SCIENTIFIC BOOKS

TEXT-BOOKS ON COLLOIDAL CHEMISTRY

A writer, recently commemorating the centenary of a famous volume, remarked that no science rises above its text-books. I sincerely hope that this is not true, but if it is true, then we are under strict obligation to see to it that our text-books maintain a high standard. In many ways, this is a difficult if not impossible task, but, through cooperative work, it is at least possible to prevent the repetition from text-book to text-book of errors to which critics have repeatedly called attention. I am told that in past years some text-books in zoology carried the obviously erroneous statement that the erythrocyte of the camel was the only known nucleated mammalian red blood cell. The error persisted for thirty-five years until some one had the ingenious idea of going to the zoo, getting some camel's blood and looking at it. Of course, he found that the cells, like all other mammalian red blood cells, were without nuclei.

It is about thirty-five years since Wolfgang Ostwald presented his "emulsoid" classification of colloids, and it is nearly twenty-five years since Hatschek stated that the emulsoid nature of jellies is untenable. It is now more than a dozen years since I called attention to the confusion which the term emulsoid has caused, and pleaded for its rejection.

A new biological text has just come into my hands. The author states that "protoplasm is an emulsion or, technically, an emulsoid," and that it may exist as a sol or gel. Aside from the fact that protoplasm is only superficially an emulsion, and the fact that the recognized technical name of an emulsion is not emulsoid, there is the further fact that a pure emulsion can not form a gel.

If the author of a biological text turns to a book on physical chemistry he may find the matter accurately presented, as it is in Getman and Daniels, but very briefly so and without a colloidal flavor. There will probably be no mention of jellies. If he turns to a volume on colloidal chemistry or biochemistry he may or may not find the matter correctly put. Thus, the lone statement that "emulsoid usually emphasizes a combination of the phases" is but feebly true and misleading.

Gortner enumerates the customary eight classes of colloids, and under liquid-in-liquid systems discusses gelation and the properties of lyophilic colloids in general. He there makes no mention whatever of the true liquid-in-liquid systems, the emulsions, but later under a separate heading takes them up. To call gelatin a liquid-in-liquid system is enough to coagulate the blood of any living member of that noble assemblage of colloid chemists which includes such familiar names as Donnan, Ellis, Hatschek, Freundlich, Proctor and Wilson, and to these I feel confident that I can add Staudinger, Sheppard, Kraemer and Williams.

Liquid-in-liquid systems are emulsions, and if they must be below the limit of microscopic vision they can be made so. It was Donnan and Ellis who pointed out that pure colloidal emulsions are model *suspension* colloids, and therefore of the same class as solid-in-liquid systems, such as colloidal gold. Gelatin is but feebly related to the suspensions, so little so that Duclaux in 1925 excluded the solid and liquid suspensions from his book on "Les Colloids," which shows that all along some workers have understood and some have not.

Classifying gelatin as a liquid-in-liquid system is the kind of reasoning which leads to confused situations such as the attempt to distinguish between emulsions, emulsoids and jellies, by defining emulsoids as stabilized emulsions. This is probably an effort to explain why emulsions such as milk and latex coagulate. The misunderstanding is a common one and is due to the fact that natural emulsions are not pure systems. Milk is certainly an emulsion, but it just as certainly contains casein, and when milk curdles it is the casein that does so, the emulsion or butter-fat having nothing whatever to do with it.

The same situation arises in connection with protoplasm. It is impossible to characterize protoplasm as this or that kind of system except by some such all-inclusive term as polydispersoid, which tells very little except that protoplasm is exceedingly heterogeneous.

I have never been a strong advocate of precise terms and final definitions. A name too often takes the place of an idea, frequently to obscure an erroneous idea. Yet, a language consists of words and only through them can we express our thoughts.

Relatively little attention has been given to colloidal nomenclature. Rheologists have made a thorough study of the terms of their science, and the anatomists and taxonomists hold conventions for this sole purpose. It is twenty-five years since Thomas wrote that the use of the term gel "is deplorably loose and confusing." I doubt if its use is any more specific to-day.

In many instances the loose use of an expression is not serious. Thus, "colloidal solution," deplored by some, is much used by others. Zsigmondy thought it not inaccurate. It at least does little harm because of the adjective colloidal. Quite other, however, is it with that *bête noir,* the word "emulsoid." In order that this troublesome term may once and for all be thrown out, and also that other erroneous or misleading words and ideas may be eliminated or corrected, I suggest that the Colloid Chemical Division of the American Chemical Society and the Colloid Committee of the National Research Council officially adopt an at least tentative nomenclature.

