bring forth other information concerning it. (See
above reference for other observations on Naegleria bistodialis
that have no bearing on the paper and cannot be included here)

The equipment involved in securing the data of this
paper included the following:

Microscope: Spencer Research No. 3 with interchangeable
binocular and monocular tubes, quadruple nosepiece bearing
16 mm. and 4 mm. dry objectives, 7 mm. water immersion and a
1.6 mm. Na 1.30, apochromatic oil immersion objectives.
A Spencer Research Mechanical Stage #492 with double concentric
control buttons was used bearing the same slide in all exper-
iments, a 52 x 76 mm. glass slide of low refractory index. The
substage used was the standard Spencer adjustable bearing an
Abea achromatic condenser, N. A. 1.40.

6. Bibliography No. 11.
The light source was an E. Leitz Wetzlar, Mignon Type, substage lamp equipped with 11.5 volt, 6 amp. Mazda bulb and a Jefferson step-down, double secondary, transformer giving a reduction of the 110 volt main current to either 6 or 12 volts. The lens in the lamp was of a converging type with a focal length of about 22 mm. A removable flanged sleeve made possible the attachment of a diaphragm cut from 12 pound white bond paper of a good uniform grade which the patterns, to be later discussed, were formed by additional layers of the same paper. No ink, crayon or other light absorbing medium was used in making any of these diaphragms.

To the monocular tube, carrying a 10x ocular, was attached a Bausch and Lomb, no. 1620, Abbe camera lucida, through which all recording of position of organisms was made upon sheets of white paper upon which the field of the scope through the camera lucida had been previously prepared.

All light entering the field was passed through a water cell of 5 mm. thickness, internal dimension, which was free from air spaces and filled with triple distilled water. A blackened cardboard funnel attached to the objective was inverted over the culture mount during all experimentation to exclude all light from entering through the controlled source.
Room temperatures were recorded for all readings and varied from 68 degrees F. to 96 degrees F. The room was kept in absolute darkness during all experiments except for the light of a small substage lamp directed upon the paper upon which the position tracings were made. A rheostat in the lamp circuit made possible the adjustment of its intensity to that of the microscope field. A shield was used to screen all reflected rays from this lamp or from the paper receiving the tracings.

The procedure of experimentation was as follows: A drop of culture was placed on the slide and illuminated by the experimental light with the condenser in a lowered position so that the field was homogeneously lighted. With the binocular tubes the drop was studied for regions best adapted by concentrations of organisms for experimental purposes. When this was decided a quick change was made to the monocular tube with its attached camera lucida, and final adjustments of the paper and the auxiliary light were made to receive the tracings. Then the condenser was raised to such a point as to focus exactly the diaphragm pattern of the experimental light upon the upper surface of the slide.

In the 2½ minute period following final adjustment of the auxiliary light on the paper was made and at its close a tracing of the position of all organisms in the field was
made. At successive 5 minute periods such tracings were repeated throughout the time of experimentation. Three shades of pencil, black, red and blue were used for successive tracings to simplify the later studies of these tracings especially where outlines of organisms overlapped. For the same purpose all outlines were numbered successively in the pencil color with which they were drawn at the time of their drawing. A slight modification of this procedure was necessary in securing the first section of the data hereafter presented and will be noted in its introduction.

Careful records were kept of incidental conditions during experimentation as well as those of interesting aspects of organisms, behavior, etc.

A micro-pipette and rubber tubing were attached to the 'scope stage and permitted the addition of culture water at intervals as needed to prevent dying. This method was found to supply water with a minimum of disturbance of the experimental field under observation.
CHAPTER II

PRESENTATION and EXPLANATION of DATA

A. Reactions to "Gradated" Fields.

B. Reactions to "Barrier" Fields.

C. Reactions to Multiple "Barriers."
CHAPTER II

A. 2. Recorded results.

The experimental situation necessary seemed to be a method of dividing the microscope field into three bands of intensity, bright, medium and dim. A variation in this might be accomplished by a change in the intensity of the light. However it was decided that a simpler and more effective method might be the addition of a fourth intensity which might be used with two of the other intensity bands. This was done.

A diaphragm for the experimental lamp was made with four bands of intensity by increasing the number of layers of paper from one to four across the diaphragm. When focused on the field of the microscope, this resulted in four intensity areas: very bright, bright, dim, and very dim. The light was so placed that the field at any time included only three of these, the fourth being added by a movement of the mirror which removed the extreme band on either side and brought on the other extreme. For example, the field, let us say, includes the bright, dim and very dim areas. By a slight movement of the mirror the very dim area is removed and the very bright area brought on at the opposite side so that the field, after the change, contains the very
bright, bright and dim areas. Obviously, by this change in either direction, what was the neutral or medium intensity, at one time, becomes one of the extremes after the change.

The position of the lamp remained the same for all experiments although both the 6 volt and 12 volt current was used. The data also include cases where the culture was shifted with the light, forcing the organisms to change position on the slide to attain the region of neutral intensity and also cases where only the intensities were changed.

The procedure was to place the culture on the striated field as previously explained. After a period of $2\frac{1}{2}$ minutes a reading (count) was made of their position and number. After the interval of $2\frac{1}{2}$ minutes the field was shifted and 2$\frac{1}{2}$ minutes later another reading was made; and so on throughout the period of the experiment. The experiment was usually so timed that the readings fell on the even 5 minute periods and the change of field on the $2\frac{1}{2}$ minute periods intervening.

The tables which follow show the record of these readings, as well as the other information concerning them.
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**Comments**

- One lost from V, D, zone just before change of field.
- H2O added.
- One enters from outer darkness to D, zone.
- One temporarily lost to enter again at 11:59.
- Addition to V, D, zone probably due to marginal which caused cessation of observation 5 min. later.
- H2O added.
- Organism had remained 12 min. in field before 1st. reading was taken.
- N. bistiodialis.
- Two organisms move against gradient.
- One organism moves with gradient.
- One entered moving rapidly through V, B. zone.
- Perfect transposition.
- Perfect transposition.
- One remains in B. zone.
- H2O added.
- Two lost by movement from field.
- Perfect transposition.
- One enters D. zone at 90 degree angle.
- H2O added.
- Shift made by 2 organisms.
- No transposition.
- One moves against gradient.
- Two organisms transpose.
- One lost from D. zone.
- Perfect transposition.
- Perfect transposition.
- Two enter due to marginal drying of slide.
- Shift slight due to drying which closed observations.

**4/27/34**

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**4/30/34**

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- Observations on N. bistiodialis.
- Organisms remained 7 min. before 1st. observation was recorded.
- Perfect transposition.
- Perfect transposition again.

- Observations on N. bistiodialis.
- Organisms remained 7 min. before 1st. observation was recorded.
- Perfect transposition.
- Perfect transposition again.
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5/2/34

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Comments

- Two remain in D. zone.
- Perfect transposition.
- Perfect transposition of 4.
- 2 organisms lost from V.B. zone.
- Perfect transposition again.
- H2O added.
- Only 3 change position against gradient.
- Only 3 change position with gradient.
- H2O added.
- Perfect transposition
- Perfect transposition of 4.
- 1 lost at 90 degree angle from field.
- One lost (?) return at lowest point of gradient from dark.
- One enters D. zone just before time of reading.
- H2O added.
- One lost from D. zone at 90 degree angle.
- Perfect transition
- Perfect transition again.
- One entered D. zone due perhaps to marginal drying since drying had occurred so as to spoil culture for further observation.
- A culture of N. bistoldialis was placed and allowed to remain for eight minutes before reading at 8:05 was made. Culture very rich. All but 2 made a perfect transposition. 2 moving toward D. zone. Note one in V.B. zone.
- One lost out D. zone.
- H2O added.
- Perfect concentration at center of gradient.
- Perfect transposition (Largest number ever observed to make perfect transposition)
- All but one make perfect adjustment.
- H2O added before observation might account for loss of one.
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5/3/34
8:45

Six organisms were placed in striated field for 9 min. before first reading was made at 8:45. N. bistodiulis.
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Addition of 2 organisms probably due to marginal drying. H₂O added at 9:12

Perfect concentration at center of gradient.

Perfect transposition.

Two organisms lost from D. zone at 9:27

Perfect concentration again.

Perfect transposition.

Two organisms entered V.D. zone at 10:23. Marginal drying (?) Culture was disturbed when H₂O was added at 10:31. Note that an organism found its way to V.D. zone only once in 1 hr. 45 min.

One organism enters D. zone at 10:08

Two organisms enter V.D. zone at 10:23. Marginal drying (?) Culture was disturbed when H₂O was added at 10:31. Note that an organism found its way to V.D. zone only once in 1 hr. 45 min.

Culture of N. bistodialis was placed on striated field for 7 minutes before first reading at 7:25

H₂O added at 7:46.

Perfect transposition.

Perfect transposition again. The first time a perfect transposition has occurred 3 times successively.

One organism enters D. zone from dark.

Two more enter V.D. zone probably due to marginal drying. H₂O added at 8:21

Loss of one organism at 90 degree angle from D. zone at 8:29
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Three enter D. zone (Drying ?) \(\text{H}_2\text{O}\) added at 8:51

Transposition fails. Organisms moving very slowly

Poor concentration. One organism lost from V.D. zone at 9:16

Marginal drying caused increase of organisms in field and terminated series of observations.

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Culture of \(\text{H. histodiialis}\) was placed on striated field at 7:52

Perfect transposition though not wholly centered on gradient.

Perfect centering and transposition.

\(\text{H}_2\text{O}\) added.

Perfect transposition. One organism lost from V.D. zone.

One organism lost from D. zone at 8:48

Perfect transposition

Perfect transposition again.

Addition of 3 to V.D. zone due to marginal drying.

Time lost due to excessive addition of \(\text{H}_2\text{O}\) and loss of concentration of organisms on striated field.

