

as a punishment in Central Asia, the fact is that his decoration with some 400 figures all over his body except the soles of his feet, was evidently done by Burmese tattooers, and is a masterpiece of their unpleasant craft. There is an account of him by Mr. Franks in the *Journal of the Anthropological Institute* for 1872.

E. B. TYLOR

PROF. STRASBURGER'S RECENT
RESEARCHES

Ueber den Bau und das Wachsthum der Zellhäute. Von Dr. Ed. Strasburger, Professor an der Universität Bonn. (Jena, 1882.)

THE work before us is another evidence of Prof. Strasburger's untiring industry and minute research. Interesting as all his books have been, this one may be said to surpass its predecessors in this respect, inasmuch as the questions with which it deals are of such fundamental importance in botanical science. The main object of the researches here published is to throw light upon two difficult and much-discussed points, namely, the intimate structure of organised bodies, and the mode of growth of cell-walls and starch-grains. With regard to the researches themselves it need only be said that they appear to have been carried out with Prof. Strasburger's accustomed thoroughness and accuracy, and that they are abundantly illustrated by beautiful drawings. The conclusions deduced from them are so remarkable that a brief *résumé* will not be out of place.

With regard to the intimate structure of organised bodies, Prof. Strasburger entirely dissents from that view which is known to all botanists as Naegeli's micellar hypothesis.¹ This hypothesis was based upon the phenomena of "swelling-up" which are so characteristic of organised bodies, and upon the optical properties which certain of these bodies possess. Prof. Strasburger points out that swelling-up may be as well ascribed to the taking-up of water between the molecules of the body as to its being taken up between Naegeli's micellæ. He shows also in a striking manner that the double refraction of organised bodies, such as cell-walls and starch-grains, depends upon their organisation as a whole, for when once their organisation is destroyed their double refraction is lost, a result which cannot be explained on the micellar theory since the particles of the disintegrated micellæ would, like particles of broken crystals, still retain their double refraction. According to Prof. Strasburger the molecules of an organised body are not aggregated into micellæ which are held together by attraction, but they are linked together, probably by means of multi-valent atoms, by chemical affinity in a reticulate manner. Swelling-up is then the expression of the taking-up of water into the meshes of the molecular reticulum, where it is retained by intermolecular capillarity. The more extensible the reticulum, that is, the more mobile the groups of molecules within their position of equilibrium, the greater the amount of swelling-up. The limit is reached when the chemical affinity of the molecules and the force of the intermolecular capillarity are equal; if the latter exceed the former at any moment the result is the destruction of the molecular reticulum, or, in other words, of the organisation. Protoplasm differs from

other organised bodies in that the grouping of its molecules is undergoing perpetual change, the result of this molecular activity being the phenomena which we term vital.

The growth in thickness of cell-walls and of starch-grains takes place, according to Prof. Strasburger, by the deposition of successive layers. Here again he is at issue with Naegeli, who believed that the mode of growth was intussusceptive with subsequent differentiation of layers. It is impossible to go into detail with regard to the observations from which his conclusion has been formed; it need only be said that they are very numerous and elaborate, and that they confirm those of Dippel and of Schimper. Prof. Strasburger goes indeed so far as to say that even the surface-growth of cell-walls is not intussusceptive, but is merely due to stretching. It must be admitted that, assuming that all cellulose is derived from proteid, it is difficult to understand how proteid particles can be intercalated into the cell-wall to become subsequently converted into cellulose, but it is equally difficult to imagine that the wall of large cells, such for instance as an internodal cell of *Nitella* or a laticiferous cell of *Euphorbia*, is simply the much-stretched wall of the small cell from which these originated. Surely the amount of solid substance in the wall of such cells as these increases with its increased surface! Here further investigation is doubtless needed.

