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SURFACE TENSION IN RELATION TO CELLULAR PROCESSES[1]

THE record of investigation of the phenomena of the life of animal and vegetable cells for the last eighty years constitutes a body of knowledge which is of imposing magnitude and of surpassing interest to all who are concerned in the studies that bear on the organic world. The results won during that period will always constitute, as they do now, a worthy memorial of the intense enthusiasm of the scientific spirit which has been a distinguishing feature of the last six decades of the nineteenth century. We are to-day, in consequence of that activity, at a point of view the attainment of which could not have been predicted half a century ago.

This body of knowledge, this lore which we call cytology, is still with all this achievement in one respect an undeveloped science. It is chiefly—nay, almost wholly—concerned with the structural or morphological side of the cell, while of the functional phenomena our knowledge is only of the most general kind, and the reason is not far to seek. What little we know of the physiological side of the cell—as, for example, of cellular secretion, absorption and nutrition—has only to a very limited extent been the outcome of observations directed to that end. It is in very great part the result of all the inferences and generalizations drawn from the data of morphological research. This knowledge is not the less valuable or the less certain because it has been so won, but simply

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[1] Address to the Physiological Section of the British Association for the Advancement of Science, Sheffield, 1910.

because of its source and of the method by which we have gained it, it is of a fragmentary character, and therefore less satisfactory in our estimation.

This state of our knowledge has affected—or, to express it more explicitly, has fashioned—our concept of living matter. When we think of the cell it is idealized as a morphological element only. The functional aspect is not ignored, but we know very little about it, and we veil our ignorance by classing its manifestations as vital phenomena.

It is true that in the last twenty years, and more particularly in the last ten, we have gathered something from biochemical research. We know much concerning ferment or catalytic action, of the physical characters of colloids, of the constitution of proteins, and their synthesis in the laboratory promises to be an achievement of the near future. We are also in a position to understand a little more clearly what happens in proteins when, on decomposition in the cell, they yield the waste products, urea, and other metabolites, with carbon dioxide and water. Further, fats can be formed in the laboratory from glycerine and fatty acids, a large number of which have also been synthesized, and a very large majority of the sugars of the aldohexose type have been built up from simpler compounds. These facts indicate that some of the results of the activity of animal and vegetable cells may be paralleled in the laboratory, but that is as far as the resemblance extends. The methods of the laboratory are not as yet those of nature. In the formation of carbohydrates, for example, the chlorophyll-holding cell makes use of processes of the most speedy and effective character, but nothing of these is known to us except that they are quite unlike the processes the laboratory employs in the artificial synthesis of carbohydrates. Nature works unerringly, unfalteringly, with an amazing economy of material and energy, while "our laboratory syntheses are but roundabout ways to the waste sink."

In consequence, it is customary to regard living matter as unique—*sui generis,* as it were, without an analogue or parallel in the inorganic world—and the secrets involved in its actions and activities as insoluble enigmas. Impelled by this view there are those, also, who postulate as an explanation for all these manifestations the intervention in so-called living matter of a force otherwise and elsewhere unknown, biotic or vital, whose action is directed, according to the character of the structure through which it operates, to the production of the phenomena in question. Living protoplasm is, in this view, but a mask and a medium for action of the unknown force.

This is an old doctrine, but it has again made headway in recent years owing to the reaction from the enthusiasm which came from the belief that the application of the known laws of physics and chemistry in the study of living matter would explain all its mysteries. A quarter of a century ago hopes were high that the solution of these problems would soon be found in a more profound comprehension of the laws of the physical world. Since then there has been an extraordinary increase in our knowledge of the structure and of the products of the activity of living matter without a corresponding increase in knowledge of the processes involved. The obscurity still involving the latter appears all the greater because of the high lights thrown on the former. Despair, in consequence, has taken the place of hope with some, and the action of a mysterious force is invoked to explain a mystery.

It may be admitted that our methods of

investigation are very inadequate, and that our knowledge of the laws of matter, seemingly comprehensive, is not at present profound enough to enable us to solve all the problems involved in the vital phenomena. The greatest factor in the difficulty of their solution, however, has been the fact that there has been a great lack of investigators specially trained not only in biology, but also in physics and chemistry, for the very purpose of attacking intelligently such problems. The biologists, for want of such a wide training, have emphasized the morphological aspect and the readily observable phenomena of living matter; while the physicist and chemist, knowing little of the morphology of the cell and of its vital manifestations, have been unable to apply satisfactorily the principles of their sciences to an understanding of its processes. The high degree of specialism which certain departments of biology has in recent years developed has made that difficulty greater than it was.

