DISCUSSION

A THEORY OF PROTOPLASMIC STREAMING

PROTOPLASMIC movement is now recognized as a general and fundamental property of metabolically active cells. It has risen to this position from the controversial one held not many years ago when the streaming of protoplasm was looked upon by some physiologists as a pathological condition in opposition to the view that it is evidence of a normal and healthy state.

The force responsible for protoplasmic flow has been the subject of much lively and stimulating speculation. My earliest memory goes back to the days when surface tension, the *sine qua non* of so many cellular activities, was regarded as the energy source of protoplasmic streaming. There was also the suggestion that the one-way "shuttle" type of flow in the filaments of coenocytes, where movement is first in one direction and then in the other, may be due to hydration at one end and dehydration at the other end; but the protoplasm flows equally well in both directions, even when fully submerged. Obviously, dehydration can play no part.

Physiology next entered the colloidal epoch when it became apparent that many cellular activities had their counterpart in colloidal systems. There thus arose the suggestion that protoplasmic streaming is a cataphoretic migration of particles or the electro-endosmotic flow of an aqueous medium; if either, it is the latter, for in streaming protoplasm the entire mass of material moves and not just the suspended particles. In spite of some attempts to prove that streaming protoplasm is associated with electric potentials, there has been no convincing evidence that the potentials measured are real, in the sense of innate to the protoplasm, and if real, are the cause rather than the result of the streaming. Most electrical determinations made on cells and tissues are subject to the criticism that the potentials measured may not reside in the living material alone, but are produced by the experimental equipment as a whole. This statement is in no way intended to deny or question the existence of electric potentials in organisms; on the contrary, all necessary conditions for the production of potentials are present, but do the potentials measured exist in the living material?

Should the source of energy responsible for protoplasmic streaming be found, there would still remain the question, why the direction of flow reverses in the shuttle type of streaming. The protoplasm of slime-molds flows but one way at a time. Were a potential responsible there would have to be a reversal of it every forty or fifty seconds, and so the problem takes on a two-fold aspect—why the streaming, and why the reversal?

The slime-molds, or myxomycetes, sometimes referred to as mycetozoa, possess a body or plasmodium which is a non-cellular protoplasmic mass with ever-shifting channels of flow. A brief study of slime-mold plasmodia is sufficient to suggest the presence of a nearly perfect rhythm in the reversal of direction of flow. The average of many time records gives forty to fifty seconds for each period of flow. The minimum time is twenty seconds, and the maximum sixty between reversals. (The average maximum rate of flow is 0.07 mm per second.) The foregoing limits in time of flow suggest a lack of rhythm, but most often the periods of flow vary little from the mean. Pronounced divergences from the average of forty-five seconds are to be attributed in part to physiological disturbances in an artificially mounted specimen of plasmodium.

My own motion pictures of slime molds had all been taken at moderately high magnification and at normal speed. I therefore failed to see the essential feature of the rhythm. This first became obvious to me on viewing the excellent cinematographs of slime-molds taken by Drs. J. Comandon and P. de Fonbrune, of the Pasteur Institute at Garches, France. My visits to these laboratories have always been profitable. The first films of slime-mold plasmodia by Dr. J. Comandon and Professor P. E. Pinoy were made as long ago as 1912.

When the moving-picture of a small plasmodium is taken at one hundredth of the normal speed and projected at the customary rate, the entire plasmodium is seen to go through rhythmical contractions and expansion similar to the pulsation of a heart. At each contraction, the direction of flow reverses. The mechanism of protoplasmic movement in slime-molds is, then, one of rhythmical contraction and relaxation of the plasmodium as a whole, with a period of 40 or 45 seconds for each pulsation.

If the area covered by a plasmodium, or a branch of it, is noted with the aid of a micrometer, it will be seen that the plasmodium contracts with outgoing protoplasm and expands with incoming protoplasm. I had interpreted this swelling and shrinking as the result instead of the cause of streaming. Contraction is not due to the exit of fluid, and expansion to its entrance, but on the contrary, as in the case of the heart, the exit and entrance of fluid are due to the contraction and expansion of the living substance.

The beat of the heart is controlled by a sympathetic nervous system. There is no nervous system in the sense of nerve-centers and nerve-fibers in slime-molds, but these are not necessary in order to have response and control.

