Diphasic Liquid Systems and Bio-electrical Phenomena.—A Reply to Criticism.

BY

R. BEUTNER.

IT is generally recognized that an attempt should be made to apply physical and chemical laws to all biological phenomena. It has been a strange feature of the special field of electro-physiology that an application of this kind has been almost wanting. A number of peculiar theories were extensively discussed, viz. the 'pre-existence theory' (E. du Bois-Reymond), the 'alteration-theory' (R. Herrmann), the 'theory of ionic selective permeability' (W. Ostwald, Bernstein, and others), the 'lipoid theory' (M. Cremer), &c. Little was contributed from physical and chemical experimental work, so that a gap really existed between electro-chemistry on the one side and electro-physiology on the other.

This was partly due to the failure of older physiologists (du Bois-Reymond, Herrmann) to furnish precise quantitative measurements relating to biological electric currents. *It is principally to the merit of an English physiologist, J. S. Macdonald, to have given, for the first time, such quantitative data.* Based on an extensive series of measurements of the current in a frog's nerve, he found that a regular relation existed between the electric current of the nerve and its salt content. One of his chief conclusions may be fully quoted: '*There is no sign of any critical point marking the separation of two possible phenomena, one a function of the condition of life, and the other a physical phenomenon dominated by the salt-content of the nerve and capable of continuation after its death*' (Proc. Roy. Soc., lxvii. 310).

This most important work was one of the starting-points for the present author's investigation of the physico-chemical nature of biological currents. In the laboratory of the well-known American biologist, J. Loeb, extensive electric measurements on plants were made by him which revealed the same phenomena in a more precise manner; later the author succeeded in imitating closely these electrical phenomena by means of synthetic organic substances.¹

¹ A description of all results obtained is found in the book of the author on Entstehung elektrischer Ströme in Geweben und ihre künstliche Nachahmung, Stuttgart, 1920.

As a detailed description of these experiments is not possible here, it may be stated that the conclusions of Macdonald, cited above, were completely confirmed. Further, it was possible to make clear the nature of the physiological law which he discovered. The logarithmic relation, between the salt content and the electromotive force, observed by him in the nerve, was traced as a property of certain systems composed of well-known organic substances (immiscible with water) and aqueous solutions.

It is very unfortunate indeed that the work of Macdonald has been entirely neglected by electro-physiologists, apparently because it contained little theoretical discussion of well-known hypotheses; it merely presented the results of a large number of careful measurements.

The majority of physiological workers were not inclined to accept such experimental results as a basis for further progress. It seemed more important to them to find new arguments *for* or *against* any of the existing theories. Macdonald's work was published in 1900 in the most prominent English scientific journal; hardly any quotation of it is found in later research work, however. No one of the contemporary physiologists pointed out the great importance of the success he had achieved. The attempt to continue the line of research inaugurated by Macdonald—which was undertaken by the author as described above—was doomed to meet the same disregard. This is clearly manifest in a criticism published by Dr. Dorothy Haynes in this Journal, a short time ago.¹

Dr. Haynes begins her criticism by pointing out the importance of the statement 'that it is the salt content of a cell which determines its electric behaviour'. 'Such a view, if substantiated, is of fundamental importance for physiology, and no further justification will be needed for a careful review of the evidence upon which it is based.' Referring to the statement of Macdonald cited above, it is obvious that the evidence in question was brought forward by him 23 years ago; since the statement of Macdonald is based on experiments which admit of no other interpretation it is hardly possible to speak of a 'view' in this case. No priority in regard to this statement should be attributed to the present author, as is done by Dr. Haynes, who apparently was not cognizant of the work of Macdonald.