It must also be said that in some instances in which the physicist and chemist attempted to aid in the solution of biological problems the result on the whole has not been quite satisfactory. In, for example, the phenomena of osmosis, the application of Arrhenius's theory of ionization and van't Hoff's gas theory of solutions promised at first to explain all the processes and the results of diffusion through animal membranes. These theories were supported by such an array of facts from the side of physics and physical chemistry that there appeared to be no question whatever regarding their universal validity, and their application in the study of biological phenomena was urged with acclaim by physical chemists and eagerly welcomed by physiologists. The result in all cases was not what was expected. Diffusion of solutes, according to the theories, should, if the membrane is permeable to them, always be from the fluid where their concentration is high to that in which it is low. This appears to happen in a number of instances in the case of living membranes—or, at least, we may assume that it occurs—but in one signal instance at least the very reverse normally obtains. In the kidney, membranes formed of cells constituting the lining of the glomeruli and the renal tubules separate the urine, as it is being formed, from the blood plasma and the lymph circulating through the kidney. Though the excreted fluid is derived from the plasma and lymph, it is usually of much greater osmotic concentration than the latter.

It may be urged that this and other discrepancies are explained by the distribution (or partition) coefficient of the solutes responsible for the greater concentration of the product of excretion, these solutes being more soluble in the excreted medium than in the blood plasma and distributing or diffusing themselves accordingly. If such a principle is applicable here as an explanation, it may be quite as much so in other physiological cases in which the results are supposedly due only to the forces postulated in the theories of van't Hoff and Arrhenius. Whether this be so or not, the central fact remains that the enthusiastic hopes with which the theories were applied by physiologists and biologists in the explanation of certain vital phenomena have not been wholly realized.

The result has been a reaction amongst physiologists and biologists which has not been the least contributory of all the causes that have led to the present revival of vitalism.

Another difficulty in accounting for the vital phenomena has been due, until recently, to a lack of knowledge of the physical and chemical properties of colloids and colloidal "solutions." The importance of this knowledge consists in the fact that protoplasm, "the physical basis" of life, consists mainly of colloids and water. Till eleven years ago what was known regarding colloids was derived chiefly from the researches of Graham (1851–62), Ljubavin (1889), Barus and Schneider (1891), and Linder and Picton (1892–97), who were the pioneers in this line. In 1899 were published the observations of Hardy, through whose investigations very great progress in our knowledge of colloids was made. In 1903 came the invention of the ultramicroscope by Siedentopf and Zsigmondy, by which the suspension character of colloid material in its so-called "solutions" was visually demonstrated. During the last seven years a host of workers have by their investigations greatly extended our knowledge of the physical and chemical properties of colloids, and now the science of collochemistry bids fair, the more it develops, to play a very important part in all studies bearing on the constitution and properties of living matter.

Then, also, there are the phenomena of surface tension. This force, the nature of which was first indicated by Segner in 1751, and described with more detail by Young in 1804 and La Place in 1806 in the expositions of their theories of capillarity, was first in 1869 only casually suggested as a factor in vital processes by Engelmann. Since the latter date and until 1892, when Bütschli published his observations on protoplasmic movements, no serious effort was made to utilize the principle of this force in the explanation of vital phenomena. Even to-day, when we know more of the laws of surface tension, it is only introduced as an incidental factor in speculations regarding the origin of protoplasmic movement and muscular contraction, and yet it is, as I shall maintain later on in this address, the most powerful, the most important of all the forces concerned in the life of animal and vegetable cells.

It may be gathered from all that I have advanced here that the chief defect in biological research has been, and is, the failure to apply thoroughly the laws of the physical world in the explanation of vital phenomena. Because of this too much emphasis is placed on the division that is made between the biological and the physical sciences. This division is very largely an artificial one, and it will in all probability be maintained eventually only as a convenience in the classification of the sciences. The biologist and physiologist have to deal with problems in which a wide range of knowledge is necessary for their adequate treatment; and, if the individual investigator has not a very extensive training in the physical sciences, it is impossible for him to have at his command all the facts bearing on the subject of his research, unless the problem involved be a very narrow one. The lack of this wide knowledge of the physical sciences tends to specialism, and, as the specialism is ever growing, it will produce a serious situation eventually, for it will develop a condition in the scientific world in which coordination of effort and a broad outlook will be much more difficult than is the case now.

