tee 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

Atmosphere and the Constitution of the Planets" was made up of papers by F. L. Whipple, Harvard Observatory; C. T. Elvey, McDonald Observatory, and R. Wildt, Princeton, with O. R. Wulf, Chicago, as chairman. The afternoon session on "Atomic and Molecular Spectra" returned to the physicists with papers by W. F. Meggers, J. E. Mack and G. Herzberg. Dr. Meggers showed how the very complicated spectra of the rare earth metals are at last being unravelled. The conference dinner on Tuesday evening was followed by brief talks by A. H. Compton, O. Struve, K. K. Darrow, J. Franck and W. F. Meggers. Professor Struve spoke on "Astronomy Faces the War."

On Wednesday and Thursday the program dealt mainly with the spectra of increasingly complicated molecules, with chemists and physicists taking part. The Wednesday morning symposium, with four physicists participating, was on "Triatomic Spectra." The speakers, E. F. Barker, Michigan, R. S. Mulliken, Chicago, S. Mrozowski, Chicago, and H. H. Nielsen, Ohio State, discussed both infrared and ultraviolet spectra. In the afternoon, physical chemists and a physicist took part. The speakers were W. H. Rodebush, Illinois; Miss H. Sponer, Duke; A. L. Sklar, Catholic University, and A. Turkevich, Columbia, with K. F. Herzfeld, of Catholic University, as chairman. The discussion referred largely to ultraviolet spectra of benzene and its derivatives. Professor Rodebush mentioned that the absorption spectrum of rubber changes on stretching. At the evening session on "Spectra of Dye Molecules," after an introduction by W. G. Brown, Chicago, L. G. S. Brooker, Eastman Kodak Research Laboratory, organic chemist, presented empirical generalizations based on a wealth of examples, and A. L. Sklar, presenting work by himself and K. F. Herzfeld, showed that these can be explained remarkably satisfactorily by quantum mechanics.

On Thursday morning, Miss E. P. Carr and Miss L. W. Pickett, of Mount Holyoke, described their extensive work on diene spectra (including butadiene); R. S. Mulliken and Mrs. C. A. Rieke discussed some quantum-theoretical studies on benzene and other molecules, and Mrs. M. Goeppert-Mayer, Columbia, reported the results of quantum-mechanical computations on the Wurster salts; G. W. Wheland, Chicago, was chairman. The Thursday afternoon and evening sessions were on "Cooperative Spectra," with J. Franck and P. Pringsheim, of Chicago, as chairmen. S. E. Sheppard, Eastman Kodak Research Laboratory, presented much interesting material about dye spectra. E. Rabinowitch, Massachusetts Institute of Technology, and S. Freed, Chicago, discussed the spectra and structure of ion complexes. P. Pringsheim, Chicago, described experiments on absorption and phosphorescence of potassium-thallium-halide phosphors, carried out in Belgium and Berkeley. G. N. Lewis, who had expected to give a paper, was prevented from coming by last-minute developments.

In the introduction on the first day, the writer described the background and purposes of the conference, then expressed the belief that the maintenance of at least a minimum of fundamental research activity even in those fields of pure science having no obvious connection with the war effort, is wise national policy. This talk, with some omissions and changes, was as follows:

Viewed broadly, spectroscopy is a very large subject, with ramifications in many directions in physics, astronomy, chemistry and biology. The data of modern astrophysics, for example, are obtained very largely through the use of the spectroscope.

In recent years, a number of very successful conferences have been held which have been devoted primarily to the practically very important field of the application of spectroscopy in spectrochemical analysis. Less attention has been given lately to spectroscopy as pure science.

It therefore seemed to us that the time was ripe for a conference on spectroscopy to present the broad picture of spectroscopy as pure science.[1] We also thought it would be desirable to further better acquaintance and a stronger consciousness of underlying unity among those using spectroscopy in different fields of pure science. We therefore planned a conference to consist of a number of symposia, each in a special field connected with or based on spectroscopy, and so arranged as to cover each day a considerable range of subjects. In this way, we hoped that visitors would find some familiar things and also other less familiar things to interest them and to bring them into contact with others in different fields. For instance, the astrophysicists might exchange ideas with the pure spectroscopists or with those interested in the structure of atoms or molecules; the ultraviolet spectroscopists with the infrared spectroscopists; or the chemical physicists with the organic chemists interested in spectra.