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One organism lost from V.D. zone at 10:02
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5/9/34

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Comments

H₂O added 10:11
One organism lost from V.D. zone at 10:16
One organism enters from V.D. zone at 10:23

Two organisms enter D. zone at 10:34 (Marginal drying?)
H₂O added at 10:41. Two in V.D. zone moving out at 90 degree angle.
Two organisms lost through disturbance of H₂O addition.

Set up disturbed by accident and observation concluded at 10:57.

A culture of N. bistiodialis was placed in striated field at 9:48.
Perfect transposition.
Perfect transposition.
Perfect transposition.
One organism enters from dark to V.D. zone.

Two organisms enter V.D. zone. Marginal drying. H₂O added at 10:32.
One organism moves out of V.D. zone as H₂O is added at 10:32.
Perfect transposition.
One organism moves out of D. zone at 10:43.

Perfect transposition.
Perfect transposition.
Perfect transposition.

Note that eight perfect transpositions occurred in 1 hr. 10 min. of observation. Great activity of organisms due perhaps to ideal room temp. and fact of food depletion of culture. See discussion.
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<td>Perfect transposition from all 3 zones.</td>
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<p>| 13/34 | 8:05 | 1 | 2 | 0 | 3 | A small culture of <em>N. bistocialis</em> was placed on striated field at 7:58. Temp of room high. Motility poor. |
| 8:10 | 0 | 3 | 0 | 3 | Failure to adjust to gradient attributed to sluggishness. |
| 8:15 | 1 | 2 | 1 | 4 | One enters V.D. zone at 8:18. Movement slow. |
| 8:20 | 0 | 3 | 0 | 3 | Poor distribution. H₂O added at 8:31. |
| 8:25 | 1 | 2 | 1 | 4 | Little or no movement since last reading. |
| 8:30 | 0 | 2 | 2 | 4 | |
| 8:35 | 2 | 2 | 4 | |</p>
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A culture of *N. bistodialis* was placed on the striated field at 7:07.

Perfect transposition.

Perfect transposition.

One organism enters V.D. zone at 7:54.

One organism enters D. zone at 7:59. Marginal drying.

H₂O added at 8:06.

One organism left V.D. zone at 8:13.

Perfect centering and transposition.

Perfect transposition from all 3 zones.

Two organisms enter D. zone at 8:48.

One organism enters V.D. zone at 8:53. Marginal drying.

H₂O added at 8:58. Two organisms leave D. zone at 8:59.

One organism lost from V.D. zone at 9:14. Room temp. becoming high enough to cause sluggishness. Observations concluded.

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A small culture of *N. bistodialis* was placed on striated field at 7:58. Temp. of room high. Motility poor.

Failure to adjust to gradient attributed to sluggishness.

One enters V.D. zone at 8:13. Movement slow.

Poor distribution. H₂O added at 8:31.

Little or no movement since last reading.
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<td>One organism leaves V.D. zone at 8:47.</td>
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<td>Failure to transpose due to almost total lack of movement. Culture old. Readings concluded. Fission of one organism occurred at 9:12.</td>
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<td>Culture placed 8:14. Temp. lower than last night. N. bistodialis. Perfect transposition.</td>
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<td>Two entered at 8:38 into V.D. zone.</td>
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<td>One organism lost from D. zone at 8:51.</td>
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<td>Perfect transposition.</td>
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<td>Perfect transposition.</td>
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<td>One entered V.D. zone (9:12) just before field change.</td>
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<td>One entered V.D. Zone 9:19.</td>
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<td>Two entered V.D. zone 9:21. Drying probable cause since at 9:27 it had progressed til observation had to be concluded.</td>
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CHAPTER II

A. Reactions to Graduated Fields

SUMMARY

The preceding data may be summarized as follows:

(a) Number of experiments.......................... 12.
(b) Total time of observation...22 hours, 25 minutes.
(c) Number of readings.............................. 281.
(d) Number of perfect field transpositions........... 27.
(e) Total organism-readings......................... 1603.
(f) Total organism-transpositions.................... 1005
(g) Number of organisms failing to transpose.... 97.
(h) Number of reversals against gradient.......... 23.
(i) Organisms leaving field during reading interval.49.
(j) Organisms entering field during reading interval.65.

A word of explanation of terms in the above table may be advisable.

Perfect field-transpositions(a) were counted only when all organisms were in the medium intensity band at one reading and all changed to the medium intensity band of the next successive reading.

Total organism-readings (e) is the sum of all organisms
upon which readings were made; the actual number of individuals undergoing the experimental condition, added from reading to reading.

Total organism-transpositions (f) is the same kind of total as (e) but for those organisms showing transposition. Reversals (h) against the gradient were those cases where organisms moved from the medium intensity zone into the upper, or more intense extreme, cases where individuals forced to leave the medium zone after the shift, by which this zone became the extreme, are included in those failing to transpose (g).

Cases of organisms entering or leaving the field during the reading interval will be discussed in the following chapter.

A number of cases of organisms moving in an extreme zone at the time of the change of field which remained in or moved into the corresponding extreme zone after the change have not been treated as transpositions although in a sense, they might be so considered. It is the writer's feeling that in such cases too many complicating factors are involved, (factors to be spoken of in the following chapter) to justify their inclusion as transpositions.
CHAPTER II

B. Response to "barrier fields."

1. Response as turning to remote "barriers."

During the preceding experiments a particular type of behavior on the part of individual organisms was observed so repeatedly as to suggest the advisability of a more definite experimental test of that behavior than could be made with fairness to the results then being sought.

Individuals moving in the area of medium intensity were frequently observed to approach the boundary of the upper extreme and then, without actually entering that area, move off for a distance and roughly paralleling it. It was further noticed that this turning occurred at varying distances from the boundary and suggested, at first, that different individuals made their adjustment at different distances. Because of the nature of the experiment then in progress it was impossible to follow the behavior of a single organism in its turnings or to be sure whether or not the varying distances noted were for different organisms or for the same organism at different times. It seemed advisable to arrange a situation in which a single organism could be observed in this act of turning and followed through repeated turnings. This was done by using a diaphragm for the experimental
lamp in which the entire field was homogenously dimmed by three layers of paper (Chapter I. D) with the exception of an area on one side laid off by a chord whose greatest diameter was roughly \(\frac{1}{4}\) of the diameter of the field measured on the diameter at right angles to the chord. This area was covered by one layer of paper. This bright area shall be spoken of hereafter as the barrier zone. This zone was not intensely illuminated but was relatively much brighter than the remainder of the field.

The procedure was to isolate a single organism in movement in the field with the condenser lowered so that the field was homogenously lighted. Then by raising the condenser, as in the foregoing experiment, the diaphragm pattern was focused upon the upper surface of the slide. Since the comparative distances of the organism from the barrier region and from its position at the previous reading were the crucial consideration it was felt that a subjective estimate of the observer was not to be entirely trusted. Therefore a camera lucida tracing of the organism at each reading was made so that its position at successive periods with respect to the structure of the entire field could not be open to question. The diaphragm was rotated so that the organism was moving directly toward the barrier and its successive positions traced at intervals.
of 5 minutes. After the organism had changed its course, it was allowed to move out of the field or in most cases, to a region near enough the boundary to determine the angle at which such departure would occur (presuming it would occur) provided the field were left unchanged. Then by movement of the stage the organism was returned to a more central position and the barrier once more rotated to a position directly in its path. Thus the organism, of whose identity there was no question, could be forced to change its course five to ten or more times in the interval of a few hours observation.

As explained earlier (Chapter I, D.) these records were made upon large sheets on which a single field was recorded. Later, for the sake of space and in order to place the records of all readings on an organism on a single sheet, these sheets were drawn to a reduced scale on which the position of an organism was indicated by a dot through which an arrow was drawn to indicate the direction of movement at the time of its tracing. The reduced scale drawings indicate the organism's path and its changes of direction more plainly than did the larger sheets without loss of accuracy in the relationship of the figures.

Experiment number 9 is the result of a fortunate
coincidence. The experimental organism, after having been followed for a considerable time during which it was made to change its direction a number of times, settled down and under-went fission. The daughter amoebae, when activity was resumed were followed till their paths diverged so widely as to make recording of both impossible. Then one daughter was arbitrarily selected and followed through a number of changes of direction. The conclusions to be drawn from this experiment will receive special attention in the following chapter.
Date: 4/24/34
Room temp.: 76 degrees F.
Magnification: 100x

Time:
7:00 - 8:00
8:00 - 9:00
9:00 - 10:00
10:00 - 11:00
11:00 - 12:00
12:00 - 1:00
1:00 - 2:00
2:00 - 3:00
3:00 - 4:00

#2, a--j.
Room temp.: 73 degrees F.
Date: 4/26/34.
Magnification: 100x.

Time:

105° - 1135°

95° - 105°

84° - 95°

720° - 84°
#2, a-e.
Date: 4/30/34.
Room temp.: 78 degrees F.
Magnification: 100x.

#5, a-e.
Date: 5/2/34.
Room temp.: 80 degrees F.
Magnification: 100x.
#6, a--o.
Date: 5/3/34.
Room temp.: 78 degrees F.
Magnification: 100x.

#1, a--o.
Date: 5/4/34.
Room temp.: 72 degrees F.
Magnification: 100x.
Time:
7:25 - 8:25

10:05 - 10:55

11:00 - 11:45

12:00 - 12:30

2:00 - 2:30

8:00 - 8:25

9:10 - 10:05

10:55 - 11:45

12:00 - 2:10

2:55 - 3:45
Date: 7/14/34
Room temp.: 94 degrees F.
Magnification: 100x.
$^{9}$ (continued), $k-\tau$. 
9 (continued), u--v.

Date: 7/9/34.

Com temp.: 96 degrees F.

Magnification: 100x.

Time lapse during mission: 1 hour 45 minutes.
#11, a--e.
Date: 7/12/34.
Room temp.: 93 degrees F.
Magnification: 100x.