This growing defect in the biological sciences can only be lessened by the insistence of those in charge of advanced courses in biological and physiological laboratories that only they whose training is of a very wide character should be allowed to take up research. It is, perhaps, futile to expect that such a rule will ever be enforced, for in the keen competition between universities for young teachers who have made some reputation for original investigation there may not be too close a scrutiny of the qualifications of those who offer themselves for post-graduate courses. There is, further, the difficulty that the heads of scientific departments are not desirous of limiting the output of new knowledge from their laboratories by insisting on the wider training for the men of science who are in the process of developing as students of research.

It is perhaps true, also, that there still remains a great deal unobserved or unrecorded in the fields of biology, physiology and biochemistry, in the investigation of all of which a broad training is not specially required to give good service; and that, further, this condition will obtain for one or two decades still. It is quite as certain, however, that the returns from such service will tend to diminish in number and value, and, if the coming generation of workers is not recruited from a systematically and broadly trained class of students, a period of comparative sterility may supervene.

As it is to-day, there are few who devote themselves to the direct study of the chemical and physical properties of the cell, the fundamental unit of living matter. There are, of course, many who are concerned with the morphology of the cell, and who employ in their studies the methods of hardening and staining which have been of very great service in revealing the structural as well as the superficial chemical properties of the cell. On the facts so gained views are based which deal with the chemistry of the cell, and which are more or less widely accepted, but the results and generalizations drawn from them give us but little insight into the chemical constitution of the cell. We recognize in the morphologists' chromatin a substance which has only in a most general way an individuality, while the inclusions in the nucleus and the cytoplasm, on whose distinction by staining great emphasis is laid, can only in a most superficial way be classified chemically.

The results of digestion experiments on the cell structures are also open to objection. The action of pepsin and hydrochloric acid must depend very largely on the accessibility of the material whose character is to be determined. If there are membranes protecting cellular elements, pepsin, which is a colloid, if it diffuses at all, must in some cases at least penetrate them with difficulty. In *Spirogyra,* for example, the external membrane formed of a thick layer of cellulose is impermeable to pepsin, but not to the acid; and, in consequence, the changes which occur in it during peptic digestion are due to the acid alone. Even in the cell whose periphery is not protected by a membrane, the insoluble colloid material at the surface serves as a barrier to the free entrance of the pepsin. It is, however, more particularly in the action on the nucleus and its contents that peptic digestion fails to give results which can be regarded as free from objection. Here is a membrane which during life serves to keep out of the nucleus not only all inorganic salts but also all organic compounds, except chiefly those of the class of nucleo-proteins. That such a membrane may, when the organism is dead, be permeable to pepsin is at least open to question, and in consequence what we see in the nucleus after the cell has been acted on by pepsin and hydrochloric acid can not be adduced as evidence of its chemical or even of its morphological character.

The results of digestive experiments on cells are, therefore, misleading. What may from them appear as nucleo-protein may be anything but that, while, if the pepsin penetrates as readily as the acid, there should be left not nucleo-protein, but pure nucleic acid, which should not stain at all.

The objections which I now urge against the conclusions drawn from the results of digestion experiments have developed out of my own observations on yeast cells, diatoms, *Spirogyra,* and especially the blue-green algæ. The latter are, as is *Spirogyra,* encased in a membrane which is an effective barrier to all colloids. When, therefore, threads of *Oscillaria* are subjected to the action of artificial gastric juice, a certain diminution in volume is observed owing to the dissolving power of the hydrochloric acid, and an alteration of the staining power of certain structures is found to obtain, but the pepsin has nothing to do with these, as may be determined by examination of control preparations treated with a solution of hydrochloric acid alone.

It is thus seen how slender is our knowledge of the chemistry of cells derived from staining methods and from digestion experiments. That, however, has not been the worst result of our confidence in our methods. It has led cytologists to rely on these methods alone, to leave undeveloped others which might have thrown great light on the chemical constitution of the cell, and which might have enabled us to understand a little more clearly the causation of some of the vital phenomena.

