ceived too late to be so published will be brought to the attention of the International Commission at the time of the commencement of voting on the application in question.

5. Under the decision by the International Congress of Zoology, the period within which comments on the applications covered by the present notice are receivable is six calendar months calculated from the date of publication of the relevant parts of the *Bulletin of Zoological Nomenclature.* The five parts now in question were scheduled to be published on September 28, 1951. In consequence, any comments on the applications published in these five parts should reach the Secretariat of the International Commission at latest by March 28, 1952.

FRANCIS HEMMING
Secretary to the International Commission on Zoological Nomenclature

Bovine Albumin Standard for Serum Protein Determinations

THE usual procedures for the determination of serum proteins are the falling drop, Kjeldahl, and the biuret reaction. The biuret method depends upon the use of pooled sera as a standard protein solution. Anyone experienced in the use of the latter knows the unreliability of such solutions as standards, and the difficulty encountered in the duplication of calibration curves with the same biuret reagent and a new lot of pooled sera. In addition, their keeping qualities are very poor. We have found that the only reliable and reproducible standard for protein estimations is a pure protein such as albumin. We have employed such a standard in all our protein work for a considerable length of time and have found it to be completely satisfactory.

We originally employed a salt-poor 25% human albumin solution as standard. Protein concentrations varying between 100 mg and 6.0 g % were prepared by dilution with physiological saline. The protein content of each of the diluted solutions was determined in triplicate by the Kjeldahl method ($N \times 6.25$). For the biuret reaction, the procedure described by Gornall, Bardawill, and David (*1*) was used. Suitable calibration curves were prepared from the results of each set of determinations. Exactly the same procedure was employed, using pooled sera as the standard. Under the same experimental conditions, when protein determinations were made upon fresh specimens of serum, it was repeatedly observed that the readings from the albumin curve gave the most consistent and reproducible results.

The use of human albumin is not feasible in general routine work, because of its high cost and unavailability; however, we have found that bovine albumin is a perfect substitute for the preparation of protein calibration curves. Lever and his co-workers (*2*) have suggested the use of crystalline bovine albumin for the preparation of a calibration curve obtained from biuret determinations, but this requires very accurate weighing and is time-consuming. The present communication proposes the use of a bovine albumin solution as a protein standard. It is a sterile, stable solution, the albumin content of which has been carefully standardized by its nitrogen content.[1] The bovine albumin curve obtained from the biuret determinations can be superimposed upon the curve obtained by the use of human albumin.

The most important features of the proposed bovine albumin solution for the calibration of serum protein curves are: (1) The solution keeps indefinitely; (2) the protein content is guaranteed to be accurate; (3) it may be used to spot-check reagents and previously prepared calibration curves; (4) it is economical to use; (5) it is easily obtainable. The use of this bovine albumin solution offers clinical biochemical laboratories a means of standardization of serum protein determinations hitherto unobtainable.

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1. GORNALL, A. G., BARDAWILL, C. J., and DAVID, M. M. *J. Biol. Chem.*, **177**, 751 (1949).
2. LEVER, W. F., *et al. J. Clin. Invest.*, **30**, 99 (1951).

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[1] We are indebted to Albert H. Holland, Jr., of Armour Laboratories, Chicago, for supplying the standard bovine albumin solution.

Catalysis and Olfaction

I AM preparing a paper on the sense of smell for Vol. VIII of the international series "Colloid Chemistry, Theoretical and Applied," and will appreciate receiving any germane reprints and information, so that they may be given consideration.

Most of the papers dealing with olfaction published recently in SCIENCE center about the views of L. H. Beck and W. R. Miles (SCIENCE, **106**, 511 [1947]), which Professor Pfaffmann in Steven's recent *Handbook of Experimental Psychology* (New York: Wiley [1951]) characterizes as a restatement of the infrared theory in vogue "ever since Faraday first noted that many odorous materials strongly absorb radiation in the infra-red region of the spectrum." All these writers were obviously unaware of the catalyst theory of olfaction advanced by J. Alexander in *Colloid Chemistry*, 4th ed. (New York: Van Nostrand [1937]), which was epitomized in *Chemical and Engineering News* of Aug. 1, 1949 (p. 2227) as follows: "The odor-producing substances affect the catalyst balance of the olfactory cells by any or all of the following mechanisms: (1) by modifying existing catalysts, an effect analogous to that of promotors in commercial catalysts; (2) by forming new catalysts (neocatalysts); (3) by inhibiting all or part of the activity of normal cellular catalysts—e.g., H. S. Taylor showed that catalysts may have several different specific catalyst areas, which can be selectively inhibited."

