has accepted the post of dean of the faculties of Pennsylvania State College.

THE following appointments have been announced for the medical department of the University of Pennsylvania: Dr. Edward Lodholz is to be assistant professor of physiology; Dr. W. N. F. Addison, assistant professor of normal histology; Dr. George H. Fetterolf, assistant professor of anatomy; Dr. L. A. Ryan, assistant professor of chemistry and toxicology.

DR. E. T. WHITTAKER, F.R.S., royal astronomer of Ireland, has been appointed professor of mathematics in the University of Edinburgh, in succession to the late Professor Chrystal.

DISCUSSION AND CORRESPONDENCE

REPLY TO HOLMES'S CRITICISM OF "LIGHT AND THE BEHAVIOR OF ORGANISMS"

IN a review of the book entitled "Light and the Behavior of Organisms," which appeared in this Journal, June 23, 1911 (pp. 964–966), the author raised several points that call for elucidation. Before entering upon the discussion of these points, however, I wish to take this opportunity to state my regret in having overlooked the work of several investigators bearing on some of the subjects treated, especially that of R. S. Lillie on the reactions of *Arenicola* larvæ, to which Holmes calls attention.

After referring to the numerous attacks made in the book in question, on Loeb's theories of orientation, Holmes says (p. 964):

> Mast's own investigations seem to afford about as good support as has been furnished for the theory which he so persistently attacks.

He then gives two cases in support of his contention:

> 1. No clearer case of orientation through the local response of the part directly stimulated could well be imagined than the one afforded by *Amœba*, and the author admits that the "method of orientation is in harmony with much in Verworn's theory and also with the essentials in Loeb's." But he adds that "it does not, however, support the idea connected with these theories, that a constant intensity produces a constant directive stimulation." I am not sure that I understand the pertinency of the criticism, for there is nothing in the theories of either of these writers which implies that the actual stimulating effect of any directive agency is subject to no variation.

2. Referring to orientation of *Arenicola* larvæ he says (p. 965):

> Orientation in this form is apparently as automatically regulated an activity as one might expect according to the well-known theory of Loeb.

The point at issue here clearly concerns the question as to whether the methods of orientation in *Amœba* and *Arenicola*, as described in my book, are in accord with Loeb's theories of orientation. To settle this question it is of course necessary first of all to understand these theories. I say theories, for, contrary to my critic's assumption, there are three instead of one, as pointed out in my book, pp. 23–35, especially in the summary (p. 54) where the following statement is found:

> In 1888 Loeb held that orientation in animals is controlled by *the direction in which the rays of light pass through the tissue.* From 1889 to 1903 he advocated the idea that orientation is controlled by the *direction in which the rays strike the surface, or the angle they make with the surface.* His statements from 1906 to 1909 indicate that he thinks that orientation is regulated by the *relative intensity of light on symmetrically located sensitive structures on opposite sides of the organism.*

The idea that orientation is the result of continuous action of light is common to all of these theories and is undoubtedly their most important distinguishing characteristic. Loeb has repeatedly stated this in unmistakable terms. Witness, *e. g.*, the following statement found in "Dynamics of Living Matter" (p. 135): Heliotropism is "a function of the constant intensity," and the same idea expressed more fully in the same publication on pp. 117–119, 130–131, 138–139. My critic has evidently failed to grasp this idea in spite of the fact that I have repeatedly stated it in different forms in quotations from the references just given and others, indeed even to such an extent that one of my reviewers objects to the repetition as superfluous.

In order to show that an organism orients in accord with Loeb's theories it is consequently necessary to prove that the response is due to "constant intensity," *i. e.*, that the orienting stimulus acts continuously and is not due to changes of intensity. This I have been unable to do in any case whatsoever in spite of persistent efforts, and as far as I am aware it has never been done. It was owing to this that I made the statement that the orienting reactions in *Amœba* do not support the idea connected with the theories of Loeb and Verworn that "a constant intensity produces a constant directive stimulation" quoted by Holmes. His failure to "understand the pertinency of this criticism," as he says, indicates that he did not understand these theories.

The second case which Holmes cites in support of his contention that my investigations lend support to Loeb's theories shows even more clearly than the first that he did not understand these theories. After admitting that in case of the orientation in *Arenicola* larvæ "the question remains open whether the stimulus is produced by the direct action of light on the sensitive surface of the animal or by changes in intensity of the stimulus caused by the lateral movements of the body" he maintains that orientation in these animals is in accord with "the well-known theory of Loeb" because it is "*apparently automatically* . . . regulated" (italics mine). Thus admitting that it can not be proved that orientation is due to continuous stimulation, he would make automaticity the criterion of Loeb's theories, ignoring all other distinguishing characteristics found in them.