Some modern workers are inclined to strip protoplasm of all the attributes of higher organisms, for-

getting that many of these attributes exist because they are properties of protoplasm. The tendency to regard the protoplasm of more primitive forms of life as less intricate, less responsive, if not "less living" than that in more highly developed forms is evident in such statements as, "there is considerable objection to the use of the word 'injure' in reference to plants." I recall the delightfully courteous remark of the English chemist, H. R. Proctor, who, finding my mechanistic interpretations of protoplasmic behavior rather harsh, asked if he might not still be allowed to regard life as, so to speak, a new departure.

In my turn, I ask the reader merely to admit that protoplasm is alive, for in so doing he tacitly grants that it exhibits irritability, in other words, nervous response.

It is interesting to contemplate the possible relationship between the rhythmical pulsations responsible for protoplasmic streaming in myxomycetes and the rhythmical contraction of sympathetically controlled muscle-tissue.

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IN-VITRO SYNTHESIS OF LACTOSE

RECENT work by Graham and Turner, working with goats, and the undersigned, working with dairy cattle, have shown that the active mammary gland removes lactic acid from the blood. The inactive mammary gland does not remove lactic acid. The quantity of lactic acid removed from the blood is such that lactic acid was suspected of being concerned with the synthesis of lactose in the milk. Galactose, which with glucose forms the lactose molecule, can theoretically be accounted for by the condensation of two molecules of lactic acid.

Proof of the correctness of the hypothesis that lactose is synthesized from lactic acid and glucose would lie in in-vitro synthesis of lactose from lactic acid and glucose. Solutions of glucose with lactic acid and various salts of lactic acid were prepared, to which was added macerated mammary gland tissue from lactating cows. This was incubated under toluene at 37° C. The mammary gland tissue was squeezed in muslin bags before and after grinding to express, in so far as possible, the milk retained in the ducts and aveoli. Blanks containing only mammary gland tissue and water incubated simultaneously with the experimental lots showed but the faintest traces of lactose.

Positive proof of the synthesis of lactose was established by the formation of lactosazones and by the isolation of 847 milligrams of material shown to be lactose.

Heating concentrated solutions of lactic acid or salts of lactic acid with glucose also produced lactose as indicated by osazones.

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THE USE OF MUCUS BY MARINE PLANKTON FEEDERS

WITH the exception of the crustacea, and perhaps the protozoa and sponges, apparently *all* the marine plankton feeders make use of mucus to entangle the microscopic materials upon which they live. This fact seems to have been overlooked by zoologists in general. After many years of study and reference work I have found that the rôle of mucus as an essential part of the feeding mechanism of marine animals has been greatly underestimated.

In 1928 I described the feeding habits of the gephyrean, *Urechis caupo,* in joint papers with Dr. W. K. Fisher,[1, 2] in which he gave the classification and description of the worm. At that time this method of feeding was considered unique by those biologists who became acquainted with the paper; but I have since found that this method is not unique, for other animals use a similar method of entangling their food, for example, *Chaetopterus variopedatus* and the tunicate, *Diplosoma macdonaldi.* In the case of many other animals in which the cilia have been credited with the selective function of obtaining food, I have found that the mucus forms a plate through which water is strained, and actually the cilia furnish only the mechanical power for creating the currents. One reason why mucus has not heretofore been accredited with its important rôle is that it is perfectly transparent, unless heavily laden with food; and another reason is that investigators have used such materials as carmine, India ink, etc., which, in most cases, cause a cessation of the secretion of mucus. Hence, what the investigators have done is to make plots of the ciliary currents, which often were reversed from what they actually are during feeding operations.

I have found that the method of feeding in *Chaetopterus* is by the secretion of a slime bag or funnel through which all water entering the burrow during feeding passes. As the bag is being secreted at the top by the aliform notopodia, it is rolled into a ball at the bottom by the accessory feeding organ; but at intervals secretion of slime ceases and this food ball is passed forward to the mouth, after which a new slime bag is formed. Therefore, the actual operation of food getting by *Chaetopterus* is quite different from that described by Enders,[3] whose paper is by far the

[1] W. K. Fisher and G. E. MacGinitie, *Ann. Mag. Nat. Hist.*, ser. 10, Vol. 1, pp. 204–213, 1928.
[2] *Ibid.*, Vol. 1, pp. 199–204, 1928.
[3] H. E. Enders, *Jour. Morph.*, 20: 3, 479–532, 1909.