The aim of the present author was to imitate artificially Macdonald's '*phenomenon dominated by the salt content*', which he describes as not being separated from the '*function of the condition of life*'. Some of the numerous systems which were found to exhibit an analogy with this phenomenon contain salicylic acid. Dr. Haynes tries to prove that in these cases the effect of the salt content is *really an effect of an acid as the adding of a neutral salt is assumed to increase the acidity*. This would mean that adding 1/10 mol. NaCl (or KCl) should decrease the pH value of a saturated

¹ Vol. xxxvii, p. 96, 1923.

solution of salicylic acid from about 3 (which is roughly the pH of saturated salicylic acid) to at least 1, since the electromotive force of the system

saturated aqueous solution of salicylic acid without KCl	oil intermediate conductor, saturated with salicylic acid	saturated aqueous solution of salicylic acid containing KCl
--	---	---

is higher than 0.12 volt (compare table on page 147 and 148 of the book of the author), and since each 0.057 volt corresponds to a decrease of the pH value by one unit, as is well known. A decrease of pH from 3 to 1 would mean an increase of the H-ion concentration of about 1/1000 normal HCl to 1/10 normal HCl, and yet such a concentration is brought about by the mere addition of KCl. Although the impossibility of this assumption is apparent, the author has performed a measurement of the pH of a saturated solution of salicylic acid (= 2, 8) which appeared not to be *affected to more than 0.1* by the adding of 1/10 mol. KCl.¹

No experimental evidence relating to salicylic acid, nor any quantitative calculation of the pH-values, is found in the criticism of Dr. Haynes. She asserts, however, that the author 'has altogether failed to substantiate his statement that salts rather than acids play the predominating rôle in the systems he investigates'. 'A careful review of the experimental evidence shows the differences of potential which he obtains can be correlated with a difference of H-ion concentration whenever free acid is present in the oil and that the theory of salt action is derived from a misinterpretation of the complex chemical systems with which he [the author] deals.'

In connexion with these assertions another experimental conclusion of Macdonald may be quoted here: 'Solutions of NaOH, HCl, NaCl, or KCl mainly affect the demonstrable value of the demarcation source according to their concentration.' This shows the impossibility of potential differences correlated with the H-ion concentration being present in tissues, since pH is, of course, affected by HCl-concentrations in the opposite sense to that by NaOH-concentrations.²

No further details of the physio-chemical objections raised by Dr. Haynes need be discussed here, part of them being due to ascribing assumptions to the author which he has never put forward, e. g. the assumption of

¹ In an article in the *Biochem. Journal*, xv. 440-61 (1921), Dr. Haynes describes measurements on the action of salts upon buffer solutions and finds that the adding of N/2.5 KCl decreases pH from 6.88 to 6.7 (with a phosphate mixture instead of salicylic acid). It is obvious that in this case, also, the decrease of pH is far too small to account for such powerful electromotive effects as may be produced by the salt content.

² Macdonald's statement is based on experiments with animal nerve. A similar experiment on plants was described by J. Loeb and the author (*Biochem. Zeitschr.*, lxi. 15, 1912). It was found that the potential difference on the cuticle of a plant was not changed if HCl or NaOH was added while the salt content was kept constant. The large range of pH between 3 and 11 was found to be without any influence. It is very striking to compare these results with the statement cited above, that 'the potential difference can be correlated with a difference of H-ion concentration'.

the non-existence of diffusion potentials in 'oils'.¹ Many details relating to the book of the author are described by Dr. Haynes as 'curious mis-interpretation', 'faulty interpretation', or as 'being very hard to understand', for all of which she appears to think the author is to be blamed.

In order to solve the problem of the origin of electric currents in tissues it seemed of prime importance *to find substances which actually reproduce electromotive phenomena observed with a tissue*. After years of experimental work the author succeeded in finding some such substances. This could only be done by *experiments*; the detailed questions about the physical nature of the electromotive action of those substances could be solved only after they had been found. In the old electro-physiological theories, however, no definite substances were known which might have been responsible for electro-physiological effects. Instead, hypotheses were put forward—e. g. in the 'membrane theory'—about the possible properties of the 'plasma membrane', the composition of which was entirely unknown.

