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.

ROLAND W. BROWN
U. S. GEOLOGICAL SURVEY

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]

JAMES B. SUMNER
DEPARTMENT OF PHYSIOLOGY AND BIOCHEMISTRY
MEDICAL COLLEGE
CORNELL UNIVERSITY

[1] R. Willstätter, *Berichte*, 59, 1591, 1926; *Naturwiss.*, 15, 585, 1927.
[2] E. Waldschmidt-Leitz, SCIENCE, 78, 189, 1933.
[3] K. Zeile and H. Hellström, *Zeit. Physiol. Chem.*, 192, 171, 1930; K. Zeile, *Zeit. Physiol. Chem.*, 195, 39, 1930.
[4] R. Kuhn, D. B. Hand and M. Florkin, *Zeit. Physiol. Chem.*, 201, 255, 1931.
[5] Waldschmidt-Leitz employs the term "hemin."
[6] K. G. Stern, *Zeit. Physiol. Chem.*, 215, 35, 1933.
[7] R. Willstätter, *Berichte*, 55, 3601, 1922; 59, 1, 1926.
[8] E. Waldschmidt-Leitz and F. Steigerwaldt, *Zeit. Physiol. Chem.*, 195, 260, 1931.
[9] R. Willstätter and M. Rohdewald, *Zeit. Physiol. Chem.*, 204, 181 and especially 186 and 187, 1933.
[10] J. B. Sumner and J. S. Kirk, *Zeit. Physiol. Chem.*, 205, 219, 1932.
[11] J. B. Sumner, J. S. Kirk and S. F. Howell, *Jour. Biol. Chem.*, 98, 543, 1932.
[12] F. Kubowitz and E. Haas, *Biochem. Zeit.*, 257, 337, 1933.
[13] J. B. Sumner, *Jour. Biol. Chem.*, 69, 435, 1926.