It was the futility of some of the old methods that led me, twenty years ago, to attack the chemistry of the cell from what appeared to me a correctly chemical standpoint. It seemed to me then, and it appears as true now, that a diligent search for decisive chemical reactions would yield results of the very greatest importance. In the interval I have been able to accomplish only a small fraction of what I hoped to do, but I think the results have justified the view that, if there had been many investigators in this line instead of only a few, the science of cytochemistry would play a larger part in the solution of the problems of cell physiology than it now does.

The methods and the results are, as I have said, meager, but they show distinctly indeed that the inorganic salts are not diffused uniformly throughout the cell; that in vegetable cells they are rigidly localized, while in animal cells, except those devoted to absorption and excretion, they are confined to specified areas in the cell. Their localization, except in the case of inorganic salts of iron, is not due to the formation of precipitates, but rather to a condition which is the result of the action of surface tension. This seems to me to be the only explanation for the remarkable distribution, for example, of potash salts in vegetable cells. We know that, except in the chloroplatinate of potassium and in the hexanitrite of potassium, sodium and cobalt, potassium salts form no precipitates; and yet, in the cytoplasm of vegetable cells, the potassium is so localized at a few points as to appear at first as if it were in the form of a precipitate. In normal active cells of *Spirogyra* it is massed along the edge of the chromotophor, while in the mesophyllic cells of leaves it is condensed in masses of the cytoplasm, which are by no means conspicuous in ordinary preparations of these cells.

This effect of surface tension in localizing the distribution of inorganic salts at points in the cytoplasm would explain the distribution of potassium in motor structures. In striated muscle the element is abundant in amount, and is confined to the dim bands in the normal conditions. In *Vorticella,* apart from a minute quantity present at a point in the cytoplasm, it is found in very noticeable amounts in the contractile stalk; while in the holotrichate infusoria (*Paramœcium*) it is in very intimate association with the basal elements of the cilia in the ectosarc. This, indeed, would seem to indicate that the distribution of the potassium is closely associated with contraction, and, therefore, with the production of energy in contractile tissues. The condensation of potassium at a point may, of course, be a result of a combination with portions of the cytoplasm, but we have no knowledge of the occurrence of such compounds; and, further, the presence of such does not explain anything, or account for the liberation of energy in motor contraction. On the other hand, the action of surface tension would explain not only the localization of the potassium but also the liberation of the energy.

In vessels holding fluids the latter, in relation to surface tension, have two surfaces—one free, in contact with the air, and known as the air-water surface; the other, that in contact with the wall of the containing vessel (glass). In the latter the tension is lower than in the former. When an inorganic compound—a salt, for example—is dissolved in the fluid it increases the tension at the air-water surface, but its dilution is much greater here than in any other part of the fluid; while at the other surface its concentration is greatest. In the latter case the condition is of the nature of adsorption. The condensation on that portion of the surface where the tension is least is responsible for what we find when a solution of a colored salt, as, *e. g.*, potassium permanganate, is driven through a layer of dry sand. If the latter is of some considerable thickness the fluid as it passes out is colorless. The air-solution surface tension is higher than the tension of each of the solution-sand surfaces on which, therefore, the permanganate condenses or is adsorbed. The same phenomenon is observed when a long strip of filter paper is allowed to hang with its lower end in contact with a moderately dilute solution of a copper salt. The solution is imbibed by the filter paper, and it ascends a certain distance in a couple of minutes, when it may be found that the uppermost portion of the moist area is free from even a trace of copper salt.

If, on the other hand, an organic compound—as, for instance, one of the bile salts—instead of an organic compound is dissolved in the fluid, the surface tension of the air-water surface is reduced, and in consequence the bile salt is concentrated at that surface; while in the remainder of the fluid, and particularly in that portion of it in contact with the wall of the vessel, the concentration is reduced.

The distribution of a salt in such a fluid, whether it lowers surface tension or increases it, is due to the action of a law which may be expressed in words to the effect that the concentration in a system is so adjusted as to reduce the energy at any point to a minimum.

