society in 1859, after which the society's Medal was presented to Mr. Emil Montag, Swiss consul in Liverpool, for services rendered to the society during his twenty-four years' active membership, his editorship, his contributions to British and Swiss geology and his work in providing facilities for study in Switzerland. Professor H. H. Read, Herdman professor of geology in the University of Liverpool, vice-president of the society, then delivered a short lecture on earthquakes, followed by a demonstration of the university seismograph. Dr. E. Neaverson lectured on paleontological exhibits, and there was a demonstration of rock-cutting and of new maps. Amongst the exhibits was a collection of fossils, new instruments and minerals, the latter including specimens of two new British minerals recently found in Scotland; chondrodite, found in association with metamorphic limestone, and stichite, found in association with ultra-basic rocks.

LEGISLATION to enable the dairy industry of California to make and enforce marketing agreements in the state, is to be proposed in the forthcoming session of the legislature by the Dairy Control Legislative Committee, of which W. J. Kuhrt, of Los Angeles, is chairman. To prepare the proposed laws, an advisory drafting committee has been named, with F. H. Abbott, of the dairy industry division of the University of California at the Branch of the College of Agriculture, as chairman. Other members of the committee are: A. A. Brock, director of the State Department of Agriculture; F. T. Robson, chairman of the California Farm Bureau Dairy Department; G. E. Gordon, dairy specialist of the University's Agricultural Extension Service; M. R. Benedict, acting director of the Giannini Foundation of the College of Agriculture; J. H. Kagler, of the California Milk Institute; Stewart Westover, of the Evaporated Milk Agreement, and J. W. Pauluchi, dairy producer. The bill will be prepared in two parts, one covering market milk and the second covering the various other dairy products. It will be based on consumers' areas, rather than on production.

DISCUSSION

THE NATURE OF ENZYMES

I AM obliged to Professor James B. Sumner[1] for concluding his criticism of my note on "Enzymes, Vitamins, and the Zone of Maximum Colloidality"[2] with reference to the subsequent paper of H. Theorell,[3] for this is confirmation and extension of the experimental evidence (though Dr. Sumner says none exists) favoring the possibility (suggested by me but considered improbable by Dr. Sumner) that excessive kinetic activity might be a factor in reducing enzyme efficiency. Those familiar with the ultramicroscopic behavior of particles of colloidal dimensions (including enzymes) realize that when flavine/protein complexes are split, the higher particulate activity of the fragments might be a factor tending to reduction of enzymic action. This does not, of course, exclude the operation of other factors which I mentioned as basic, *e.g.*, the annihilation of, or unfavorable orientation of specific electronic areas.

Similar failure to understand physico-chemical factors appears in Dr. Sumner's attack[4] on the carrier or *Träger* theory of Willstätter. Deprecating the analogy drawn by Waldschmidt-Leitz[5] between "one of our best known proteins, namely hemoglobin," and catalase, Dr. Sumner states: "If the carrier acted merely as a protective colloid, then hematin suspended in almost any lyophylic colloid should possess high catalase activity; such, however, is not the case." Here Dr. Sumner has bludgeoned a straw man and shown himself oblivious to the vital factor of molecular orientation, developed in the researches of W. B. Hardy, I. Langmuir, W. D. Harkins, N. K. Adam and many others.[6] It is well known that many colloidal protectors inhibit catalytic action. Only such carriers can be effective as hold a prosthetic group in proper orientation, and/or serve to form or complete a specific electronic area.

Willstätter[7] has recently proposed the term *"symplex"* for compounds where high-molecular substances are bound by residual valencies—*e.g.*, a prosthetic group and a high-molecular carrier. Symplexes are distinguished from mere mixtures by one or more of the following characteristics: (1) alteration or enhancement of specific reactivity of one component; (2) change in solubility or dispersion of one component; (3) change in optical properties; (4) change in stability; (5) change in toxicity; (6) change in reactions, *e.g.*, color reactions. Among the symplexes discussed are enzyme compounds with substrates, activators, inhibitors and adsorbents; toxin-antitoxin; hemoglobin; and O. Warburg's oxidation enzyme (which was considered by Theorell[3] and by me.[2]

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[1] SCIENCE, 80: 429, 1934.

[2] SCIENCE, 80: 79, 1934.

[3] *Biochem. Zeit.*, 272: 155, 1934.

[4] SCIENCE, 78: 335, 1933.

[5] SCIENCE, 78: 189, 1933.

[6] Early in this century Devaux (see review in Smithsonian Annual Reports, 1913) showed that if a lens of fatty acid is allowed to chill on water and is then carefully dried, the air/acid interface repels water, whereas the water/acid interface can be wetted.

