

Although I am at present not fully convinced as to the validity of the quantitative values calculated by Finkelstein, it nevertheless appears that some interesting explanations of the structure of electrolytic solutions are to be expected by comparison with experimental results.

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¹ Jones and Dole, *J. Amer. Chem. Soc.*, **51**, 2950; 1929.

² Falkenhagen and Dole, *Zeit. f. physik. Chem.*, **6**, 159; 1929; also *Phys. Zeit.*, **30**, 611; 1929.

³ See the article by me entitled "Modern Theories of Ionisation" in the *Chemiker Kalender*, vol. 3, pp. 346-362; 1931; or my forthcoming monograph on electrolytes.

⁴ Joy and Wolfenden, *NATURE*, Dec. 27, 1930, p. 994.

⁵ *Phys. Zeit.*, in press.

⁶ B. N. Finkelstein, *Phys. Zeit.*, **31**, 130, 165; 1930.

Electromotive Force of Dielectrics.

I FOUND some time ago¹ that glasses in aqueous solutions show the electromotive force of a solid electrolyte: the ion in the solid glass determines the potential difference against the solution. Moreover, it could be shown that cations from the solution were exchanged against the cations in the glass: the glass behaves then like a mixed electrode, but in a certain range of concentration practically like an electrode reversible to the ions taken up from the solution. Glasses which show definitely the behaviour of a solid electrolyte (sodium electrode) also show always exchange electrodes, particularly the silver and hydrogen electrode. Certain soft glasses show only the hydrogen electrode except in alkaline solution.

It was of interest to investigate whether other dielectrics would also show the same behaviour. With J. Hafner and lately with J. E. Ferguson, I investigated the electromotive behaviour of fused silica. In this case also we found the existence of the sodium, silver, and hydrogen electrode.

In the case of quartz, as in the case of glasses, the electromotive behaviour corresponds to the observations made when a current passes the solid.

J. E. Ferguson and I² have investigated during the last year the electromotive behaviour of thin paraffin films. We found also in this case the existence of sodium, potassium, hydrogen, and silver electrodes, and also (with some slight deviations, however) the existence of a calcium electrode, which we failed to find in any of the glasses investigated.

These phenomena can be understood³ if we make the following assumption: The number of places available for cations in the dielectric is limited and constant (= a). Only one kind of ion, cations, are taken up by the solid, and only these ions can migrate in the solid. In the state of equilibrium the difference of potential, solid-solution, is the same for all kinds of ions present. Neglecting the number of ions given off by the solid as compared with the concentration, c , in the solution, and treating the solid phase in first approximation like a dilute solution, the following formula is obtained for two kinds of ions present:

$$E = \frac{RT}{F} \ln \frac{K_1 u_2 a}{c_1 + \frac{K_1 u_2 c_2}{K_2 u_1}}$$

and ΔE the potential difference in the 'concentration cell' which alone is being measured:

$$\Delta E = \frac{RT}{F} \ln \frac{c_1' + A c_2}{c_1 + A c_2} \left(A = \frac{K_1 u_2}{K_2 u_1} \right).$$

In this formula c_i are the concentrations in the solution, u_i are the mobilities in the solid phase, K_i are the integration constants in the expression for the thermo-

dynamic potential of the ions present in the solid phase (solution tension).

In this formula all the terms can be measured directly, since also A is given by a single experiment. For permutites and certain glasses, more complicated formulæ have to be used, since the ions in the solid phase cannot be treated as independent of one another, and since the amount of ions exchanged is comparable with the concentration of the ions in the solution.

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¹ *Zeit. Physik.*, **15**; 1923.

² *Bull. Am. Phys. Soc.*, Nov. 1930.

³ This formula has been used by me since 1924 for the interpretation of different experimental results. See, for example, *Bull. Am. Chem. Soc.*, April 1927, and J. W. V. Osterhout, *Bull. Nat. Res. Council*, **69**, p. 193, footnote 52; 1929.

Protein Structure and Denaturation.

ASTBURY and Woods' fundamental work upon the micellar structure of the protein of wool fibres,¹ and the hypothesis they put forward as an explanation of the changes observed in the X-ray pattern when such fibres are stretched, would seem to be full of significance for protein chemistry in general.

Within the last ten years, different lines of evidence have been converging upon the view that some regularity, as regards pattern and molecular size, underlies the disordered confusion of data we possess relating to the proteins of the animal and vegetable kingdoms. The two most striking demonstrations in recent years of such uniformity are afforded by Svedberg's brilliant application of the ultracentrifuge to determine the particle mass of soluble proteins,² classes of 'molecular weight' 1, 2, 3, and 6 times the common factor 34,500 being distinguished, and Gorter and Grendel's demonstration³ that under appropriate conditions soluble proteins exhibit the phenomenon of surface spreading on liquids, and that all occupy the same surface area irrespective of particle mass (1, 2, 3, or 6 times 34,500). Using Svedberg's common factor 34,500 for the basis of their calculations, the Dutch workers obtain a value for the radius of the unit particle (22.5 Å.) identical with that determined by Svedberg experimentally.

The most significant feature of Gorter and Grendel's work, however, is that their results imply a loosening, brought about by the surface forces, of the cohesive attraction holding the units of the aggregated proteins together. Astbury and Woods' investigations reveal a somewhat similar, although internal, deformation of the keratin structure of the wool fibre, brought about by purely physical means. Our conceptions of the chemical reactivity of protein structures clearly need revision in an attitude of greater attention to modern valence conceptions.

One more point cannot be too clearly emphasised which is common to the essential findings of Svedberg, Astbury and Woods, and Gorter and Grendel: the changes observed by these workers are strictly reversible.

In conclusion, I should like to touch upon the problem of protein denaturation, and to inquire whether it is not in the direction of such work as that of Astbury and Woods that we have to look for a solution of this problem? Denaturation of proteins, which can be brought about by mechanical as well as by chemical forces, is characterised by a loss of solubility at the isoelectric point. It was always thought to be an irreversible change, but Anson and Mirsky⁴ have recently demonstrated its reversible nature in