#11, f
Time: 9:15 - 10:15

#11, e
10:15 - 11:00

#11, c
11:00 - 11:40

#11, d
11:40 - 12:20

#11, b
12:20 - 1:00

#12, a
Time: 7:05 - 9:05

#12, b
9:15 - 9:55

#12, c
9:55 - 10:55

#12, d
10:45 - 11:20

#12, e
11:20 - 12:15

#12, f
9:15 - 10:15

Date: 7/15/34.
Room temp.: 94 degrees F.
Magnification: 100x.
CHAPTER II

B. Reactions to "Barrier fields."

SUMMARY

Two general conclusions may be drawn from the data on the reactions of this amoeba to light "barriers."

1) Adjustment of the organism indicated by a change in its path occurs not after having moved into the area being avoided but at varying distances from that area.

2) Such adjustment occurs more promptly and at increasingly greater distances with repetition of the experience by the same organism.

Two other interesting facts, which will be more completely discussed in the following chapter, may be mentioned here. It will be noted that in many cases the path of an organism from point of turning to the point where the field is shifted describes a curve suggestive of a parabola. This similarity, if sound, is significant. In experiment number 9, where the daughter organism of one having demonstrated a progressive adjustment to a situation apparently begins activity without any additional power to adjust, is seeming proof for the view that environment adjustments do not span successive generations. Of course single examples such as this neither prove nor disprove anything, yet are interesting in themselves.
In conclusion, there follows a table of items in this division of data:

(a) Total time of observation..........83 hrs., 40 minutes.
(b) Total number of adjustments........100.
(c) Failures to adjust.................none.
(d) Total number of tracings of organism...1094.
(e) Individuals failing to show progressive adjustment......................none.

Total number of adjustments (b) is the sum of turnings in all experiments.

Failures to adjust (c) refers to failures to turn which if they had occurred would have carried the organism across the barrier into the light zone.

Item (e) refers to cases where the distance of turning from the barrier did not increase with repetition. The fact that the value for both (c) and (e) is none is significant, as will be later shown.
CHAPTER II

C. Response to Multiple Barriers.

1. Vectoral behavior.

The behavior of organisms to the light barriers in the experiments just described suggested that their behavior to more than one such area might be interesting and led progressively to a study of individuals reacting to two such regions and finally to a situation in which it was surrounded by a triangle of such barrier areas.

The following experiments include several of these barrier combinations. First, those in which the barriers form a v, the sides of which are at 90° to each other and are of equal intensity. The relative intensity of the barriers to the remainder of the field was the same as in the preceding experiment (1-3 ratio; e.g. one layer on the diaphragm for the bright areas, 3 layers for the rest of the field.) Organisms were observed when started in motion, with respect to this pattern, in a number of ways, as the diagrams will indicate.

A second arrangement, similar to the first, except that the barriers had a 1-2 relationship, the field remaining as before, was tried with the interesting result shown by the diagrams.
Those experiments in which the organism was completely surrounded by barriers were of two types: Those in which all three barriers had an intensity of 1 as compared to the field of an intensity of 3, and those in which the barriers were each a different intensity, having a value of 1, 2, and 3 as compared to a field of the value of 4. The similarity of behavior in both will be noted but the interesting path in the latter triangle combination is worthy of especial attention.

The final group of experiments arose out of the question of a colleague as to the extent in which diffusion from the bright barrier areas might be determining the path of the organism. It was decided to prepare a triangle-shaped barrier of 60° rather than 90°, as before, in the hope of getting a more direct path of exit by the organism. The first figure in the diagrams of these experiments show this unaltered path. Then in successive fields of the same organism, the path was followed until the organism reach a place of greatest distance from the barriers at which time the field was rotated through a varying amount and the effect of that rotation upon the remainder of the path observed. Records of two such experiments follow both of which show about the same response to modification of the field regardless of the angle through which the rotation occurs.
A word of explanation of the diagrams of this particular experiment may not be amiss. The field as drawn with its vectoral value indicated by a dashed line indicates the position of the pattern throughout the first part of observation. The dotted line indicates the vectoral value of the field after its rotation. The position of the organism at the time of rotation is indicated by a short line across its direction - indicating arrow thus: $\neq$
#1, a--f.
Date: 6/17/34.
Room temp.: 36 degrees F.
Magnification: 100x.

#2, a--d.
Date: 6/19/34.
Room temp.: 83 degrees F.
Magnification: 100x.
#4, a--j.
Date: 6/25/34.
Room temp.: 93 degrees F.
Magnification: 100x.
#5, 2--J.
Data: 6/30/34.
Room temp.: 93 degrees F.
Magnification: 100x.

- Time: 6:15 - 7:15
  - 5, a

- Time: 7:15 - 8:15
  - 5, b
  - 8:15 - 9:15
  - 5, c

- Time: 9:15 - 10:15
  - 5, d

- Time: 10:15 - 11:15
  - 5, e

- Time: 11:15 - 12:15
  - 5, f

- Time: 12:15 - 1:15
  - 5, g

- Time: 1:15 - 2:15
  - 5, h

- Time: 2:15 - 3:15
  - 5, i

- Time: 3:15 - 4:15
  - 5, j
Date: 7/12/34.
Room temp.: 92 degrees F.
Magnification: 100X.
Date: 7/18/34
Room temp.: 89 degrees F.
Magnification: 100x.

Date: 7/19/34
Room temp.: 83 degrees F.
Magnification: 100x.
Date: 7/22/34.
Room temp.: 96 degrees F.
Magnification: 100x.

Date: 7/23/34.
Room temp.: 91 degrees F.
Magnification: 100x.
#13, a--e.
Date: 8/5/34.
Room temp.: 95 degrees F.
Magnification: 100x.

#14, a--e.
Date: 8/8/34.
Room temp.: 93 degrees F.
Magnification: 100x.
#'s 15-24, inclusive
Room temp. 87-94 degrees F.
Dates: Fields 15, 16, 17: 8/15/34.
   18, 19: 8/17/34.
   21, 22: 8/20/34.
   23, 24, 25: 8/27/34.
Magnification: 50x.
CHAPTER III


a. Consideration of experiments.

The data of this first section, having been placed in table form need little further explanation than to point to some interesting aspects of its summary to be found immediately following the table and to draw, briefly, some conclusions that seem justified.

First it may be well to note that all organisms did not make their adjustment, did not transpose. Of the 1603 organism-readings (the number of organisms reacting at the time the reading was made added together from reading to reading), 1005 made perfect transpositions. Those not so included are all that in any way failed to give a complete, characteristic response. The question may be raised as to why these exceptions occurred. In answer it may be well to cite the fact that only a single, environmental factor was being controlled, that of light. On the other hand, the organisms during the experiment were carrying on an otherwise normal life activity subject to all the influences to which they respond in the natural state. When this is considered it seems remarkable that so large a percentage should for a time be controlled in all their movements by the one aspect of their environment so manipulated.
A second question presents itself as to why organisms moved into or from the field since the surrounding culture was in total darkness except for, perhaps, a marginal region not under observation, which however, was striated just as was the part being studied. In answer to this it must be remembered that no assumption has been made in this paper as to whether or not light is necessary to the normal life activity of these animals or as to comparative value to them of light and darkness. The point here is that organisms in a light field of certain pattern structure do, to a significant percentage, react in the manner shown. Those that leave or enter must be considered as behaving atypically and such behavior, I think has been adequately accounted for in the preceding paragraph.

The converse of the foregoing questions may be raised: How can one be sure that certain organisms which seem to transpose are not actually reacting to some other influence (food, i.e.) than light? Such might be the case in single instances but it is inconceivable that such other influences would reverse themselves concomitantly with the shift of field and the tables only need be studied to show that these transpositions did occur alternately with the shift of the field many times in succession. That such transposition as occurred was due,
therefore to the change of field pattern, seems the most logical and economic explanation.

In conclusion to this summary of transpositional behavior a few conclusions may well be drawn.

CHAPTER III, 

b. Nature of Reaction.

First, reactions were to the entire field. It will be remembered that the shifting of the field changed the actual intensities of all three bands of intensity when considered in their relationship. The intensity of any given region remained the same throughout all experiments. Only their relationship, with respect to the entire field, was changed. Yet changes in behavior occurred with these shifts so it would seem that the only possible explanation is that such behavior was to the structure of the entire field. That alone was being manipulated.

Secondly, behavior was a purely relative type of adjustment and not to a given intensity since the neutral region sought after a shift of field was in actual intensity, one of the extremes previously avoided. In other words, the neutral band was such at any time only with respect to the rest of the field at that same time. Thus the adjustment was not to a given intensity but to a region of certain intensity relations to the entire field.
Thirdly, since the reaction of these organisms was to such relative relationships of the entire field it is impossible to account for their behavior in terms of an environmental force "impinging" upon them. It may be argued that the light of the zone which they occupied at any given instant (let us say the neutral zone) did not impinge upon them. Granting this, momentarily, the fact remains that, as we have seen, they were reacting quite as truly to the extreme zones as to the one they occupied (for a shift in the total field, to which they responded; obviously could not be accomplished in terms of a part of that field) and the remoteness of these extremes obviates the possibility of their "impinging" upon the organism in its central position. Some concept other than that of "environmental forces impinging upon an assumedly discrete organism" seems imperative to account for these transpositional reactions.

A final question may be raised: How did these organisms "sense" this total field in order to adjust themselves to it? Or, wording it differently, what was the relationship of these organisms to this light field which made possible their response to its total pattern? The question will be left unanswered temporarily, since it occurs in summaries of the remaining data, and will be considered in the later part of this Chapter.
A, 2. Summary of Reactions to Barrier Fields.

a. Consideration of experiments.

The data of the second series of experiments treated in this paper have two distinct aspects which are worthy of notice in this chapter.