Not only terms and definitions should be given consideration but some antiquated text-book ideas as well. One which I should like to have dealt with is the following. Several years ago I was an "expert witness" in a legal trial having to do with the harmful effects of sulfuric acid "fumes," or mist, set free in a commercial plant. The opposition pointed out that there could be no fumes because of the very low vapor pressure of sulfuric acid, to which all agreed, but I held it was mist, and not fumes, with which we had to deal. The opposition, still preferring to condense fumes which were not there rather than disperse the liquid acid by spattering, insisted on the presence of dust in order to produce mist, but owing to wet floor and walls no dust could be present. Their contention that dust must be present was based on the old text-book statement that atmospheric vapor is condensed on dust particles, forming colloidal droplets which, suspended, constitute mist. Though the discussion seemed to me irrelevant, for acid was being dispersed and not fumes condensed, I nevertheless answered the question on the need of dust particles in the negative, and was adjudged in error. The "expert witness" of the opposition was not a colloidal chemist, so, of necessity, had accepted what he had read in colloidal text-books.

Texts are so often assumed to be a collection of facts, when, actually, they are a collection of opinions. That mist results from the condensation of atmospheric moisture on the surface of dust particles is an opinion that I have always doubted. It persists as a text-book hypothesis, a relic of the early days of colloidal chemistry. There is no reason why the aggregation of atmospheric moisture should necessarily take place only under special conditions requiring nuclei, when so many other forms of colloidal and molecular aggregation take place without nuclei. Nuclei are not necessary for the precipitation of solutions, the formation of colloidal suspensions from matter in solution, the production of gels by coagulation of colloidal dispersions and the agglutination of living cells in suspension.

As in the case of salts crystallized out of solution, nuclei may hasten the process, but they are not necessary. Bancroft states that dust is not necessary for the production of mist, though nuclei cause the formation of mist at lesser supersaturations than would ordinarily be necessary.

We move on rapidly to an understanding of profound and far-reaching problems and leave many simpler questions just where they were in the early days of our science.

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SOCIETIES AND MEETINGS

THE JUNE SPECTROSCOPY CONFERENCE AT THE UNIVERSITY OF CHICAGO

FROM June 22 to 25 a conference was held at the University of Chicago, consisting in a series of symposia on various pure science aspects of spectroscopy. The program and the participants ranged over the fields of chemistry, physics and astronomy. Papers were presented by thirty invited speakers at eleven sessions, including some discussion papers prepared in advance. The estimated total attendance was between 250 and 300. The papers and discussion are being published as the April–July number of *Reviews of Modern Physics*.

After an introduction by the writer describing the background and purposes of the conference, the first morning session, on Monday, June 22, consisted in a symposium by physicists on "Spectroscopic Methods." Henry G. Gale, of the University of Chicago, who had agreed to act as chairman at this session, was prevented from doing so by a recent operation. W. F. Meggers, of the Bureau of Standards, W. E. Williams, of University College, London (now in Pasadena), and H. G. Beutler, of Chicago, were the chief speakers in discussions on standard wave-lengths and the concave grating. The use of the isotope 198 of mercury, obtained by transmutation of gold, as the source of a new primary standard of wave-length was proposed. The afternoon session was a symposium by astronomers and astrophysicists on "The Spectra of Comets." The speakers were N. T. Bobrovnikoff, Perkins Observatory; G. Van Biesbroek, Yerkes Observatory; A. McKellar, Dominion Astrophysical Observatory, and P. Swings, Yerkes Observatory. In addition, a paper was communicated by G. Herzberg, professor of physics at the University of Saskatchewan, in which for the first time a polyatomic molecule, the CH_2 radical, was identified as giving rise to cometary spectra. This was particularly interesting in that the spectrum of this important radical had not hitherto been known. Since the conference, Dr. Herzberg has reproduced the cometary CH_2 spectrum in a laboratory discharge tube. At the Monday evening session, four physicists who have worked in the field took part in a very satisfying symposium on "Atomic Beam Spectra." They were K. W. Meissner, Purdue; R. A. Fisher, Northwestern; J. E. Mack, Wisconsin, and W. E. Williams, Pasadena.

The Tuesday morning program on "The Earth's