Our knowledge of this action of inorganic and organic substances on the surface tension in a fluid and of the differences in their concentrations throughout the latter was contained in the results of the observations on gas mixtures by J. Willard Gibbs, published in 1878. The principle as applied to solutions was independently discovered by J. J. Thomson in 1887. It is known as the Gibbs's principle, although the current enunciations of it contain the more extended observations of Thomson. As formulated usually it is more briefly given, and its essential points may be rendered in the statement *that when a substance on solution in a fluid lowers the surface tension of the latter the concentration of the solute is greater in the surface layer than elsewhere in the solution; but when the substance dissolved raises the surface tension of the fluid, the concentration of the solute is least in the surface layers of the solution.*

It is thus seen how in a system like that of a drop of water with different contact surfaces the surface tension is affected and how this alters the distribution of solutes. It is further to be noted that for most organic solutes the action in this respect is the very reverse of that of inorganic salts. Consequently, in a living cell which contains both inorganic and organic solutes, and in which there are portions of different composition and density, the equilibrium may be subject to disturbance constantly through an alteration of the surface tension at any point. Such a disturbance may be found in a drop of an emulsion of olive oil and potassium carbonate in the well-known experiments of Bütschli. When the emulsion is appropriately prepared, a minute drop of it, after it is surrounded with water, will creep under the cover glass in an amœboid fashion for hours, and the movement will be more marked and rapid when the temperature is raised to 40 to 50° C. All the phenomena manifested are due to a lowering of the surface tension at a point on the surface, as a result of which there is protrusion there of the contents of the drop, accompanied, Bütschli holds, by steaming cyclic currents in the remainder of the mass.

Surface tension also, according to J. Traube, is all-important in osmosis, and he holds that it is the solution pressure (*Haftdruck*) of a substance which determines the velocity of the osmotic movement and the direction and force of the osmotic pressure. The solution pressure of a substance is measured by the effect that substance exercises when dissolved on the surface tension of its solution, or, to put it in Traube's own way, the more a substance lowers or raises the surface tension of a solvent (water) the less or greater is the solution pressure (*Haftdruck*) of that substance. This solution pressure, Traube further holds, is the only force controlling osmosis through a membrane, and he rejects completely the bombardment effect on the septum postulated in the van't Hoff theory of osmosis.

The question as to the nature of the factors concerned in osmosis must remain undecided until the facts have been more fully studied from the physiological standpoint, but enough is now known to indicate that surface tension plays at least a part in it, and the omission of all consideration of it as a factor is not by any means a negligible defect in the van't Hoff theory of osmosis.

The occurrence of variations in surface tension in the individual cells of an organ or tissue is difficult to demonstrate directly. We have no methods for that purpose, and, in consequence, one must depend on indirect ways to reveal whether such variations exist. The most effective of these is to determine the distribution of organic solutes and of inorganic salts in the cell. The demonstration of the former is at present difficult or even in some cases impossible. The occurrence of soaps which are amongst the most effective agents in lowering surface tension may be revealed without difficulty microchemically, as may also neutral fats, but we have as yet no delicate microchemical tests for sugars, urea, and other nitrogeneous metabolites, and in consequence the part they play, if any, in altering the surface tension in different kinds of cells, is unknown. Further research may, however, result in discovering methods of revealing their occurrence microchemically in the cell. We are in a like difficulty with regard to sodium, whose distribution we can determine microchemically in its chief compounds, the chloride and phosphate, only after the exclusion of potassium, calcium and magnesium. We have, on the other hand, very sensitive reactions for potassium, iron, calcium, haloid chlorine and phosphoric acid, and with methods based on these reactions it is possible to localize the majority of the inorganic elements which occur in the living cell.

By the use of these methods we can indirectly determine the occurrence of differences in surface tension in a cell. This determination is based on the deduction from the Gibbs-Thomson principle that, where in a cell an inorganic element or compound is concentrated, the surface tension at the point is lower than it is elsewhere in the cell. If, for example, it is concentrated on one wall of a cell the surface tension there is less than on the remaining surfaces or walls of the cell. The thickness of this layer must vary with the osmotic concentration in the cell, with the specific composition of the colloid material of the cytoplasm and with the activity of the cell, but it should not exceed a few hundredths of a millimeter (0.02–0.04 mm.), while it might be very much less in an animal cell whose greatest diameter does not exceed 20 μ.

Numerous examples of such localization may be observed in the confervoid protophyta. In *Ulothrix,* ordinarily, there is usually a remarkable condensation of the potassium at the ends of the cell on each transverse wall. The surface tension, on the basis of the deduction from the Gibbs-Thomson principle, should be, in all these cases, high on the lateral walls and low on those surfaces adjoining the transverse septa.

