Technology to be held in Pittsburgh on October 20. Dr. Jordan's address will be the concluding event in an all-day meeting which will be held at the Carnegie Institute of Technology. At the morning and afternoon sessions reports of researches done in the metallurgical laboratories at the institute will be given.

THE twenty-third annual clinical congress of the American College of Surgeons opened at the Century of Progress, Chicago, on October 8 under the presidency of Dr. J. Bentley Squier, professor of urology at Columbia University.

THE annual meeting of the American Public Health Association was held in Minneapolis from October 9 to 12. Dr. John A. Ferrell, associate director of the International Health Division of the Rockefeller Foundation, is president. He will preside at the first general session and will make an address on "America's Contributions and Problems in Public Health." At the banquet on October 9 as many as possible of the living members of the U. S. Army Yellow Fever Commission who volunteered as subjects in its historic experiments, will be present. Major Walter Reed, chairman of the Army Commission, made the first announcement of his success in transmitting the disease by the mosquito at the meeting of the American Public Health Association at Indianapolis just thirty years ago. The Surgeon General of the U. S. Army, Major General R. U. Patterson, and General Frederick F. Russell, director of the International Health Division of the Rockefeller Foundation, made the principal addresses.

THE one hundredth anniversary of the founding of the Faculty of Medicine of the National University, Mexico City, will be celebrated in October. There will be a week of "Medical Days," October 23–28, during which lectures, clinics and laboratory demonstrations will be given in public and private hospitals. The committee in charge of the centenary celebration has invited the governments of several countries and the principal universities of the world to send representatives and is preparing a program of entertainment.

THE British Institute of Metals, according to *Nature*, held its silver jubilee autumn meeting in Birmingham from September 18 to 21, under the presidency of Sir Henry Fowler. The institute was founded in Birmingham twenty-five years ago. An inaugural lecture on "Twenty-Five Years' Progress in Metallurgical Plant" was given by Mr. W. R. Barclay, vice-president, and a series of fourteen papers dealing with various phases of metallurgical work are to be delivered. A feature of the meeting was the reproduction of the first office of the Institute of Metals. This was housed in the metallurgical department of the University of Birmingham, and was set up by the institute's first and only secretary, Mr. G. Shaw Scott, who, with the president and the chairman of the executive committee, will receive the remaining original members of the institute in the reconstituted office. Here visitors will have an opportunity of inspecting the earliest records of the institute—including the original membership applications and photographs taken during the inaugural meeting in 1908. During Mr. Shaw Scott's period of office as secretary and editor the membership of the institute has increased from 200 to 2,200. Whereas originally the membership was confined almost entirely to Great Britain, it now covers the whole world. The institute removed in 1909 to London, where it now has offices at 36 Victoria Street, Westminster, S.W.1.

A RESOLUTION was adopted at the recent Leicester meeting of the British Association for the Advancement of Science "to request the council to consider by what means the association, within the framework of its constitution, may assist towards a better adjustment between the advance of science and social progress, with a view to further discussion at the Aberdeen meeting."

# DISCUSSION

## PRONUNCIATION OF BOTANICAL TERMS

THE diverse pronunciation of botanical terms aroused H. A. Gleason[1] in 1932 to publish an article on that subject. He found that some of the causes for diversity may be attributed to the conflict between the properties of classical and modern languages. Rather guardedly he suggested as a remedy six arbitrary rules, the aim of which is to cause the root elements of spoken scientific terms to be heard as distinct, undistorted entities. Except for an apparent minor blemish in that the vowels of some syllables illustrating the application of the rules are not marked according to familiar dictionary usage for syllables ending with consonants, this is an admirable paper and should receive wide circulation. Its proposals should be accepted and practised universally by English-speaking botanists and zoologists.