There is, however, one point of detail which is of such general interest that it deserves some consideration; it is with reference to the mode of formation of the cell-wall and of the thickening-layers. Schmitz some years ago expressed the opinion that the cell-wall is formed by the actual conversion of a layer of the protoplasm, that is, chemically speaking, by the production of a layer of cellulose from a layer of proteid. With this opinion Professor Strasburger entirely agrees, and he supports it by a number of remarkable observations. When a mass of protoplasm is about to clothe itself with a membrane, the peripheral layer becomes densely filled with minute proteid bodies, the microsomata, and this layer then becomes converted into cellulose. The wall of a young wood-cell of *Pinus*, for instance, is clothed internally with a layer of protoplasm filled with microsomata, which are arranged in spiral rows; the microsomata then gradually disappear, and the layer of protoplasm is found to be replaced by a layer of cellulose, which presents spiral striation corresponding to the previously existing rows of microsomata, and which constitutes a thickening layer of the cell-wall. In cells the walls of which become much thickened, the whole of the protoplasm may be gradually used up in this way. Again, the wall of pollen-grains and of spores is formed from a peripheral layer of the protoplasm which contains abundant microsomata. Its subsequent growth, and especially the development of the asperities which it commonly presents, is effected by the surrounding protoplasm which is derived from the disorganised tapetal cells; this is especially well shown in the development of the epispore (perinium) of *Equisetum* and of *Marsilia*. When an intine or endospore is present, it is produced like the outer coat, from a peripheral layer of the protoplasm of the pollen-grain or spore. Further, the septum which is formed in the division of a cell is

¹ See NATURE, vol. xxiii. p. 78.

produced in the same way. The cell-plate, like the peripheral layer of the protoplasm of a young pollen-grain, contains micropomata which disappear, and it is then converted into a plate of cellulose. Finally, the successive layers of a starch-grain are produced by the alteration into starch of layers of proteid-substance derived from the starch-forming corpuscle (amyloplast).

Besides dealing thoroughly with these main points, Prof. Strasburger touches upon others which are also of great importance. He points out that the starch which makes its appearance in the chlorophyll-corpuscles under the influence of light, is derived from the proteid of the corpuscles by dissociation. The formation of this starch is therefore not the immediate product of the synthetic processes going on in the chlorophyll-corpuscles, but only a mediate product. The processes in question produce proteid. Prof. Strasburger is inclined to accept Erlenmeyer's hypothesis, that methyl aldehyd is formed in the chlorophyll-corpuscles from carbonic dioxide and water and to believe that by polymerisation a substance is produced which can combine with the nitrogenous residues of previous dissociations of proteid to reconstitute proteid. He does not agree with the suggestion of Loew and Bokorny that the methyl aldehyd may combine with ammonia and sulphur to form proteid *de novo*.

Lastly, Prof. Strasburger makes an interesting suggestion as to the probable physiological significance of the nucleus. He points out that the nucleus cannot be regarded as regulating cell-division, for instances are known of cell-division taking place without previous nuclear division, and, conversely, of nuclear division taking place without cell-division. He is of opinion that the nucleus plays an important part in the formation of proteid in the cell. This view is founded upon the facts that one or more nuclei have been found to be present in the vast majority of plant-cells, that the nucleus is, as a general rule, the most persistent protoplasmic structure, and that it gives the various proteid reactions in a very marked manner.

SYDNEY H. VINES

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Behaviour of Sulphate of Lead in a Secondary Battery

SINCE the meeting of the British Association at Southampton I have made several experiments on the action of sulphate of lead at the negative pole of a decomposition cell, with a view to ascertain, not whether the sulphate was reduced in bulk by the action of the nascent hydrogen, a matter concerning which I had satisfied myself before in the negative, but the less practically important matter whether any trace of metallic lead could be obtained upon the negative plate by this action.

I used, therefore, platinum electrodes, immersing them in a paste of sulphate of lead in dilute sulphuric acid. And at the suggestion of Prof. McLeod, in order to obtain sulphate pure and in a fine state of subdivision, I precipitated a quantity from dissolved carbonate.