The first of these are the characteristics of these reactions that are common to those studied in the division on transpositional behavior. It will be remembered that the light field is obtained in exactly the same manner. The bright region which acts as a barrier to the organism when placed in its path is not a source of light but is lit by a perpendicular beam just as all patterns are in all of these experiments. The possibility that sufficient refraction occurs at the field side of the boundary of the barrier to give direction to the path of the organism is ruled out by three facts. First a slide of low refractive index ( ) was used; therefore any refraction due to the slide would result in a very narrow band and the turning of the organism in practically all cases occurred at a distance from the barrier appreciably greater than the possible width of this band. In experiment number 2 such influence might account for the first turn (field number 2:a) but it will be noticed that all remaining turns were made at much greater distances.
and with greater promptness. This is true, likewise, for every other case where the initial change of direction occurred near the boundary of the barrier. (See Fields Number 6:a; 9:a, b and r; Number 10:a; Number 11:a; Number 12:b.)

The possibility that light from the barrier was being diffused by material in suspension in the culture seems unlikely since the culture was quite free of suspended material and also by the fact that such diffused light, due to the very fact that it was diffuse, would have no directional effect on the path of the organism, since it would be coming from all directions.

In the third division of these experiments, several were conducted in which the position of the field was changed after the organism was at a considerable distance from the barrier. The results of these, to be discussed with the remaining section of data, prove beyond question that the change of path with respect to a barrier was not due to light being refracted from it to the organism.

With the possibility of direct influence from the barrier zone removed, it becomes apparent that here, as in its transpositional reactions, the organism is reacting to the field in its entirety. Thus its avoidance of the barrier region occurs at varying distances with equal readiness since any reaction to the barrier, seemingly, is actually a reaction to the entire field.
of which the barrier is but a part.

Out of this, however, comes the same question which the transpositional responses invoked: How does the reacting organism "sense" the entire field and react to its pattern aspects which exist only in terms of the entire field? Again we leave the answer till the latter part of this chapter.

A second interesting characteristic of these barrier reactions, and one not encountered before, is the evident improvement in response of the same organism to repeatedly experiencing the necessity for turning. This improvement is evident in both the increasing distance from the barrier at which turning occurs in successive experiences and the decreasing time required for the accomplishment of successive turns. In only one instance in the turns which comprise the data of this division is such improvement not noticeable. In experiment Number 12 it will be noticed that the organism approached nearer the barrier on its second turn (Field Number 12:b) than it did in its first turn (Number 12:a). However, thereafter its adjustment progresses just as in the case of the others. The best possible explanation would seem to be, as before, that at this particular time other factors than light were preponderant in their influence.
CHAPTER III


a. Consideration of Experiments.

The data of the third section of this paper may be divided into three groups: vectoral responses to two barriers of equal intensity, those to two barriers of an intensity ratio of 1 to 2 and those to three barriers which completely surround the reacting organism, which in some cases were of equal intensity and in others where the intensity ratio was of a 1, 2, 3,-order.

The finding of the first two groups may be considered together. As can be seen from the diagrams, organisms were studied as they reacted to these V-shaped barriers from many points and angles of entrance. Further it may be noted that the point of departure from the field varied in its coincidence or nearness to the vector drawn on all diagrams. Those organisms left the field nearest to this vector which were allowed to follow it from their point of first response to the field with the least turning or other required adjustment. (See Fields Number 1:e and f; Number 5:d; Number 6:e; Number 8:e; Number 9:e; Number 10:e; #12:b, c and e; and the initial field of #13 and #14) Next nearest to a perfect vectoral response were those organisms whose turns were accomplished high in the apex of the V of the barrier. (See #2:b; #3:g and i; #5:g, e, f, 5, i; 10:a, d; and
and 11:b) As will be seen, however this rule did not hold in all cases but seemed to vary with individual organisms. Mention is made of this aspect of these reactions, especially the first, because it is indicative of an aspect of behavior wholly without place in the tropistic systems of explanation. It will be presented in the following section of this chapter.

What has been said of the foregoing experiments may be said of these reactions to multiple barriers. Response is to relative intensities, and to the total field. Especially is this latter fact shown by Fields #13: a to e and #14: a to e₂ where the rotation of the pattern brings about the noticeable change in the path of exit. As before any concept of ray impingement is ruled out, as is especially well shown in the rotated field experiments just sighted. New aspects are the definite vectorial paths and the persistance of path which seems to be a function of path direction and time which was mentioned in the preceding paragraph.

The same question that presented itself twice before again may be asked: How explain the apparent "sensing" of this pattern by these organisms?

In conclusion to this generalized view of these experiments it may be said that here are three types of behavior seemingly without explanation in terms of tropisms. Reactions
are not to definite intensity values, they are not to impinging rays, and they are progressively modifiable to repetition of the situation. Some approach other than the mechanistic one of tropistic response seems imperative.
CHAPTER III

B. The "Field" as a dynamic biological concept.

1. The place of hypothetical constructs in the scientific explanation of phenomena.

The arm of science is the systematic observation of natural phenomena, their interpretation or explanation to the end that future events may be predicted and, in certain fields, controlled. Thus scientific procedure has two inseparable aspects: first the objective observation of naturally occurring or experimentally produced data and, second, its interpretation, evaluation or consideration in the light of which often widely differing phenomena are found to have meaning and unity. The first, the collection of data, without the second would never, in any sense, explain to us the universe in which we find ourselves and would have, if forced to stand alone, about the same relation to our large body of scientific fact that the dictionary has to the finished essay. On the other hand the evaluative aspect could not exist, obviously, without data to be evaluated. Even the most speculative, "arm-chair philosophizing" must have some body of observations, regardless of how uncritically acquired, to serve as "grist" for its conjectural "mill."

Little need be said of the necessity of data to a science; the point here is that data alone do not constitute a science.
It is in this direction that the error of our thinking is inclined to tend. Eldridge\textsuperscript{10} in his preface, states this point rather well:

"There is a tendency, as methods of observation and experiment are developed, to emphasize the collection of data, and to underestimate the value of a thorough analysis of the data collected." One might extend this criticism further. Not only is an analysis of data often omitted by the investigator but even more the disposition to criticise or re-evaluate the hypothetical system of explanation which in many cases, he unconsciously uses. Haldane\textsuperscript{11} feels a growing tendency to such re-evaluation as characteristic of the present scientific era. He says:

"There are not many features of the intellectual life of the twentieth century more interesting than a new disposition that is becoming very prominent. It is the disposition to search for and drag to light unconsciously made assumptions. How much may not the individual mind of the observer have deflected the results which his observation has yielded?" Whether or not we share the optimism that such disposition is peculiar to the twentieth century, the fact remains that there is serious need for the "draging to light" of the hypotheses, which are

in many cases "unconsciously made assumptions" upon which rests the meaning of our observations. Especially is this true of the biological sciences.

In the field of physics, where the consideration of method has progressed so much farther than in other branches of science, save perhaps in mathematics, we find the best example of the harmonious and fruitful interrelation of the collecting of objective data with the systematizing of those data in terms of purely conceptual systems. The recent revolution wrought in the science of physics by the discovery of radio-activity and by the introduction of the theory of relativity are too familiar to require any discussion here to make them illustrative of our point. The thing to note is that both were revolutions in the underlying thought systems of the science. The fact that such revolutions have occurred and may at anytime occur again may explain the present state of the science in its eminence over the present day biological sciences.

It may not be amiss to look more closely at the type of conceptual systems that physics has found necessary to the explanation of natural phenomena. Perhaps the most historically interesting and at the same time most significant to the present age is the physical concept of the dynamic field. To Aristotle

The universe was a vast machine. To explain the regulation of the heavenly bodies he postulated their paths as determined by crystal spheres, etc. To him causality was a part-to-part process in a pre-existing organization, that organization determined in no sense by the process going on within it. But the physics of Galileo, of Newton and of the present have substituted a conceptual system at the same time much wider and much simpler than this unapproachable, unexplainable machine universe of the ancient mind. Lewin has pointed out that the underlying thought of the two approaches, the underlying assumptions of the Aristotelian as compared with Galilean systems, are exactly opposite.13 This is doubtless true. Our interest here is to look more closely at the latter concept that has made possible the modern science of physics.

Kohler14 has pointed out in several of his works that regulation in physical systems may be of two distinct types. At one extreme is what he calls the "machine" type of regulation. Here the functional process is confined to narrow limits of expression by pre-established organization. The steam expanding in the engine cylinder impart its pressure in all directions but the nature of organization is such that

13. Bibliography #49.
motion occurs in only one direction, etc. Next he speaks of a type of regulation in which preestablished organization tends to set limits to internal activities which in themselves are dynamically regulated by their own processes. In the example of water flowing through a pipe of some diameter, the regulation of an arbitrarily selected particle within the column of water is a matter of the dynamics of surrounding particles and the spatial relationship of such particles is a direct result of their dynamic interrelationship except that only in an immediate sense is this true because of the confining pipe and its influence in directing the total movement of the water column. But let us suppose, he goes on to say, that this column of water is not confined by a pipe but is an arbitrarily chosen part of a vast ocean. Then does the regulation of any particle become the result of its dynamic interrelation with every other particle of the total system. Then the organization of the total system, currents, eddies, etc. is quite as truly the result of the dynamics of the system as is the regulation of any part as such. Thus organization of the whole is but an aspect of its total dynamics quite as truly as is the regulation of the part and vice versa.

It might occur to the critical reader to ask if the first two illustrations are not actually subtle example of
the third. Be that as it may, this is not the place to fur-
ther criticise Kohlers classification. The thing to be con-
sidered is this third type of regulation and its implications
as a hypothetical construct. It is an example of the dynamic
field, a concept that has been most fruitful in making physi-
cal data meaningful.

B, 2. The Construct of Physical fields.