The use of this deduction may be extended. There are in cells various inclusions whose composition gives them a different surface tension from that prevailing in the external limiting area of the cell. Further, the limiting portion of the cytoplasm in contact with these inclusions must have surface tension also. When, therefore, we find by microchemical means that a condensation of an inorganic element or compound obtains immediately within or without an inclusion, we may conclude that there, as compared with the external surface of the cell, the surface tension is low. It may be urged that the condensation is due to adsorption only, but this objection can not hold, for in the Gibbs-Thomson phenomena the localization of the solute at a part of the surface as the result of high tension elsewhere of the solution is, in all probability, due to adsorption, and is indeed so regarded.[2]

It is in this way that we can explain the remarkable localization of potassium in the cytoplasm at the margins of the chromatophor in *Spirogyra* and also the extraordinary quantities of potassium held in or on the inclusions in the mesophyllic cells of leaves. In infusoria (*Vorticella, Paramœcium*) the potassium present apart from that in the stalk or ectosarc is confined to one or more small granules or masses in the cytoplasm.

How important a factor this is in clearing the active portion of the cytoplasm of compounds which might hamper its action, a little consideration will show. In plants very large quantities of salts are carried to leaves by the sap from the roots, and among these salts those of potassium are the most abundant as a rule. Reaching the leaves these salts do not return, and in consequence during the functional life of the leaves they accumulate in the mesophyllic cells in very large quantities, which, if they were not localized as described in the cell, would affect the whole cytoplasm and alter its action.

Enough has been advanced here to indicate that surface tension is not a minor feature in cell life. I would go even

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[2] See Freundlich, "Kapillarchemie," p. 50, 1909.

farther than this and venture to say that the energy evolved in muscular contraction, that also involved in secretion and excretion, the force concerned in the phenomena of nuclear and cell division, and that force also engaged by the nerve cell in the production of a nerve impulse are but manifestations of surface tension. On this view the living cell is but a machine, an engine, for transforming potential into kinetic and other forms of energy, through or by changes in its surface energy.

To present an ample defence of all the parts of the thesis just advanced is more than I propose to do in this address. That would take more time than is customarily allowed on such an occasion, and I have, in consequence, decided to confine my observations to outlines of the points as specified.

It is not a new view that surface tension is the source of the muscular contraction. As already stated, the first to apply the explanation of this force as a factor in cellular movement was Engelmann in 1869, who advanced the view that those changes in shape in cells which are classed as contractile are all due to that force which is concerned in the rounding of a drop of fluid. The same view was expressed by Rindfleisch in 1880, and by Berthold in 1886, who explained the protoplasmic streaming in cells as arising in local changes of surface tension between the fluid plasma and the cell sap, but he held that the movement and streaming of *Amœbæ* and *Plasmodiæ* are not to be referred to the same causes as operate in the protoplasmic streaming in plant cells. Quincke in 1888 applied the principle of surface tension in explaining all protoplasmic movement. In his view the force operates, as in the distribution of a drop of oil on water, in spreading protoplasm, which contains oils and soaps, over surfaces in which the tension is greater, and as soap is constantly being formed, the layer containing it, having a low tension on the surface in contact with water, will as constantly keep moving, and as a result pull the protoplasm with it. The movement of the latter thus generated will be continuous and constitute protoplasmic streaming. In a similar way Bütschli explains the movement of a drop of soap emulsion, the layer of soap at a point on the surface of the spherule dissolving in the water and causing there a low tension and a streaming of the water from that point over the surface of the drop. This produces a corresponding movement in the drop at its periphery and a return central or axial stream directed to the point on the surface where the solution of the soap occurred and where now a protrusion of the mass takes place resembling a pseudopodium. In this manner, Bütschli holds, the contractile movements of *Amœbæ* are brought about. In these the chylema or fluid of the foam-like structure in the protoplasm is alkaline, it contains fatty acids and, in consequence, soaps are present which, through rupture of the superficial vesicles of the foam-like structure at a point, are discharged on the free surface and produce there the diminution of surface tension that calls forth currents, internal and external, like those which occur in the case of the drop of oil emulsion.

A. B. Macallum

(*To be continued*)

METEOROLOGY AT THE SHEFFIELD MEETING OF THE BRITISH ASSOCIATION

The work on meteorology for the British Association for the Advancement of Science is organized under Section A—Mathematical and Physical Science—and under the subsection (*b*) Cosmical Physics and Astronomy. There can be no more pronounced recognition of the opinion that meteorology has already made good its claim to be considered as a subordinate branch of solar and cosmical physics,