The paste soon settled down, leaving about a quarter of an inch of clear liquid above it, which was decanted off. Small thick platinum plates stood in the paste about 2 inches apart,

and were connected with either three or two Leclanché cells. When three cells were used, the evolution of gas from both plates speedily scooped out a hole round each filled with only turbid liquid, which was kept agitated by the bubbles.

Under these circumstances a distinct darkening of both plates occurred, and after a day or two they showed a distinct though extremely thin coating of peroxide and of metallic lead respectively. Prof. McLeod had tried the same kind of experiment, and noticed that the darkening occurred more readily on portions of the plate in contact only with free liquid than on those imbedded in paste.

I therefore re-embedded my plates, and employed only two cells to charge them, so that the bubbles might not have power enough to remove the paste from contact with the plates at all parts; under these circumstances the growth of peroxide of lead at the + plate was abundant, so much so that when the plate was ultimately pulled out, it left a black mass behind it, which had penetrated into the white paste; but the growth of the metallic lead on the - plate was even less perceptible than before, and it was evident that the metallic lead was better deposited from the solution than from the paste. It seemed probable, therefore, that though the sulphate is extremely insoluble in dilute acid, yet that a sufficient trace was dissolved to be acted on by the hydrogen, and that as fast as this was decomposed more was dissolved from the large quantity of solid present, provided the liquid was free to circulate and become replenished.

To test this further, I first made a saturated solution of sulphate of lead in the acid, by shaking and stirring it up with the finely divided precipitate for many hours—though ordinary dilute sulphuric acid is probably perfectly saturated without any such treatment—and then electrolysed the clear solution. No effect is ordinarily perceived under these circumstances, and I could perceive none. Hence the quantity dissolved at one time must be something infinitesimal; and it is able to give no appreciable deposit, unless fresh solid is present to replenish it.

Next I took a vessel full of the sulphate paste, but with a third of an inch clear liquid standing above it; and into this clear liquid I dipped the platinum plates, barely letting them touch the pasty mass below. In this position they remained several days connected to two Leclanchés, and the result was a distinct blackening of the - plate with a deposit of metallic lead from the solution; but the + plate scarcely seemed to receive any deposit of peroxide except along its bottom edge, which probably just touched the paste, and which showed a narrow line of deep puce colour. The observation that the - plate received its deposit more easily from the free solution than from the paste, had been previously made by Prof. McLeod. But to get the deposit most quickly, it is best to immerse the plates in the paste, and to cause sufficient gas to be evolved to keep them free from actual contact with it; while at the same time the solution surrounding them is so near a large surface of paste, that it can be very rapidly replenished.

On neutralising the acid with ammonia, so that ammoniacal salts and common salt might be present, in which sulphate of lead is known to be somewhat soluble, the deposit of metallic lead went on with far greater rapidity.

I have subsequently repeated the experiments with a paste of ordinary sulphate of lead, and the results appear to be quite the same. A week's deposit could be dissolved off the negative platinum plate with a single drop of nitric acid, and could only be made to show a faint precipitate when sulphuric acid was added to this solution in a watch-glass.

Moreover, unless the plate were rinsed on extracting it from the paste, the small amount of sulphuric acid clinging to it was sufficient to so whiten the deposit in the course of a night as to make it seem almost as if it had disappeared.

The matter is rather a small one to write so much about, but the behaviour of sulphate of lead in secondary batteries is really of considerable importance, and is at the bottom of a great many of the difficulties which one meets with in practical operations with secondary lead cells.

Moreover, it is only due to Dr. Gladstone that I should say how far I have been able to obtain his results; and he will perceive that if all he asserts is that platinum electrodes do show a nearly infinitesimal tarnish of metallic lead (as I understood him to say at Southampton), then my experience agrees with his. But I think that this is merely due to the partial solubility of the sulphate; and I never find that the reduction is able to spread through the paste in the slightest degree, in such a way