Let us look more closely at some of the distinguishing
characteristics of the field concept. First we must note
that it is purely conceptual. It does not have substance in
an objective sense. Consider, for a moment, the electro-
magnetic field about a conductor. We can measure its strength
in terms of lines per unit area, but such lines do not have
reality in terms of substance. Yet their "reality" is quite
as well established as the most objective thing or quality
the senses can experience. We can determine the direction
of such a field with relation to the conductor, for instance,
and predict the behavior of certain bodies when influenced by
such a field. Thus by the aid of sensible substance a very
great deal may be known of this purely conceptual thing, the
field, and its reality as well establish as if it could be
sensed directly. Yet actually it is conceptual, a hypothetical
construct, sensed only through that which it explains.
A second characteristic of physical fields is their dynamic unity. As shown earlier, their organization arises from the same dynamic relationship that tends to regulate their parts in the functional process. Hence, structure and function in a dynamic field are but different aspects of the same process. Parts do not exist, except in terms of the pre-existing unity of the whole and conversely such unity is not super-imposed upon the system from without but is a functional aspect of the field, itself.

A third characteristic to be noted is that position and change of position (motion) in a field are purely relative. The structure of the field in its entirety in terms of regions of relatively high and low potential explain the motion or position of parts. There is no place or need for postulating any "innate" self-direction of the part in its behavior for that which accounts for its existence as a part likewise explains its behavior as such. Consider the ball rolling down the inclined plane. Its motion is the product of the gravitational field in which a potential difference has been created by the position of the plane and the ball upon it. To account for its motion, the entire system, of which it is a part, must be considered. The position of the plane, the shape of the ball, the fact of gravity are all but aspects of the situation
out of which the motion of the ball arise in its process of equilibrating a potential difference between the place of origin of its motion and its place of rest, or cessation of motion.

A fourth characteristic of a dynamic field becomes evident from this illustration: Causation is not from part to part but from whole to part. Nothing "impinged" upon the ball to cause it to roll. Only the most naive observer would be deluded by the apparent "discreteness" of the ball from its dynamic situation to such extent as to assume that its motion is the result of either impinging forces or an "inner directing force." Its motion is from a region of higher potential to a region of lesser potential which explains both the roll and its direction and exists only in terms of the total situation.

The last characteristic which must be cited for the future use of this paper is that physical fields are at least quadridimensional. Since motion of a part within a field is the result of total structure and structure is dynamically determined in exactly the same sense as the regulation of the motion of the part, it becomes obvious that with motion occurring in time, the structurization of the field, itself, has a time dimension. Since the effects of dynamic fields can be sensed in our three dimensional, Cartesian space and can equally
well be experienced as occurring in time, four becomes the minimum conceivable number of dimensions in which physical fields can exist. Do they exist in more than four dimensions? That is a physical question, outside of this paper. Certain aspects of it, of interest in a biological sense, will be taken up in later parts.

A final word may be said concerning the question of the limitation of fields. Obviously any "field" being considered is arbitrarily limited in a logical sense for the simplification of the study involved. Quite as obvious, however, is the fact that such limitation is entirely arbitrary. The field concept, because of its implied unity, is, of necessity, an infinite concept in both the mathematical and metaphysical senses. Any postulation of boundaries or limits implies, logically, that which lies beyond. Just as the part-whole relationship in any limited field or system being studied is purely relative so is that field or system in its relationship to that which transcends it. This fact of its infinite extension as a concept in no sense limits the usefulness of the field as a hypothetical construction to make observable phenomena meaningful. Science is forever pushing the

15. For an example of our early recognition of this limitation dilemma see the arguments advanced by Nicolaus Cusanus (1401-64) against the Aristotelian concept of a spherical, limited universe, (Nordenskiold, History of Biol. p. 35 (Bibliography #3))
boundaries of the known to the outer margins of finite comprehension. The present dilemma of physics with regard to the election in no sense invalidates the physical concept of the nature and structure of matter based upon it. The field concept is indispensable in rendering objective data meaningful; the fact that logically it must be a concept of infinite dimension in no sense invalidates it for such use.

B. 3. The Construct of Biological Fields.

The foregoing brief analysis of the field theory as it has come to have meaning in modern physics finds point in this closing division of this chapter. Two facts of immediate significance should be pointed out: it arose to account for and make meaningful data which in terms of any other system would be meaningless and through its use the science of physics has progressed beyond the triteness of Aristotelian mechanism and the spiritistic-vitalistic universe of the scholastic middle ages. Modern physics and mathematics16 rest upon the universal, relativistic field type of concepts.

Now to the point of this paper: To what extent has biology learned its lesson from the methodology of physics? To what extent have biological data been interpreted, in their dynamic aspects, in terms of field theory? At first

thought, the answer is discouraging; the concept has found such scant place in one of the most dynamic branches of science, the science of the origin, structure and behavior of living things; biology.

In one branch of biological science, Psychology, the field type of theory has found its place. It has made meaningful certain problems of vision in the early works of Mach, Wertheimer, and others17 in more recent times and also has found place in general psychology18, neurology19, and animal behavior. In other branches of biology, the tendency to use such concept is apparently in its infancy. Botany, perhaps because it deals with more sessile organisms, is practically unfamiliar with field theory20. Zoology, is quite largely so with a very few instances of interpretations that approach the field type of theory but in which such an approach as fundamentally distinct is only partially realized. The widely

17. Bibliography Numbers 54, 55, 56, 57, 58, 59.
18. See works of Numerous German and American Psychologists; D. K. Adams, J. B. Brown, W. Kohler, K. Köhler, K. Lewin, R. M. Ogden, F. T. Perkins, R. H. Wheeler, to name only a few. Also the extensive bibliography of works conducted from this point of view that has been so rapidly growing in recent years.
20. For an early work and one of the very few in existence see Bibliography No 69.
differing fields of histology, embryology, morphology, physiology, neurology (already mentioned), ecology and cytology have just a few investigators sufficiently interested in the dynamic aspects of their fields to feel or sense the imperative of such a concept as the field theory. In the abstract approach of theoretical zoology two modern thinkers have passed beyond the mechanist-vitalist dichotomy of the last century to a critical consideration of the method of the science and to an appreciation of the necessity of an approach such as the field theory offers. Except for these, so much can not be said of the great number of writers in theoretical zoology.

In the study of the behavior of simple organisms, with which this paper is primarily interested, it is not unfair to

22. Bibliography No.'s 72, 73, 74, 75.
23. It is most unfortunate that the monumental work of Thompson (Bibliography No. 76) is at present out of print, not only because of the vastness of the work itself but because of the splendid history of problems of morphology and the fine bibliography it contains.
24. See the numerous publications of Royal S. Chapman on the Ecology of Insects (Reference not listed here because their main emphasis is economic)
25. See Bibliography No. 77 for a text. The entire emphasis of which is logically and convincingly organismic. Contains an excellent history.
26. See Bibliography No.'s 75, 76, 79, 80.
27. The appreciation of the common methodological problem of histology and physics is growing as the works of some clearly indicate. See Bibliography No.'s 81, 82, 83.
say no approach from the view point of field theory has been made. Such fact may be accounted for by a brief consideration of the tendencies of investigation of protozoan behavior.

Because of the assumed simplicity of the protozoa (in turn, perhaps, a result of the naiveté of the cell doctrine) investigation of protozoan behavior has led invariably to analysis of internal conditions from the view point of physiological interest, and away from the broader interests of physical ecology. Those who have investigated the physical ecology of the protozoa have done so largely by extending physiological techniques and assumptions. This is the case, it would seem for a very good reason. In no field of biology has the vitalist-mechanist feud been more keenly felt. Those who have carried on investigation have largely been of the mechanist persuasion and naturally their only approach lay in the minute analysis of the organism, itself, to account for its behavior. Where environmental conditions were studied, it was still from the approach of mechanist assumptions.

The splendid work of Jennings13 published in the first decade of this century presented, as has been shown in earlier chapters, a number of problems and a certain degree of insight into the complexity of protozoan behavior which has not been equaled in publications since. The trend since has been toward the physical and chemical analysis of protoplasm, toward

13. Bibliography #49.
the explanation of the mechanics of colloids (especially true of interest in amoebae), toward the study of response to different types of stimulation in terms of tropistic assumptions. (And so the matter stands, in a large measure, today.)

Let us turn our attention, now, to the application of the concept of biological fields first to the data of this paper, next to reactions that in the past have been interpreted in terms of tropistic concepts and finally, let us consider certain possible objections to the validity of such concept in such interpretation.

It will be recalled that at the close of each division of the data offered in Chapter II certain generalizations could be drawn: (1) Organisms reacted to the total light field; (2) Reaction was to relative not to absolute intensities; (3) Response to multiple barriers was of a sectoral or centroid type; and (4) Response became progressively improved with repetition of the experience. Yet at the conclusion always the same question presented itself: "How do these organisms 'sense' this total field to which they adjust themselves?"

We are now in a position to answer that question. The first step in the answering of it is to realize that the question, itself, arises out of a false assumption: the assumption of the discreteness of the organism from its environment; the same assumption, as earlier shown, that lies
at the bottom of the vitalist-mechanist controversy and out of which the concept of tropisms first came into being.

From the approach of field theory the organism, dynamically is in no sense discrete. It is but a highly structuralized part of a transcending situation, the field. Changes in the organization of that total field bring about concomittant change in the part, the organism.

Through changes in any aspect of the field, tensions are set up, and since the structure of that field and the regulation of the part are but different aspects of the same dynamic situation, motion occurs through time to the end of restoring equilibrium. Considered from the point of reference of the organism, that is what constitutes its "sensing" of the field (It is hoped that the old connotations of the term "sense" do not confuse the reader. It had to be used earlier, hence here; and hereafter, for fear of confusion, shall be dropped.)

Now, note that in no manner did any part of the field "impinge" upon the organism. Causality in dynamic fields is not from part to part but from whole to part. Likewise there is no problem as to what gave the organism direction in its movement. The same potential differences which required motion to restore equilibrium likewise gave direction to that motion.