Last winter we were for a time in doubt as to whether to go ahead with our plans. There were two questions. With so many scientists going into war activities, it might have been that too few could find the time to come. The second question was that of the relative importance of pure science in wartime as compared with science applied directly to winning the war.

[1] The committee on the conference consisted of O. Struve (astronomy); J. Franck and W. G. Brown (chemistry) and R. S. Mulliken (physics).

As far as speakers were concerned, we soon found that most of those whom we approached were able and willing to come. This to a large extent answered the first question. It also helped in answering the second. The fact that a large proportion of the speakers were engaged in war work, some very extensively, indicated that they agreed with us as to the importance of maintaining the development of pure science where possible.

I think we are all convinced that pure science research is a matter of very great long-run value to the nation. Granting this, there are two reasons why it is important now to keep it going. In the first place, we do not know how long the war will last; there is enough of a probability that it will last for a long time, so that weight should be given to activities of long-run importance even for their possible value in winning the war. In the second place, weight also should clearly be given to pure science so that the nation's scientific foundations will be strong when peace finally comes.

There are some fields of pure science which are so fortunate as to have had their development greatly accelerated as a direct part of the war program. Here the path is clear. In other fields, however, which are no less important for the progress of science in the long run, the effect is reversed. Although, in general, priority must be given to the fields of direct short-term value, nevertheless I am convinced that workers in the long-term fields should feel that they too are making a valuable contribution to the national effort by carrying on their work as effectively as possible.

Quoting a letter received this spring from a British colleague, "I am so sure that above all we must see that some fundamental research tradition is preserved at our universities. There is a danger here in Britain of it stopping, through sheer pressure of work. I hope that it won't stop with you in America. In the last war we lost about 15 years in our British universities through it; we must not let that happen again."

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SPECIAL ARTICLES

GROWTH OF CANCER TISSUE IN THE YOLK SAC OF THE CHICK EMBRYO

BY the simple process of injecting a suspension of tumor cells directly into the yolk, we have succeeded in growing cancer tissue in the yolk sac of the developing chick embryo. Tumors up to 3.5 grams in size have been produced in this manner with an initial inoculation of .05 gram of tumor tissue.

It has been known for some time that cancer tissue can be grown on the chorio-allantoic membrane of the developing chick embryo.[1] This technique, however, has proved to be somewhat limited in its scope, since it involves removing a piece of the egg shell and the depositing of the tumor tissue directly on the chick membranes. This effects considerable interference with the embryo and the mortality rate among eggs so treated is about 65 per cent., according to Stevenson.[2] Further, the initial inoculation must be small (.003 to .005 grams), and large tumors can not be produced regularly because they are likely to interfere with the growth and development of the chick.

In the present method each egg can be inoculated in a few seconds, the mortality rate is little more than that of untreated eggs, and 100 per cent. takes can be expected.

Mammary carcinoma transplants of the DBA and C_3H strains of mice were used. Moderate size tumors (1 to 2 grams in weight) were dissected out aseptically and squeezed through muslin cloth so as to disperse the cancer tissue. This material was diluted with saline solution to the extent where each ml of suspension contained about .2 gram of tumor tissue. Tumors which had external lesions and were infected could not be used as donors. It is well known that tumor transplants in mice may grow in an apparently normal manner even when some bacterial infection is present. Such tumor material naturally could not be used for injection into the egg yolk. The presence of necrotic tissue in the injected material also resulted in the death of the embryo.

Fertile eggs after incubation for 4 or 5 days at 38° C were used for inoculation. A needle-sized opening was made in the shell area over the air sac and .25 ml of the tumor suspension was injected hypodermatically into the yolk, using a 20-gauge needle 1¼ inches in length. The opening in the shell was then sealed over with cellulose tape. It has been our experience that the egg can accommodate a much heavier inoculation of tumor tissue.

After inoculation, the eggs were incubated at 37° C for 12 or 13 days or until the total incubation time was 17 days. The injected tumor tissue became attached to the inner wall of the yolk sac from which it obtained its blood supply. The bulk of the tumor, which tended to conform in appearance with the mouse-grown variety, grew down into the yolk of the yolk sac cavity. In this position there was plenty of room for growth without mechanical interference with the embryo mem-

[1] J. B. Murphy, *Jour. Am. Med. Asn.*, 59: 874, 1912.
[2] H. N. Stevenson, *Jour. Cancer Research*, 3: 63, 1917.