Although Dr. Gleason dealt with some of the difficulties attending the pronunciation of scientific names derived from personal and other proper names, he did not consider the surgical method I shall now propose for treating commemorative genitives. According to the codes of nomenclature, these names receive the

[1] H. A. Gleason, "The Pronunciation of Botanical Names," *Torreya*, 32: 3, 53–58, 1932.

termination *i* or *ii* when masculine and *ae* or *iae* when feminine. My proposal is that these endings be omitted. Thus, instead of writing *Picea engelmanni, Quercus michauxii* and *Pedicularis furbishiae,* I would write *Picea engelmann, Quercus michaux* and *Pedicularis furbish.* This method has a number of distinct advantages, namely, (1) it eliminates tongue-twisting and ambiguous pronunciation. As matters stand now, *engelmanni* may be pronounced engel-mann-ee or engelmann-eye and *michauxii,* michaux-ee-ee or michaux-ee-eye. (2) It honors the commemorated person by giving his name as it actually was or is without distortion. True, there will be no distinction as to sex, but is that necessary in such scientific names? (3) It eliminates a grammatical rule and its subsidiary regulation which says when to use *i* or *ii.* (4) It simplifies spelling. (5) It dispels the misconception of possession implied in the use of the genitive. Possession does not exist in fact in the application of scientific names. I do not object to calling a plant Engelmann spruce or Furbish lousewort, but I am considerably irritated by Engelmann's spruce and Miss Furbish's lousewort. Common sense suggests that while these plants were named for those persons, they do not belong to those persons. In short, such specific terms are functions of the scientific names and not of the persons or places from which they were derived, and the names themselves pertain to the objects named, not to the namer or namee.

The elevation of commemorative genitives to the level of substantives in the nominative without endings would place these specific names on a par with similar substantives derived from mythological or historical characters and exemplified in zoology, as, for example: *Satyrodes eurydice, Pyrameis atalanta, Polygonia faunus, Atrytone pontiac,* and *Poanes massasoit.* The parallel may be extended to include many other names in which the specific name, for one reason or another, is a common noun in the nominative, as *Felis leo* and *Polygonia comma* in zoology and the following in botany: *Brassica napus, Cynodon dactylon, Hicoria pecan, Myrica gale, Prunus cerasus, Quercus ilex, Quercus suber, Salsola kali,* and *Rhus toxicodendron.* In such names as *Crataegus crus-galli* and *Paliurus spina-christi,* which have hyphenated specific names involving phrases, there is justification for the genitive; but the making of such names should be discouraged unless the words be consolidated, as *crusgalli, spinachristi, uvaursi* and *bonanox. Aster novae-angliae* should, in my opinion, be *Aster nova anglia.*

The outcome of a surgical operation may be unfavorable unless important accessory procedures accompany the treatment. Likewise, in this matter of amputating the genitive endings of proper names, serious complications likely to arise from the use of capitals must be anticipated by appropriate measures. Capitalization of some specific names is approved by most botanists, but is discouraged by many others. Zoologists, however, are now almost a unit in opposition to the practise. The botanists hold that specific names are to be capitalized when (1) the name is derived from that of a person or place and (2) the name is the ancient generic name. The omission of capitals in these instances is scored succinctly by Britton and Brown[2] as follows: ". . . Should this custom prevail, much information concerning the history and significance of the specific names would be lost." This argument, it seems to me, can be met easily. (1) The professional botanist who does not recognize the name of Engelmann in *Picea engelmanni* or *Picea engelmann,* no matter what his other qualifications may be, lacks the historical background and perspective of his subject. (2) To all other persons the name *Picea engelmanni* is merely what it is intended to be, namely, a depersonalized label designating a species of tree. (3) The use of the ancient generic name as a specific name does not elevate the total scientific name to greater distinction than it would have if the specific name were some other word; and capitalizing the name adds nothing to its value but really subtracts from its usefulness, because apart from its context it may be confused with a capitalized generic name. (4) The author of a new name is credited by having his name attached thereto; and when he coins new generic and specific names whose derivation and use are not obvious, he owes it to his reader to supply the etymology and all other pertinent information at the time of publication. This should be made mandatory in all codes. Thereafter any one eager to deal with such matters can usually, without much difficulty, discover all the information which Britton and Brown say would be lost by non-capitalization.