It will be recalled that potential differences in dynamic fields are purely relative. Therefore the extent of a tension
existing in a field is proportional to the relative difference of potential of part or regions and not to any absolute value of a region. This fact accounts for the transposition-al behavior studied in the first series of experiments. In the sense of the relative differences of intensity the structure of the field remained quite uniform on either side of the time shift. The organization of the animal (as a system in itself) in relation to this particular type of field (light) was such that equilibrium was attained at a region of neutral or middle intensity. Regardless then of the absolute intensity of either extreme, or of the neutral zone itself, the relative relationship in terms of potential difference remained approximately the same and transposition occurred.

With regard to barrier reactions it need only be pointed out that the barrier apparently was a region of high potential with respect to the rest of the field. If so, then four observable aspects of response must be accounted for: (1) The path of the organism was not directly away from this barrier but was in the form of a curve to which the inner margin of the barrier was roughly tangential. This can be accounted for partly by two facts; the first that this experimental field was not the only one to which the organism was reacting and, second, to the certain persistence of path that has been noted before and will be further considered shortly.
(2) The organism underwent adjustment by which it changed its course at increasingly greater distances from the barrier. To account for this it must be recalled that the organism was spoken of, a few paragraphs earlier, as "a highly structuralized part" of the dynamic field accounting for its total behavior. That is exactly what the organism, any organism, in a dynamic sense, seems to be. Such structuralization is, of course, a field product quite as the regulation of its behavior. It always has been and always will be since the organism never exists except as a part of a transcendental system. Logically, then response and change of organization of the organism are different aspects of the same thing. The first, we speak of as behavior; the second we recognize in its various aspects as learning, maturing, aging or living. Obviously, then, an organism ("even an amoeba!") shows progressive adjustment to a repeated experience.

(3) Little need be said concerning vectoral response nor the fact that the paths of the organism from multiple barrier fields did not perfectly follow the theoretical value of the vector. The line of resolution of a tension situation would be from a purely physical point of view, a vectoral value by the very meaning of the term, vector. Digression of the actual path from such value may properly be accounted for as have been other tendencies to digression.
(4) There remains the evident fact of persistence of actual path of the organism in numerous cases, yet to be explained. In physical fields objects (parts) tend to maintain motion or rest and such tendency is known, of course, as the inertia of the object. It needs only be cited here, that in physical fields such inertia is the result of the time dimension of such fields. It may be argued that the inertia of a body is a function of its mass. That of course is true, when considered in an immediate sense, but considered in its more ultimate sense, mass is itself a field property since, when relativistically considered, mass is quite as truly a variable as is velocity of motion. Therefore in a physical sense, inertia is a field property manifest in time.

Now to the problem at hand: Is it not logical, in terms of field theory, that organisms in their expression of field dynamics should manifest a field property comparable to that of physical fields which inertia denotes? One cannot escape such concept as this. In addition to its logical necessity are the facts, first that it explains these persistence aspects of the paths described in the data of this paper and second that a number of broader biological phenomena take on new meaning in the light of such concept. In the following chapter an interpretation of some of these phenomena shall be attempted from such point of view.
A few words remain to be said concerning that larger body of data on the subject of protozoan behavior which has been, and still is, interpreted in terms of tropisms. As pointed out earlier in this paper the concept of tropisms has arisen out of the dilemma of the investigator and interpreter which in turn is the consequence of his more or less unconscious assumption of the discreteness of the organism from its environment. Because of this dilemma in explaining organic response in terms of impinging physical forces, the term tropism has been shown to have shifted from its original meaning as a descriptive term and has become more and more a term used to explain the very thing it was originally coined to describe. Field theory avoids this dilemma by avoiding any assumption of organic discreteness, and still will account for any aspect of behavior accounted for in terms of tropisms. The problem of optima of stimulation is one of the organization of individual organisms in relation to the potentials and organization of different action-producing fields. Research on the subject from this point of view will yield values for such relationships, or at least equational statement, from which, if certain information is at hand, such "optima" in given situations can be predicted.

Likewise the problem of threshold intensities becomes one of the potential differences existing in fields and, with
study, could beyond question be equationally expressed to the end of prediction.

The great body of data on the orientation of protozoa to light, etc. offers no instance not explicable in terms of field theory. Certain things occurring in these data (selective response, for example and first digressions from established responses, etc.) find meaning only in terms of field theory. The concept of tropisms has always been helpless in cases of spontaneous or "self-initiated" response as also in those having to do with what appears to be choice of response. Part of the following chapter will be devoted to this sort of behavior in protozoa as well as in higher forms.

As a conclusion to this chapter it may be well to consider briefly some possible criticisms that may be leveled at the concept of biological fields. The first that may be expected is that of parallel logic: the accusation that a pretty analogy has been made at the expense of physics without benefit to biology. If such is made the answer is threefold: first, that if the transfer of methodological approach from one science to another is indulgence in "pretty analogy" then physics and astronomy are guilty of exactly that in the light of the convergence of these sciences upon purely mathematical grounds. In the second place, as to whether field theory offers anything to biology, the answer lies in the
open-minded consideration of several facts: the present lack of biology in the matter of a critical consideration of its methods; the immense mass of unsystematized biologic data of a dynamic type that exists and to which continued research is contributing; the numerous dichotomies that have mocked the history of biology and still exist today (i.e. Vitalism-mechanism, the numerous "brands" of evolution, the problem of progressive organization in embryology vs. the cell theory, etc.) The field theory rests on criticism of method for its existence and is the first concept in terms of which data in widely differing biological fields may be systematized. Such possibility certainly offers some benefit to biology.

The third possible answer to this first criticism is to cite the fact that science is built and progresses upon the law of parsimony, that unconscious, unwritten favoring of that method whereby the largest body of data is made meaningful by the most economical use of theory. The law of parsimony, certainly, would not limit a system of explanation to a single field of data or discredit its use in one division of science because it had proved of value in another.

A second criticism may be that field theory is unapproachable, that it pushes final causes to infinite remoteness and gives no place for analysis.

The answer, again, would seem to be three-fold: (1)
Science seeks, not "final causes" but the nature of causality; (2) The fact that field theory must be of infinite extension certainly makes it no more "unapproachable" than are such concepts as "tropisms", "instincts", "drives", "innate capacities" "determiners" and "organizers" terms given to those gaps in mechanistic explanation which even the mechanists themselves realize they cannot bridge; (3) there is place in the field theory for analysis but its place would not be the analysis of a system for attributed admittedly transcendental to that system. Future study of the nature, structure, strength, changes, etc. of fields will be of an analytical order.

A third possible criticism may be that the concept of dynamic biological fields is too simple to account for the vast complexity of living things. If made, such criticism would arise out of a confusion of principle and phenomena. Principles underlying natural events do not have varying degrees of complexity. Principles are simple; phenomena exhibiting them are complex. The physical principle of the lever is equally simple whether exemplified by the pencil I balance across my finger or by the wheels, cams, etc. in the watch keeping time upon my wrist. Complexity arises from the phenomenological expression of the functioning of principles, and may be, as hinted earlier, in the case of living things of infinite order. Such fact, if so, does not discredit simple systems of explanation provided those systems are capable of infinite extension.
"It is through being surprised that man has now and always begun to philosophize."

Emanuel Radl

"Geschichte der Biologischen Theoren der Neuzeit"
CHAPTER IV

A Consideration of some of the Broader Implications of the Concept of Biological Fields.

I. Introduction

A. The relation of Zoodynamics to the branches of Zoology having to do with the dynamics of animals.

Regardless of the fact that zoology, popularly considered is a "morphological science," chiefly concerned with the description and classification of animals, it is true that it has several dynamic branches. In such sense may be named: Animal Behavior; Bio-chemistry and Biophysics; Cytology, in so far as it is concerned with cellular physiology; Ecology; Genetics; Paleontology in its historic and evolutionary interest; Physiology, both general and special; certain general aspects of Psychology, Taxonomy at least in the narrow sense of its concern regarding evolutionary processes; and Zoogeography. These branches, in a general sense, might be thought to comprise a division of zoology which might well be designated as Zoodynamics.

Yet there is another thing to be considered. The great and commendable increase in experimentation in many of these branches has tended to center the mind of the
investigator more and more upon minute problems peculiar to his specialization. This, of course, has led to the advance of these branches in a specialized sense. It is equally true, however, that it has tended to lead away from a consideration of several problems that more or less underly these sciences, yet are not peculiar to any one or several of them. A few problems of such order might well be sighted:

(a) The historically significant and still perplexing problem of the unifiedness of organisms, organizational unity;

(b) The basic problem of the descent of organisms with modification from ancestral lines, the contingency of generations to form races, species, etc., the basic problem of evolution, so-called;

(c) The problem of purposive behavior, especially of simple animals in the sense, at least, in which it is more akin to zoology proper than psychology, the ecological sense;

(d) The related problems of biological tune, aging and death in the broader sense than that of either the physiological or biochemical approach; and lastly

(e) The problem of the origin of life, which in the
strictest sense may not be a scientific problem since conclusions concerning it cannot be precisely verified, has yet much significance in a theoretical sense, and deserves careful thought in light of any and all experimental data which may seem to have bearing upon it.

It is to such broad, dynamic problems of general zoology as these that the term, Zoodynamics, may best be applied. In a sense, it may be chiefly of theoretical nature, but not of necessity so far once a common ground of approach to such problems is found, much sound experimentation can and will follow. It is in the belief that field theory may offer such grounds of approach that the remainder of this paper is given to a consideration of the implication of field theory for these problems.

II. Field theory and the Problems of Zoodynamics.

The effort to interpret natural phenomena in terms of a conceptual system of explanation is inevitably attended by two possible misunderstandings on the part of the reader. The first is the feeling that the conceptual systems may explain how but never why; they are not advanced as causes of phenomena but as approaches to the problem of how causation acts. The second misunderstanding lies in the possible feeling of the reader that since a number of
phenomena find meaning in a conceptual system that such
system is being advanced as a more or less universal
explanation of everything. Such feeling on the part of
the reader is, at least, unfair to both himself and the
writer, since because of it, neither succeeds in accomplishing
anything.

