

The style is re-formed when animals are replaced under normal conditions, and the presence of food is not essential for this process as the work of recent investigators has shown. Mr. Berkeley considers this evidence in favour of his theory, since the style is regenerated by aeration alone. But I fail to see that it invalidates my theory, since in these circumstances the vital activities of the animals will be restored—even though they are starved, for lamelli-branches can undergo months of starvation and be still perfectly healthy at the end—and secretion of the style substance will be increased until it resumes its normal dimensions, and so the usual balance between the rate of production of the style and the rate of dissolution will be regained.

Again, and this is a point to which Mr. Berkeley does not refer, if his theory is correct there should be some correlation between the size of the style and the habitat of the animal. If an animal possessing a style is normally exposed to anaerobic conditions, one would expect that it would have a larger style than one which never suffered from lack of oxygen. There is, however, no such correlation, a fact to which I directed attention in my paper, while exactly the same point was raised by Prof. T. C. Nelson (*Biol. Bull.*, vol. 49, p. 86) in a review of recent work on the crystalline style published about the same time as my own paper, both of us being in complete ignorance of the work of the other. He compares the habitat of *Pisidium idahoense*, which lives in the mud at the bottom of Lake Mendota, Wisconsin, "where for two-thirds of the year the water may be completely devoid of oxygen," with that of *Macra*, which lives "in or close to the breaker line along sandy coasts, where the water at all seasons of the year is saturated with oxygen." Yet the style of *Pisidium* is no larger than that of related species living in well-aerated water, while the styles of all species of *Macra* are exceptionally firm and large.

Like Prof. Nelson, I find myself unable to accept Mr. Berkeley's views as to the cause of the disappearance of the style, nor do I agree with him that the matter should be considered *sub judice*; since I consider that my experiments demonstrate clearly that the dissolution of the style is a purely physical process and not in any way, in the words of Mr. Berkeley, "an expression of an effort on the part of the animal to offset the strain on its vital activity induced by anaerobiosis." C. M. YONGE.

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#### Ultramicroscopic Organisms of Filterable Viruses.

It appears to me that the fundamental significance (apart from its medical importance) of Mr. J. E. Barnard's<sup>1</sup> observations on the ultramicroscopic organisms known as filterable viruses has not been sufficiently recognised by biologists. These observations show the existence of a group of organisms as different from bacteria as bacteria are from yeasts or molds. The term cell cannot properly, in my opinion, be applied to them. Mr. Barnard's observations and photographs with ultra-violet light, particularly of the organism causing bovine pleuropneumonia, show minute hollow vesicles devoid of nucleus or of a cell wall in any proper sense. The 'wall' of the vesicle is evidently the living material of the organism, and it appears justifiable to apply to its substance the term protoplasm. But its methods

<sup>1</sup> "The Microscopical Examination of Filterable Viruses," *Lancet*, 1925, pp. 117-123.

of reproduction show that there can be little differentiation of the presumably protein materials composing this 'wall.'

These organisms appear to multiply in two ways: (1) by the appearance of minute papillæ on the surface of the vesicles. These are later attached to the vesicle only by threads, and they finally grow into new vesicles. This process bears some resemblance to the budding of yeast cells, but there are fundamental differences. (2) A vesicle may elongate into a flattened hollow structure which then breaks up into a row or group of particles like the papillæ, having the same property of growing into vesicles. This process might perhaps be compared with gonidia formation in such bacteria as *Crenothrix*. It was also found that the vesicle stage could be separated from the particle stage by filtration through a collodion membrane.

This group of ultramicroscopic organisms in its extreme simplicity and methods of reproduction brings us a distinct step nearer the inorganic. They are not strictly cells in the sense in which that term has hitherto been used by biologists. We appear to have here organisms reduced to their lowest terms, undifferentiated particles of protoplasmic material which can grow into hollow vesicles reproducing by budding and fragmentation. It becomes a question whether any organisms could be yet smaller and still show the essential phenomena of life. I suggest for these organisms the name *Protozia*, to contrast them with the plant and animal unicells which Haeckel grouped together as the Protista. It is quite possible that a world of organisms of this class exists, which are neither plants nor animals nor cells, and are as extensive and ubiquitous as the Protists. We have no present means of recognising their existence except in the case of those which are, like the virus diseases, parasitic in animals and plants.

That these organisms stand distinctly nearer the inorganic than bacteria is obvious. Investigations of the so-called d'Herelle phenomenon and of enzymes by similar methods, by showing the condition of aggregation of these bodies, will help to indicate just how wide is the gap which still separates such bodies from the strictly inorganic.

My apology for intruding into this field must be that it is one in which I have long been interested.

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#### Half-integral Vibrational Quantum Numbers in the Magnesium Hydride Bands.

THE new quantum mechanics predicts<sup>1</sup> that both the rotational and vibrational quantum numbers in band spectra should have half-integral values. Numerous examples of half-integral rotational quantum numbers have been reported in recent work, but as yet only one case where the evidence is in favour of half-integral vibrational quantum numbers has been recorded. Mulliken has shown<sup>2</sup> that the magnitude of the isotope effect in the BO bands is such as to argue for the adoption of half-vibrational quantum numbers. Indeed, accurate observations of isotope effects apparently constitute the only available method for getting evidence on this point.

P. Rudnick and the writer<sup>3</sup> have recently noted the presence of the approximately correct rotational isotope effect in the fundamental ( $\lambda_{5211}$ ) band of magnesium hydride. I have made some new calculations of our data with the object of deciding as to the

<sup>1</sup> W. Heisenberg, *Zeit. f. Physik*, 33, 879, 1925.

<sup>2</sup> R. S. Mulliken, *Phys. Review*, 25, 279, 1925.

<sup>3</sup> *Astro. Journal*, 63, 20, 1926.