It is true that Loeb often uses the term automatic in discussing reactions which he calls tropisms, but what does it mean? Automatic means mechanically self-acting, that is, involuntary. In accord with this definition are there any reactions whatsoever in *Arenicola* or in any other organism below man which are not automatic? Investigations and speculations without end have been directed toward the solution of this very problem and yet there are few bold enough to say that it has been solved for even a single case. Of what possible value then can automaticity be as a distinguishing characteristic of a group of reactions supposed to be specific, and what bearing can the statement that the orienting reactions of *Arenicola* larvæ are *apparently automatic* have on the mechanics of orientation (tropisms) of this or any other organisms?

It is precisely such loose and uncritical statements as Holmes has made regarding Loeb's theories with the suggestion of such impossible criteria as automaticity that have brought the discussion centering around the term "tropism" to the chaotic condition in which it is found at present. To show that a reaction is in accord with any one of Loeb's three theories it must of course be demonstrated that it is in harmony with all of its characteristics, not merely with one of them, as my critic seems to imply. It must be proved, among other things, as demonstrated above, that the external stimulating agent acts continuously in the process of orientation. Until this is done Loeb's statement that orientation is a function of the constant intensity must be classified as anthropomorphic speculation. And if the statement of my critic is true, that my investigations afford about as good support as has been furnished for Loeb's theories, it is evident that they rest on extremely nebulous foundations. If Holmes can produce a single case in which he can prove that orientation occurs in accord with any of these theories I trust that he will do so in answer to my reply to his criticism.

My discussion concerning certain theoretical views held by Jennings brings out the most caustic criticism that Holmes has to offer. He quotes the following paragraph from my book:

> Every step in the development of the theory [Jennings] is supported by numerous experimental facts and all seems to fit what is known concerning the reactions of organisms. Reactions, according to this theory, are, as stated above, primarily due to physiological states. External agents ordinarily produce reactions through the effect they have on these states. By the application of this idea all the different phenomena connected with reactions to light as summarized at the beginning of this chapter can be accounted for.

This quotation is followed by a paragraph containing the following sarcastic remarks (p. 966):

It would indeed be comforting to be able to repose with such a spirit of confidence and contentment in a general philosophy of behavior, but it is perhaps pertinent to enquire if the author has not been deceived with the delusive appearance of explanation where no real explanation has been given. . . . Phenomena may thus be "accounted for" on the basis of varying internal states, but as it is admitted that in most cases we are entirely ignorant of what these states are we are about as much enlightened as we are by the celebrated explanation of the sleep-producing effect of opium by attributing it to a dormitive principle.

If I had said nothing more than is contained in the paragraph which Holmes quoted from my book there might be some excuse for such criticism, but the very sentence following this paragraph reads:

But what are these physiological states and of what do they consist? That there are such states in organisms can not reasonably be doubted, and that the reactions are dependent upon them much as Jennings assumes, seems to me to have been well established in his work. But what regulates the physiological states is a question concerning which we have as yet but little knowledge.

The two pages in my book following this quotation are devoted to an attempt to illustrate the limitation of Jennings's ideas, and it is concluded (p. 375):

For all that is known to the contrary, subjective factors, entelechies, or psychoids, factors foreign to inorganics, may have a hand in controlling physiological changes and consequently the reactions. Such factors have been postulated by the vitalists and neovitalists, notably by Hans Driesch.

I am at a loss to know how my critic could have read even superficially these statements and the argument connected with them and still conclude that I had been deceived with the delusive appearance of explanation where no real explanation has been given. I am not certain what Holmes means by a real explanation, but I am certain that neither Jennings nor I has ever even so much as intimated that the demonstration that reactions of organisms are dependent in a definite way upon internal states constitutes a complete explanation of behavior. In the paragraph which Holmes singled out for attack with reference to this question I did not even use the term explanation, merely stating that the reactions could be "accounted for" by the application of certain ideas regarding internal states. Thus it is evident that his caustic criticism is directed not toward anything actually stated, but toward an imaginary implication.

However, to intimate as Holmes does in the second quotation given above, that a demonstration of the actual value of internal factors in behavior is useless because it is not known precisely what the internal factors are, is expressing a principle which if applied generally would at once do away with scientific investigation, for is it not well known that science in all of its aspects rests upon phenomena which are clothed in mystery? A scientific explanation, as I see it, consists of a demonstration of the order of events involved in the phenomenon. The demonstration that in the observed phenomena known as behavior events within the organism occur in a certain order in the whole series of events ending in these phenomena (behavior) is as truly an explanation as any we have in science. It is one step in the series, even if only a small one, which, as far as can be predicted at present, leads back into the unknown without end.

I can understand the statements of Holmes in his criticisms only on the assumption that he reviewed my book hurriedly and carelessly. As evidence of this we have not only his erroneous conceptions regarding Loeb's definition of tropism and his failure to grasp my ideas in the discussion of the theoretical views of Jennings, but also the fact that in his short quotation from my book there are three changes from the original. Moreover, his statement (p. 965) that I have presented "no discussion of any theoretical attempt to explain the reversal of phototaxis" is not true, as reference to page 370 will show.

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