[7] Willstätter and Rohdewald, *Zeits. physiol. Chem.*, 225: 103–24, 1934. G. Bredig (*Biochem. Zeit.*, 250: 414, 1932), by adding amino groups to fibers (cellulose, wool, silk), produced catalysts which split off CO_2 from bromcamphocarbonic acid.

Close association of particles, whether considered as "chemical" or "physical," is naturally followed by changes in kinetics and surface specificities. Professor Hugh Taylor (Princeton) has shown that with many catalysts activity is mainly due to a relatively small number of very highly efficient minute areas.

Apparently, Dr. Sumner[1] quoted Theorell's paper because it reports the *crystallization* of the oxidation enzyme. Assuming that activity is not due to an adsorbed impurity, crystallization of a substance is no evidence whatever that the same substance may not form a colloidal dispersion. For many years, teachers, writers and texts have perpetuated the erroneous notion that if a substance crystallizes it can not be a colloid, despite the fact that Thomas Graham, in his pioneer papers, clearly pointed out that the same substance (*e.g.,* silica) may exist either in crystalloidal or in colloidal form. We now know that *colloidal particles may be crystalline.*[8] Graham stated: "The inquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidality may not really be this composite character of the molecule."

There is no lack of evidence that enzymes are colloidal when in active dispersion. An interesting sidelight on the states bordering on crystallinity has just been thrown by Astbury and Lomax[9] by their x-ray studies of the interaction of water and proteins, including pepsin and trypsin. Furthermore, Svedberg's ultra-centrifugal researches show how protein molecules form groups of varied size, kinetics and external residual fields of force, depending on conditions. And I must repeat: "At the lower range of the colloidal zone, we have a reconciliation between the chemical and colloidal aspects of living matter"—even if Dr. Sumner refuses to be reconciled. Scientists should not attempt to force nature into a stoichiometric strait-jacket.

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THE DENSITY OF WATER IN RELATION TO ITS THERMAL HISTORY

THERE has recently been accumulated a considerable body of experimental data[1] which is now being interpreted on the assumption that water which has shortly before been ice (called ice water) has an internal structure different from water which has shortly before been steam (called steam water). It is postulated that ice water at a definite temperature contains a greater proportion of the polymerized or bulky molecules of water than does steam water at the same temperature; this difference in extent of polymerization gradually decreasing with time until both the ice water and steam water become identical.

It has occurred to us that an unambiguous test of these assumptions could be made by measuring the density of ice water and steam water by the methods which we have been using in connection with another research.[2] Since "trihydrol" is supposed to have a density of 0.88 and "dihydrol"[3] 1.08942, and since we can measure relative densities with an error not greater than one part per million (p.p.m.), it is evident that we should be able to detect a slight difference in the percentage of trihydrol. A study of the densities of ice and steam water was chosen for the reason that the presence of impurities would increase the density of water, whereas the presence of a greater proportion of the trihydrol would decrease the density of water.

Ordinary laboratory distilled water was redistilled from alkaline permanganate and the temperature of floating equilibrium of the totally immersed float measured at 23° C. Without removing the water from the cylinder, approximately four fifths of the water was frozen, then remelted and the temperature of floating equilibrium again determined as quickly as possible. The temperature was exactly the same as before; therefore, the freezing and melting of the water and ice produced no detectable change in the internal structure of the water. The final temperature was measured within an hour from the time that the ice melted, and a significant change in density could have been detected within half an hour. It is believed by some, however, that large block commercial ice on melting gives more trihydrol than does ice made from laboratory distilled water. Accordingly, we melted two selected blocks of clear commercial ice and within an hour from complete melting again measured the temperature of floating equilibrium. The temperature this time was higher than the two previous measurements by 0.010° C., which indicates that this ice water has a greater density than highly purified water to the extent of 2.4 p.p.m. Again there was no indication of an enhanced percentage of "trihydrol" of the ice water as compared to steam water. We conclude that the biologists and others should explain their data on hypotheses different from the hypothesis of a greater trihydrol content of ice water.

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[8] See, *e.g.,* P. Scherrer, *Nachr. Ges. Wissen.* Goettingen, 96, 1918.

[9] See *Chemistry and Industry,* November 16, 1934, report of Manchester meeting, on November 10, of the Chemical Society.

[1] For a recent complete bibliography and discussion see T. C. Barnes and T. L. Jahn, *Quart. Rev. Biol.,* 9: 292, 1934.

[2] M. Dole, *Jour. Chem. Phys.,* 2: 337, 1934.

[3] See note 1.