The advantages of decapitalization of specific names are so obvious as scarcely to need elaborate mention. (1) Decapitalization gives uniformity of type. In looking down a list of species one does not want to see disfiguring and disconcerting capitals disrupting the sequence. (2) It saves taking routine thought. The simple rule—do not capitalize any specific names—offers the saving short-cut desired by the busy person who cares for the substance, not the superficialities, of things. (3) It gives distinction to specific names as compared with capitalized generic names. Insistence upon the capitalization of specific personal names, while earnest and sincere in

[2] N. L. Britton and Addison Brown, "Illustrated Flora of the Northern United States and Canada," Vol. 1, Introduction, page 11, 1896.

motive, nevertheless seems to me to smack suspiciously of a subtle form of hero or ancestor worship. The botanists should now without hesitation follow the wise leadership of the zoologists in abandoning the capitalization of all specific names. Once this result has been realized and all new specific names derived from common and proper nouns are made substantives in the nominative, chiefly without endings, two forward steps will have been taken toward those much-desired ends, uniformity and simplicity in nomenclature and clearness in pronunciation.

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## THE CHEMICAL NATURE OF ENZYMES

NOT very long ago Willstätter[1] declared that enzymes were not proteins and claimed to have obtained some enzymes wholly free from protein. Now Waldschmidt-Leitz,[2] one of Willstätter's pupils, accepting the ideas of Zeile and Hellström[3] and of Kuhn, Hand and Florkin,[4] compares the enzymes catalase and peroxidase with one of our best known proteins, namely hemoglobin. But he is careful to speak of the hematin[5] as the important part of hemoglobin, catalase and peroxidase; the protein part acts only as carrier. If the analogy between hemoglobin and catalase and peroxidase is correct, then hemoglobin is not a true chemical compound, but merely an adsorption complex; and the properties of hemoglobin, except for quantitative differences, are to be attributed not to the molecule of hemoglobin as a whole, but to the hematin side-chain. In catalase, according to Waldschmidt-Leitz, the hematin is the enzyme proper, or active part. But catalase itself is about ten million times more active in decomposing hydrogen peroxide than hematin is[6]; so it appears to me that the protein carrier deserves considerable credit for the activity of catalase. 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.

Willstätter's[7] carrier, or *Träger* theory has been generally accepted, but in my opinion satisfactory evidence in support of this theory has never been offered. One of the defects or virtues of the theory, depending upon one's point of view, is its indefiniteness, which enables it to be interpreted to suit the occasion. At one time the carrier was simply some colloid which could be replaced by another colloid. Now Waldschmidt-Leitz admits that the carrier is responsible for great quantitative differences in enzyme activity. Waldschmidt-Leitz[8] has spoken of the protein of my urease crystals as a "possibly especially suitable carrier," and Willstätter[9] has mentioned the possibility of a "necessary carrier." As far as I am aware the exact nature of the union between the carrier and the enzyme proper, whether purely physical or weakly chemical, has never been precisely stated.

When making an argument it is customary to take notice of evidence both for and against the point in question. However, Waldschmidt-Leitz does not do this. He says, regarding crystalline urease: "Trypsin digestion of the crystalline protein of urease takes place without significant change in urease activity." He makes no mention of our finding that urease is not digested by trypsin[10]; nor does he allude to our researches which show that crystalline urease is rapidly inactivated by pepsin and papain and that the inactivation by pepsin occurs at the same rate as its digestion.[11] Yet another important point, not mentioned in his criticism of crystalline urease, is the finding by Kubowitz and Haas[12] that crystalline urease has the same type of absorption spectrum as the simple proteins and that this absorption spectrum coincides with the destruction spectrum for urease.

Waldschmidt-Leitz says in his paper: "The finding of crystalline protein-enzyme compounds may lead to the concept that enzymes are merely proteins, and thus cause investigators to disregard enzyme specificity which can only be explained by the existence of highly specialized groups." I think there is little danger of this. The enzyme, as I consider it, is in some cases a simple protein, in others a conjugated protein where the properties are to be ascribed to the molecule as a whole. But whether the specific active groups are in the protein part or in the side chain, the enzyme is a protein, as I demonstrated in 1926.[13]

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