These possible sources of misunderstanding are intended
as a preface to the following paragraphs in which it
shall be attempted to point out a possible conceptual
approach to several otherwise seemingly unrelated prob-
lems. The system advanced, the biological field, will
not be considered as an answer to these problems but as a
point of departure, in the light of which these problems
take on new meaning and perhaps a degree of relationship,
and from which fresh investigation both experimentally
and observationally may proceed.

A. The Problem of Organismic Unity.

Perhaps no single human observation of a natural
phenomenon has led to more conjecture, argument, postul-
ation and even strong contention than the very observable
fact of the unity of living organisms. Attempts to account
for such unity perhaps extends beyond our history of human
thought. Ritter's excellent history carries the problem back to Aristotle and Lucretius whom he considers as the authors, respectively, of what he calls the organismal and elementalist points of view. That contention on the subject probably existed earlier there is no doubt.

It is not our purpose here to trace the history of this observation further backward but to follow it forward in a general way and note the various problems that arose out of it. Such problems did not always have to do with unity so stated. To some it was the problems of mind and behavior; others that of development and life, itself. Yet all these are subtle manifestations of the basic problem of the living animal as a unified system and the problem of how that unity is attained.

Perhaps the oldest explanatory concept was that of the soul. Aristotle used it at least with regard to the human problem and Lucretius, though a proponent of the elementalist approach, seemed to sense such unity and the necessity for some concept such as the soul to explain it for he conjectures about the type of atoms which compose

28. See the early chapters of the first Volume Bibliography No. 4.
29. See the poem "De Natura Rerum" or Masson-The Atomic Theory of Lucretius-London, 1884.
the "Soul of the Soul."

This concept of the soul we see blossoming forth centuries later as the approach of vitalism, and in a sense continuing in the thought of some to the present. That the concept of vitalism has undergone change is true; what was a mystical and "supernatural" concept in ancient and medieval times has become what may be called an ultra-physical concept of the present and recent past. Examples of this latter might be sighted the concepts of Bergson and Driesch. Such distinction need not be dwelt on in this paper. The important fact is that vitalism of any kind seeks to solve the problem of unity by the postulation of a unifying force in addition to the physical organization of the organism which by its very nature is unapproachable and inexplicable from a physical approach, which seems to function as if "guided by intelligence."

In antithesis to this explanation has arisen from the elementalist approach of ancient times the explanation of mechanism today. As before, the history of mechanism is of less interest here than the nature of its explanation. In brief, its contention is that the problem of unity can

30. See Bibliography No. 84 for a recent view and those of Driesch (Bibliography No. 85, 86) for a somewhat earlier though still prominent approach.
be solved by the scientific analysis of the parts (or substances) which manifest such unity. However, apparently mechanism senses the inadequacy of such solution to the problem of unity as witnessed by the postulation of such ad hoc concepts the "organizers" which would explain ontogenic aspects of unity, instincts to explain unified response to environment as advocated by the behaviorists and others and the earlier mentioned concept of tropisms to account for sensitivity and response at the lower levels of life. That these latter concepts are of the same order and origin as those of the vitalist has been pointed out earlier. The chief difference, as also mentioned before, is that the mechanistic view holds them not as unapproachable by physical means but as amenable to further pursuit and analysis. That they are equally impotent to account for or offer on approach to the problem of organic unity, is implicit in their very nature.

The third approach to the problem, the organismal or organismic presupposes unity and thereby avoids the necessity of such concepts as the intelligent entelechy or the non-intelligent organizers to account for the unity manifest in ontogenetic development. The proponents of this view trace its origin in the recognition of the unity of
organisms by very early observers. That this concept as applied to the unity of the organism, alone considered, has eliminated much of the dilemma of both the vitalist and mechanist, is very evident. And by the experimental work of Childs, Herrick, Coghill and others, it has been placed upon a plane of possible scientific pursuit equal to any thing ever advocated, and more than ever accomplished, by the mechanists.

In the concept of metabolic gradients, we find the organization of the total system (the individual organism) as a product of the same dynamics as is the regulation of any part of that organism in the economy of the total individual. If the preceding paragraphs on the essentials of field theory be recalled, it becomes evident that such concept is implicit in the organismal point of view. It is field theory applied to the organism as a dynamic system. In so far as the organismal concept goes it is methodologically sound. The contention here is that its advocates have not carried it far enough, have not followed the concept to its logical fulfillment.

The problem of organic unity is a problem of more than the unity of organic parts and functions of the individual. The very materials of observation out of which the necessity of the organismal view arises, have to do with the behavior of the organism quite as much as with its
ontogenic development. Worded differently, the organism manifests not only unity in the inter-relation of its part but unity just as definitely in its relation to its environment. If a word to express this may be coined, it must be evident that such organo-environmental unity is the most observable phenomenon out of which the fact of organic unity has forced itself upon observers. Now it follows that, if one admits such organo-environmental unity, it becomes evident that any postulation of the unifiedness of the organism alone does not account for this unity of the organism in a transcendental system, the environment. Assume for a moment that the organism, alone, is admitted to have unity and that we seek to explain behavior in terms of impinging forces. We settle at once into the dilemma common to both mechanist and vitalist. How, in terms of impinging physical forces upon even an admittedly unified organism can we explain intelligent behavior? How bridge the gap between physical forces on one hand and intelligent, often seemingly spontaneous, response on the other?

The answer lies in extending the same logic that has lead to the organismal concept of organic unity to include the organo-environmental unity, to include organism and environment, the biological field concept. Then will
not only the fact of organismal unity be meaningful but likewise the problems of unified behavior. As unity of the organism explains structure and function as different aspects of a dynamically controlled system, the organism, so would the problems of control, direction, spontaneity, etc. of behavior become aspects of a similarly controlled larger system of which the organism is a highly organized, unified part. The necessity for such extension of logic has been sensed by those who have done most to establish the organismal concept. Ritter\(^3\) tows with it in one work of several years ago and Child, in postulating how metabolic gradients first came into being, found it necessity to assume that his primordial protoplasmic droplet was first stimulated to unequal metabolic rate by a surrounding light differential. Other examples of such intimation could be sighted. It is not surprising. Rather may one wonder that more has not been made of field theory in this direction since it exists so implicitly in the organismal concept.

B. The Problem of Purposive Behavior especially in the simpler animals.

As suggested a few paragraphs earlier, it is difficult

\(^3\) See parts of "The Probable Infinity of Space and Time"
to separate the problem of behavior from that of the unity of the organism, since before knowledge of embryonic development caused observers to wonder at its unified progression, behavioral unity impressed itself upon the thoughtful. Especially is this true of the higher types of behavior, purposive behavior, and particularly when seen to occur in small or simpler animals. One cannot think of the elaborate responses of nesting birds, of the social life of bees, wasps and ants, of the seemingly purposeful responses of the protozoa, even, without sensing the vast complexity of the problem. It is in no sense to minimize such complexity, nor through a lack of appreciation of the truth of it; that a few suggestions on the subject are ventured here.

The one explanation of such complex, purposive behavior that has extended down to even the present, and the only one that is "scientifically" acceptable, is that such behavior is instinctive. Where behavior seemingly cannot have been learned, it is said to be instinctive. The bird builds her first nest by instinct; the duckling swims the first time in water by instinct, etc. Farther we are told, when asking more specifically what instincts are, that instincts are inherited behavior patterns, something automatic passed on from generation to generation that is
at the same time, both the cause of behavior and the means by which such behavior is accomplished.

Now a number of questions or objections might be raised to the instinct concept: If instincts are inherit-
ed behavior patterns automatically followed by the organism how account for the individuality, the variety, even the originality and spontaneity with which so-called instinctive acts are performed? Many, many others might be (and have been) asked. Rather we would point out two serious weaknesses of a more fundamental order. The first is that the instinct concept does not explain anything for it indicates no means by which causation acts. It is a name, a word applied to all behavior not otherwise explicable at the time. It is not even descriptive for even its most ardent advocate can not say just where acts cease to be of a learned type and become of an instructive type in the behavior of any animal. The second weakness lies in the fact that is is an unapproachable type of concept. There is no means of pursuing it scientifically. It must be assumed to be a "something" within the organism and "innate properties" have little place as concepts in modern methodology.

There are several reasons for feeling that field theory has much to offer as an approach to the problem of purposive behavior regardless of the level of organism manifesting it.
First is the fact that field theory approaches the problem in the light of different assumptions. It assumes the unity of the organism and environment; stresses in fields account for behavior rather than impinging forces; the organism seen as a part of a larger system, participates in all the dynamics of that system and hence all behavior in one sense is both pruposive and spontaneous. The extreme complexity of fields when considered as existing in a space continuum of four or more dimensions makes the concept adequate to even the complexity of life responses and processes. True it may be a long time before scientific procedure can control or predict in terms of such complexity. Yet such admissions do not discredit the concept and it does have the possibility of being scientifically approached in so far as technique does exist. Finally from a methodological point of view the concept makes meaningful even so complex a phenomenon as purposive behavior in terms of simple principles. Just as the concept of gravitation is a simpler explanation of the roll of the ball down the inclined plane than any assumption of an Aristotelian order that there is a "something" innate within the ball that causes it to seek the earth so also is the explanation (for example) of the accurate return of the homing pigeon in terms
of a complex response to a complex system of dynamic fields simpler, more approachable, more scientific in so far as it opens the problem for investigation than any postulation of instincts, unlearned hereditary patterns, innate tendencies, etc. to account for such return. And, in the light of field theory, both the development of the organism and its behavior become meaningful in terms of the same type of concept.