The author is well aware of the need of further developing the results obtained so far. Every criticism would be welcome which—on the basis of experimental evidence—would indicate what substances might serve as better models for the reproduction of biological currents. It is hard to understand, however, how a justified criticism can be based on the statement that the new method 'cannot take us very far in the interpretation of the cellular mechanism of the cell.'²

Such a statement indicates the influence of the old alteration-hypothesis of L. Herrmann and his followers, to the discussion of which many volumes have been devoted, especially in Germany. As a matter of fact, nothing definite is known so far about cellular mechanism and little can be expected from further discussion of the matter, or by a prophecy that any experimental method holds no possibilities of future developments.

'Apple skin with its coating of wax and specialized nature is very far removed from the plasma membrane of the living cell' is another criticism of Dr. Haynes. Macdonald, however, has shown that *the nerve* also exhibits an effect 'dominated by the salt content'. Further experiments described in the older literature show that all kinds of other tissues (muscle, &c.) exhibit a similar effect. Also, the effect of the salt content is *of the same kind in all cases*, and it certainly is, therefore, quite a general biological phenomenon. The author holds that on the biological side no criticism of his experiments is possible without due reference to the most important work of Macdonald, who was the first to observe the electro-physical effect of the salt content.³ Since Dr. Haynes does not even mention

¹ A whole chapter in the book of the author (pp. 72–86) is devoted to this question. On the basis of experiments it is found that diffusion potentials do exist in oils.

² Dr. Haynes, loc. cit., at the end of p. 101.

³ Similar experiments were described by Biedermann (1880) and others, who found that water has

Macdonald it seems doubtful whether her statement cited above is justified, as well as the following ones: 'On the biological side Beutner lays himself open to serious criticism.' 'In his theory of the current of injury . . . Beutner assumes a simplicity and uniformity of structure of the living organism to which no physiologist can give countenance.' As a matter of fact the experiments of the author on the current of injury in plants point to a more complicated structure than assumed by any of the former theories, since the possibility of an asymmetry of the membranous constituents, giving rise to an electromotive force, had never been suggested before. The assertion of Dr. Haynes that the author 'postulates for the fruit a homogeneous flesh' is entirely unfounded and contradicted by the experiments described on pp. 140-3 of the book of the author.

It must be stated, finally, that Dr. Haynes even denies the existence of statements which are manifestly to be found in the book of the author. Concerning the higher content of fatty acid in the outer skin she declares 'that he [the present author] does not state whence his information is derived'. Although the author is doubtful whether his arguments would carry conviction to Dr. Haynes, yet he begs to say that the source of his information is his own book, where one full page (p. 145) and a table are devoted to explaining that the above statement is derived from the greater electromotive effect of the salt content on the outer skin.

a positivizing affect on the muscle, which proves that the effect of the salt content is a general phenomenon. None of these investigators, however, thought of establishing quantitative relations between salt content and electromotive force. This was first done by Macdonald.

Beutner, R. 1923. "Diphasic liquid systems and bio-electrical phenomena.—A reply to criticism." *Annals of botany* 37, 673–677.

<https://doi.org/10.1093/oxfordjournals.aob.a089872>.

View This Item Online: <https://www.biodiversitylibrary.org/item/270686>

DOI: <https://doi.org/10.1093/oxfordjournals.aob.a089872>

Permalink: <https://www.biodiversitylibrary.org/partpdf/319108>

Holding Institution

New York Botanical Garden, LuEsther T. Mertz Library

Sponsored by

BHL-SIL-FEDLINK

Copyright & Reuse

Copyright Status: Public domain. The BHL considers that this work is no longer under copyright protection.

This document was created from content at the **Biodiversity Heritage Library**, the world's largest open access digital library for biodiversity literature and archives. Visit BHL at <https://www.biodiversitylibrary.org>.