Letters to the Editor.

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The Micelle—A Question of Notation.

THERE is a class of colloids, recognised as a class for very many years, in which the substance is a salt of an ordinary ionisable type the peculiarity of which consists simply in a prodigious disparity in size and solubility between the two parts of the salt molecule. Such are many proteins, some dyes, and soaps, to enumerate them in the order in which they have been investigated. What all colloids do surreptitiously, namely, take to themselves uncovenanted ions, these do in an honest straightforward chemical fashion.

The properties of the class *qua* colloids were, I believe, first worked out by myself in the years 1898–1905, the special case examined being certain proteins, called globulins, which present the added complexity that they combine not only with acids and alkalies to form salts, but also with neutral salts themselves. In spite of this, when the large number of variables was disentangled, the behaviour of the substances was found to be singularly orderly, the phase-rule diagram, for example, being strikingly like that of a commonplace three-component system bearing no suspicion of colloidality.

These colloid salts present one striking peculiarity, namely, that though in water they ionise and hydrolyse on the whole according to the approved pattern, yet one of the molecular species, and that the one which confers upon the solution its most characteristic qualities, is a complex composed for the most part of undissociated salt molecules but with a surface electric charge due to ionisation at the surface.[1] To these bodies I gave the name "colloidal ions." This is strictly accurate notation, for it is because of these complexes that the solutions possess the characteristic colloidal trick of slurring over the obligations of the chemical law of definite and multiple proportions, and they are ions as Faraday used the word, for they wander (ἰόν) in an electric field.

I pointed out that they conformed to Helmholtz's analysis of the condition of electric endosmose, the density of the charge on the surface being constant, and the total charge, therefore, proportional to the surface area. This, of course, obtains only when the solution has had time to forget its past history and to come into equilibrium; waiting for which state needs, in colloidal society, a vast gift of patience.

In the years which followed, much excellent work was done on another example of the group, namely, congo red, by Bayliss, who described aggregates of anions, the total charge being the sum of the charges of the constituents; and still later, a third example, namely, ordinary soap, was investigated by McBain, who rediscovered the colloidal ion but christened it "micelle."

Now accuracy of notation is the breath of the life of science, and to use the word "micelle" for a colloidal ion seems to me to be positively wrong, since the word was introduced by the botanist Nägeli in 1877 to describe something entirely different. Nägeli was a man of a curious imagination but he clothed his dreams in exact language. He is precise as to what he means by "micelle." The word was coined amid a controversy which raged in the 'seventies and 'eighties concerning a distinction then drawn between organised and unorganised colloids and the causes of "swelling."

Nägeli, who was an intellectual heir of the Frankenheim of 1851, began with molecules in 1858, but by 1877 he had identified the unit of the colloidal state as an aggregate of composite type to which he gave the name "micelle." This he supposed to consist of a nucleus of solute surrounded by an atmosphere of bound water. The water atmosphere was the essence of his conception, which had nothing to do with electric charges or with ionisation. A single "micelle," or a micellar chain, contained a micellar nucleus, or nuclei and micellar water. A gel was conceived as being composed of such chains with their micellar water, disposed as membranes or bars to form a sponge enclosing extra-micellar or "enclosed" water.

In complete opposition to Nägeli was Strasburger (1882), a molecularist in the direct line of descent from Kekule. Between stood Pfeffer, whose forebears were Dutrochet, 1827, Nägeli of 1858, Graham, 1864, and Traube, 1867; van Bemmelen had no kinship with Strasburger, he follows on the latest stage of Nägeli (1880) and Pfeffer.

I have just been looking through my thirty-year-old notes of that discussion. What a lot those people knew which is now forthcoming as new knowledge! They knew, or at any rate conjectured, that the colloidal particles were strung together in thread-like masses in some colloidal solutions, and it will do no harm to remind those who propound theories of gel structure that they knew such theories must account for hydrostatic pressures of upwards of 45 atmospheres.

The colloidal ion is far removed from Nägeli's "micelle"; it is nearer to the supposed colloidal unit which Pfeffer called a "tagma," and described as an overgrown aggregate of one species of molecule, namely, those of the solute.

Had recent workers known of these earlier hypotheses they would possibly have been content with the words "colloidal ion" for the constitution of soaps. That brings me to the gist of the matter: in the early 'nineties when, as a physiologist, I was attracted to colloids, I found two schools, both of whom had done excellent work, wholly unacquainted with each other's writing. Ringer, for example, on the biological side had demonstrated on the living heart the differential action of ions and "antagonism." He did not recognise the full significance of his observations because, like all contemporary biologists, he was wholly ignorant of the work of Schulze, and of Picton and Linder. The two schools presently came together to the advantage of both, but now the striking want of acquaintance by many chemists with colloidal work published in the biological journals is symptomatic of a renewed falling apart. How many physicists or chemists know of Mines's brilliant work on membrane potential?

It is impossible to avoid rediscoveries in science because of the enormous burden of knowledge, but it is in every one's interest to minimise them. Out of the mouth of a sinner comes, I hope, good advice. I must be the greatest of sinners myself, for it is certain that no one reads other people's science with greater reluctance than I do.

W. B. HARDY.

[1] A most interesting suggestion as to their structure is that of Adam, in the Proc. Roy. Soc., A, xcix. 336, 1921.

Problems of Hydrone and Water: The Origin of Electricity in Thunderstorms.

THE subject of the electricity of rain and its origin in thunderstorms was dealt with by Dr. G. C. Simpson in a communication to the Royal Society in 1909 (Phil. Trans., 1909, A, vol. 209, pp. 379-413). Taking