C. The Basic Problem of Evolution.

Second only to the problems of unity and of behavior of organisms in point of historic interest and persistence is the problem of the evolution of animal groups. (The term "evolution" is used here with complete realization of its limitations, of its ineptness as a term to describe a process. However we have no other word to describe the decent of organisms with modification that is so commonly known and whose meaning in a general sense needs less explanation. For that reason, alone, the word will be used.) From the days of Anaximander (611-546 B. C.) to the present the problem has found expression in various theories that, though stressing certain aspects, have failed to touch the central problem. Simply stated that problems would seem to be this: A seeming paradox exists in that
life flows on from generation to generation yet the animal forms that are that life-stream are constantly changing, new species arising, new modifications appearing and disappearing. How can the great permanence of life be attained through the constant change of the living? How can this living stream flowing through time change and yet not change? Most of the theories advanced have sought to present a method by which such change could occur. Few have offered any explanation of the permanence aspect of the phenomenon. Fewer still have in any way accounted for the teleological aspects of the process. Of Darwinism is this especially true since a concept of negativism was advanced to explain the process of a positive progression. The Weissmanian concept of the immutability of the germplasm was a mechanistic attempt to explain the permanence aspect of the process of evolution. That it was nothing more than an ad hoc hypothesis attached to the already inadequate Darwinian concept, cannot be escaped. The Lamarckian concept likewise avoided the continuity part of the problem as well as being too simple a concept to bear up under experimental testing. It did have the one virtue of admitting a certain immediate teleology. Other than for that, it was a weaker concept than the Darwinian explanation.

Little need be said of the various non-evolutionary
explanations: successive special creations, etc. Many would ascribe them entirely to the ecclesiastic influence of the age of their postulation. To do so without realizing one significant fact is scarcely fair or wise, and that one fact is all that has interest for this paper; even the special creation theories arose out of the sensing of this problem of permanence through change. The fact of different creations would account for change and the supernatural nature of those creations, for the problem of continuity. And so the problem of the basic nature of evolution rests today. The paradox of permanency through change is still unanswered.

The concept of biological fields would seem to hold the answer to this paradox. If the evolving stream of animal life be looked on as a field phenomenon in time, the answers to the three major questions of evolution are at once evident. First its teleological aspect is of the same forces which account for progress, itself. In the second place, change of parts in dynamic fields, whether of relationship or of individual organization, are of a differentiative order, which would account for the progressive aspect of evolutionary change. The fact that forms evolve divergently from similar or common ancestry; each stage in
turn being potentially the root of future divergencies, etc. Thirdly the permanency aspect of evolution finds explanation in field theory in the fact that the whole is transcendental to the part. The part exists only in terms of the whole and differentiation of the part obviously cannot carry it beyond the whole since all causation in fields is from whole to part. The permanency of fields lies in the infinite capacity of their parts to change. Think of the simple physical example of this principle embodied in the whirlpool in a stream. The whirl is relatively permanent due to the very fact that its water content is changing every instant. The very forces which makes for the change (the stream current), make for the permanency of the whirl. Both are the result of the on-going flow of the water (which, we might add, is a field product, itself, due to a differential of elevation in the gravitational field!)

In concluding this suggestion as to the possible applicability of field theory to the basic problem of evolution, three things may be said in summary. First, the basic problem of permanency through change seems to find meaning in terms of field theory which cannot be said for other evolutionary hypotheses. Secondly, wherein certain concepts of the various evolutionary theories have value
(for example, the "web of life" concept of Darwin) field theory in no sense invalidates them. (In fact the "web of life" concept is a dynamic field concept had its logic ever been further pursued by Darwin or successors) Thirdly, if the evolutionary process ever becomes the domain of an experimental technique, such technique will have to proceed in terms of dynamic assumptions such as none of the various evolutionary theories provide. That field theory does provide such approach seems quite as true here as in other problems to which its application has been suggested. The technical difficulties of experimentation that are obvious in so complex a field are, in no sense, neglected in this suggestion.

D. The problems of biological time, aging and death.

There are several problems arising out of the metabolic processes of animals that are today little more than appreciated. That they may become the domain of research and investigation in the future seems largely dependent on the postulation of a theoretical approach from which such investigation may proceed. Our point here is briefly to suggest that, indefinite as these problems are at the present time, field theory may seem to offer not only this approach but serve, as well, in showing their common,
It has been pointed out earlier that life processes definitely occur in a quadri-dimensional continuum. Time is ever an aspect of life phenomena. Yet due to the vast differences in the rates at which these processes proceed, chronological time is, at best, a very poor measure of their progress. If such processes be thought of as field phenomena, it is not difficult to see them as activities moving toward equilibrium, which is never attained due the progressive differentiation of the living system, at least until death occurs. Further it may be supposed that such equilibratory activity is proceeding simultaneously in fields in many conceptual dimensions which would account for the extreme complexity of these processes when viewed as physical activity in terms of physical space and time. That such activity might in certain fields be synchronous and in others, asynchronous would account for the fact that life processes are so largely of a cyclical or rhythmic order. The fact of these rhythms is well known not only in their physiologic but ecologic manifestation. If they arise as has been suggested, then it would seem that in terms of these rhythms is the real measure of the biological time dimension. These rhythms better measure the aging of the organism than any chronological units could ever do.
True, an immense amount of work needs to be done on such rhythms to establish their periodicity, uniformity, etc., but once a body of data is accumulated on the subject it would be of great worth to the applied biological sciences, medicine in particular.

It has been suggested in the preceding paragraph that, from the viewpoint of field theory, death is probably the attainment of equilibrium in these fields where life stresses have assurred. As a biological phenomenon it has received too little attention from research. Many aspects of it might be pursued with value in terms of increased understanding of life processes, since it seems to be the only example in biological fields where equilibrium is attained. Is the progressive nature of death that may be observed the result of equilibrium in certain fields bringing about a simplification of the system to such point that differentiation cannot continue? Is this the explanation of death being the cessation of the organism in time, the termination of that "on-goingness" that is so characteristic of the living? These questions, though unanswerable and speculative now point the direction for much sound experimentation.

8. A speculative word on the possible origin of life.
That the problem of the origin of life is not a scientific one, the writer well appreciates. It is not scientific since in the very nature of things any answer cannot be checked for its validity. If the day ever comes when living matter is synthetized in the laboratory the problem will still lack such validity check since the synthetizer will still be unable to show that he has found the means by which life first came into being. He will have shown one way that it could have come into being; nothing more. So any discussion such as that which follows must be theoretical, evaluative and largely philosophical. Its only value is that, if a system of explanation can be extended to make theoretically meaningful the problem of the origin of life, such system takes on increased power of unifying thought in fields where validity can be checked. That alone justifies this speculative word.

Human thought has always found it difficult to entertain the concept of existence without the associated concept of the beginning of existence. Human logic too often fails to sense the inescapability of the fact that a beginning presupposes that which existed before the beginning, that limits imply the limitless beyond. That perhaps explains the Mosiac concept of the universe (the earth to that period) as once having been "without form and void." Human consciousness has its beginning, its run
and its close and so, to the naive mind, must everything else.

Modern physics in its theoretical aspects, has quite completely passed such thinking. Utterly illogical is the concept of order coming out of chaos, or that of matter without form. Whitehead\textsuperscript{32} postulates the view that organization is infinite in time and extension. Matter without organization is as inconceivable as thought without ideas or sensation without the sensible. In physics, field theory shows form (organization) and substance as but different aspects of the same reality. If matter is infinite then so must be the form of matter and the laws of which matter is the expression.

Slowly modern thinking has accepted this concept of the infinity of the physical system. Such acceptance, however, has not settled our biological speculations. This, I think, is true because we have been prone to think of biological phenomena as being essentially different from the physical. No matter if the physical universe is of infinite duration the essentially different biological world must have had its beginning. There lies the error of our thinking; there is where field theory applied to biological phenomena can point the way out of a dilemma.

\textsuperscript{32} See Bibliography No. 87.
otherwise without solution.

The concept of biological fields, as repeatedly shown, would explain the phenomena of the living by exactly the same laws as those of non-living systems; the difference being one of phenomenological complexity, not of principle involved. From such a view then there is no great distinction between living and non-living systems; they represent but different degrees of complexity in the expression of the same laws or principles.

Yet this fact cannot lead us to conclude that living systems are of eternal organization. Our knowledge of the history of this earth quite precludes the thought that living systems, because of the narrow physical limits in which they can exist, have always been in existence here as such. However, the fact of the progressive differentiation of fields would seem to hold the answer. As fields undergo differentiation, their phenomenology becomes increasingly complex. That living systems are the differentiation products of fields that at simpler levels we would call physical systems is quite conceivable. That we know of no systems intermediate between the living and the non-living, so-called, is in itself no objection. There are two possible explanations: first, such intermediate
systems might be ultra microscopic, as are thought to be the microphages and filterable viruses known of today. The second, and preferable, answer lies in the fact that we have arbitrarily divided the living from the non-living so sharply that we seem utterly unable to conceive of differing degrees of complexity in living systems, or the fact that the simplest living system may be only very slightly removed in complexity from the most complex non-living system, if we could make careful comparisons in an experimental manner. Of course, living systems are assumed to have certain properties characteristically not manifest by physical systems. Of the higher living systems this is certainly true. Yet we know little of the degree to which these characteristics may diminish in the simplest living systems and less still of the extent they conceivably could exist in the most complex non-living systems. That this "border land" between the living and the non-living is not so wide and profound as formerly thought is forced upon us by the fact that living protoplasm has been crystallized into a seemingly non-living substance which again assumed life activity when once more in a suitable situation. The long periods of encystment and dormancy of simple forms may be explained by some similar change in organization which
which would account for their great impregnability in these stages. Such is not unthinkable—we know little enough of the metabolism of cystic forms!

Our final conclusion must be that, in terms of field theory the distinction between living and non-living systems is one of degree of complexity of phenomena and not one of difference of principle. Hence it is conceivable that the simplest physical system and the most complex living organism are but the extremes of a universal gradation of organization. Thus would field theory suggest the differentiation of living from non-living systems since both have the same principles basic to their function and existence.
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