(unsigned) chemical valency z, and if the solutions are so dilute that we may equate a ratio of mol fractions to a ratio of molal concentrations, we obtain $c_+'' = c_+' e^x$ and $c_-'' = c_-' e^{-x}$, where now $x = \frac{zEF}{RT}$, and the suffixes + and − indicate cations and anions respectively. Let us now suppose that the solution (′) contains the same concentration c' of both cations and anions of the electrolyte considered, but that this is not true for the solution(″) which contains the indiffusible ion or ions; then it follows that

$$c_+'' + c_-'' = c'(e^x + e^{-x}) = 2c' \cosh x.$$

Denoting now by $c_\pm$ arithmetical mean concentrations of the ions of the electrolyte considered, that is,

$$c_\pm'' = \tfrac{1}{2}(c_+'' + c_-'') \text{ and } c_\pm' = \tfrac{1}{2}(c_+' + c_-') = c'$$

we get finally the equation

$$c_\pm'' = c' \cosh x = c_\pm' \cosh x,$$

which is identical with Dr. Chaudhury's equation. It is clear, therefore, that his equation is a very obvious and elementary deduction from existing thermodynamic theory and presents no novelty. Moreover, his employment of the Maxwell–Boltzmann distribution theorem offers nothing new, for the thermodynamic equations given above, such as, for example, $N_i'' = N_i' e^x$, are just examples of this theorem as applied to the special case considered. Dr. Chaudhury assumes the solution (″), which contains the colloid or non-diffusible ion (ions), to consist of "a charged material and a double layer which extends up to the membrane through which the colloidal particles or non-diffusible ions cannot pass". Such an assumption is both unnecessary and unjustifiable; but is apparently made in order to align the membrane equilibrium cases with the author's treatment of cases where (in the absence of a membrane) ions are distributed in various ways.

There is not space available to go into the consideration and criticism of other sections of the book, but enough has been said to show that, at least in the case of ionic membrane equilibria, there exists no new 'Chaudhury theory'. Nor do the equations of ionic membrane equilibria form a 'special' case of this soi-disant theory.

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[1] *Proc. Camb. Phil. Soc.*, **22**, Pt. 4, 493 (1925).
[2] *Z. phys. Chemie*, **170**, A, 41 (1934).
[3] *J. Phys. Chem.*, **33**, 842 (1929).
[4] Donnan, F. G., and Guggenheim, E. A., *Z. phys. Chem.*, **162**, A, 346 (1932); Donnan, F. G., *Z. phys. Chem.*, **168**, A, 369 (1934).
[5] See equation 27 of the second paper mentioned in ref. 4.

UNORTHODOX VIEWS ON PROTEIN STRUCTURE

On the Structure of the Protein Molecule

A Chemical Investigation. By N. Troensegaard. Second edition. Pp. 126. (Copenhagen: Einar Munksgaard; London: Oxford University Press, 1944.) 14 Danish crowns.

UNORTHODOXY in science should always be welcomed and given a hearing: how else would the theory of phlogiston have been relegated to limbo? Unorthodoxy, however, must be sure that its own eye is clear of beams while it is decrying the motes in the eyes of the orthodox. Troensegaard's researches in protein chemistry have now been going on for about twenty-five years. He points out, quite correctly, that to boil a protein for many hours in a fierce reagent like 20 per cent hydrochloric acid may be to destroy the very structures which the chemist is seeking. His alternative line of attack appears, however, to be even more violent. To start with, his proteins are thoroughly dried, even to the extent of storing over phosphorus pentoxide. Proteins exist naturally in aqueous surroundings, and when they are isolated from these, some 'bound water' goes with them, and only under very limited experimental conditions can it be removed without turning the native protein into something else. Troensegaard finds that if dry gelatin is dissolved in anhydrous methanol-potassium hydroxide and neutralized with ethyl acetate, the total amount will be precipitated if more dry methanol is added. He argues, therefore, that the gelatin has formed a colloidal solution; hence there cannot have been any great splitting up of the molecule. True, but there can have been other changes, such as ring closure following the removal of bound water from active centres in the gelatin molecule.

Troensegaard claims that all proteins have approximately the same elementary composition ($C_{10}H_{16}N_{2\cdot7}O_{3\cdot2}$) and that the polypeptide theory cannot explain this. Proteins, however, are not all alike in their nitrogen/carbon ratio, a notable example being the protamines. The recent complete analysis of β-lactoglobulin in terms of amino-acid units is also strong evidence in favour of the polypeptide theory. However, unorthodox though it may appear to most protein chemists to handle proteins in a non-aqueous system, Troensegaard's experimental findings must be taken seriously by all students of protein structure. His method is straightforward. He dissolves a dry protein in a non-aqueous solvent (a certain amount of choice exists here, the selected solvent varying with the protein, but anhydrous methanol-potassium hydroxide dissolves most proteins, and after neutralization with ethyl acetate the alkali protein is soluble in glacial acetic acid), acetylation with acetic anhydride follows, the acetylated protein is dissolved in chloroform and precipitated in dry ether. Hydrogenation with sodium in dry amyl alcohol is the next stage, and a short cold hydrolysis, after which the addition of ether separates the products into ether-amyl alcohol-soluble bases (*B* and *C* bases), a water-soluble fraction (*D* bases), and ammonia. The *B* and *C* bases are pyrroles and piperidines. The only aliphatic amine identified is isoamyl amine, which forms only 1–2 per cent of the protein. α-Methyl pyrrolidine has been isolated from a few proteins. The *D* bases from several proteins have the same acid-binding capacity for hydrochloric acid and for chloroplatinic acid reckoned in terms of equivalents of acid bound per two atoms of nitrogen. In many cases the elementary composition of the *D* bases from different proteins is the same.

Troensegaard argues that pyrrole and piperidine rings must form sub-units in the protein molecule. Certainly the theory of protein structure must account for their occurrence as end-products after his experimental treatment. Are they pre-formed in the protein molecule, as he believes, or is the pattern of the molecule such that they are formed by the bonding of spatially adjacent groups during his method of analysis? Most protein chemists will consider the latter hypothesis the more probable.

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