About a year after publication of the above abstract, G. B. Kistiakowsky (SCIENCE, **112**, 154 [1950]), obviously unaware of these earlier publications of the catalyst theory, advanced as something new an emasculated form of it, limiting it to inhibitions only, but giving cogent reasons showing its merits. So far as I know, this note of Professor Kistiakowsky is the only paper on olfaction published in SCIENCE mentioning the catalyst theory.

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A Proposed New Name for the Cohesion Theory of Water Ascent in Plants

AFTER many years of experience attempting to teach students in general botany and plant physiology something about the Dixon theory—or cohesion theory, as it is usually called—of the ascent of water in plants, the writer has reached the conclusion that a better and more descriptive name for this process is needed.

The term "cohesion theory" merely describes a property of water which makes possible this method of flow. It does not give an indication of more fundamental factors, such as the source of energy involved in the flow or the state of the water while it is moving. The terms "transpiration pull" and "transpiration stream" are to some extent immune from these objections, but they imply that transpiration is an essential factor in the process and fail to make clear that any process in cells of the shoot that uses water operates in the same way as transpiration in increasing the DPD of the water in the cells and in effecting a consequent ascent of water. The use of the term "cohesion theory" in a sentence often results in a rather awkward statement.

The writer is proposing the term "shoot tension" as being more precise and descriptive, and definitely homologous with the term "root pressure." The term shoot tension localizes the source of the motive power in the cells of the shoot, whether they happen to be in a leaf, stem, or reproductive organ; it describes the state of the water in the xylem during its ascent; and it avoids undue stress of the role of transpiration. Note how much easier it is to say, "The ascent of water in plant A is due to root pressure, whereas its ascent in plant B is due to shoot tension," than it is to say, "The water in plant A is ascending because of root pressure, whereas the water in plant B is ascending in accordance with the cohesion theory (or cohesion, or cohesion of water)."

Students on whom the term shoot tension has been tried seem to grasp the idea faster than when the term cohesion theory is used. The writer is considering using the term shoot tension in a general botany textbook on which he is now working, and would therefore be interested in having the reactions of plant physiologists, teachers of general botany, and other botanists to the term. The writer is under no illusions as to the difficulty of introducing a new term as a substitute for a widely used old one, even though it may be much better. The slow progress in the substitution of "ovulary" for "ovary" is an example from another phase of botany. However, even slow progress is probably better than none.

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Book Reviews

The Chemistry and Action of Insecticides. Harold H. Shepard. New York–London: McGraw-Hill, 1951. 504 pp. $7.00.

Insect Control by Chemicals. A. W. A. Brown. New York: Wiley; London: Chapman & Hall, 1951. 817 pp. $12.50.

Two books have recently appeared that are of particular interest to the entomologist, the agriculturalist, the agricultural chemist, and the pesticide manufacturer. More than this, since insect control by chemicals cuts across many fields, these works discuss subjects of wide importance that should commend them to the attention of all those concerned with the effects of toxic chemical substances on animals and plants.

The Chemistry and Action of Insecticides is the enlarged and extended outgrowth of *The Chemistry and Toxicology of Insecticides* (1939), by the same author. In the opening chapter one is introduced to the enormous losses to crops, farm animals, and foodstuffs caused by insects and to the important place insecticides hold in reducing these. The history of insecticides is discussed briefly here (and frequently again in later chapters); a table presents the important events in the development of insecticides from about 1000 B.C. through 1948. U. S. federal regulations concerning adulteration, misbranding, and pesticide residues on food products receive brief comment, and a list of important books, periodicals, and abstract journals is appended. This chapter, like the succeeding ones, is concluded by a list of literature citations.

Then follow chapters on the various groups of insecticides—the elements, inorganic compounds, and mixtures that have received experimental trial or have gained a place in the practical control of insects; insecticides derived from plants; the oils, soaps, and creosotes; and the synthetic organic insecticides. Separate chapters are devoted to the more general aspects of chemical control (particle size, shape, and density of dusts; wetting and spreading of sprays; adsorption of gases; and related subjects), to relative toxicity and mode of action, and to the attractants and repellents. An appendix contains a conversion table of weights,