CORRESPONDENCE

Voltes Face

SIR,—In a recent issue of *Nature* (217, 903; 1968), your Molecular Biology Correspondent is kind enough to mention a paper recently published by Ullmann, Goldberg, Perrin and Monod (*Biochemistry*, 7, 261; 1968). He congratulates us on having "rediscovered on this side of the Atlantic" the widely used method of studying the sub-unit structure of proteins in guanidine hydrochloride solutions.

May I point out that we have neither discovered nor rediscovered anything? Our modest work simply amounts to an empirical determination of the partial specific volume to be used in the Svedberg equation for polypeptide chains in 6 M guanidine solutions. Your correspondent is apparently unaware of the fact that, in the absence of proper data concerning the partial specific volume of a molecule, it is impossible to obtain its molecular weight by centrifugation (performed on either side of the Atlantic). For instance, Marler and Tanford (*J. Mol. Biol.*, 239, 4217; 1964) estimated the molecular weight of the glutamic dehydrogenase protomer to lie between 44,000 and 53,000 "depending on the value assumed for $\bar{v}$".

May I also point out that in the same note your correspondent makes two other errors:

1. He apparently thinks that Weber's recent results contradict and correct the interpretation proposed by Changeux, Gerhart and Schachman, and by Gerhart and Schachman, of the sub-unit structure of aspartate transcarbamylase. Actually, Weber's results very clearly and strongly confirm the previous authors' interpretation of this enzyme structure as being made up of four protomers, each containing two different sub-units.

2. From Weber's results, your correspondent concludes that aspartate transcarbamylase is an "octamer" when, of course, it should be considered as a tetramer, for which Changeux, Gerhart and Schachman have proposed a rather appealing schematic structure. This structure has already found its way into elementary textbooks of biochemistry which your correspondent might study profitably (see *Structure and Function of Enzymes*, by Sydney Bernhard, edit. by W. A. Benjamin, 1968).

Yours faithfully,

JACQUES MONOD

Institut Pasteur,
25 rue du Docteur Roux,
Paris.

OUR Molecular Biology Correspondent writes: I apologize if I have credited Professor Monod and his collaborators with the wrong rediscovery: I am bound to agree that the rediscovery of the apparent partial specific volume is of almost comparable magnitude. Professor Monod describes his contribution as modest, and I am quite content to let his own evaluation stand as the last word. I would merely add that the most searching analysis of the relation between partial specific volume and apparent partial specific volume in mixed solvent systems is to be found in the paper by Hade and Tanford (*J. Amer. Chem. Soc.*, 89, 5034; 1967). (The seeming confusion in Professor Monod's letter between these two functions, and indeed between sedimentation equilibrium and the Svedberg equation, I take to reflect only a double slip of the pen.) If Professor Monod cares to consult this article, he will discover there also the basis of the empirical equation given in his paper.

I do not wish to dwell on Professor Monod's second point, because I hope still to retain the freedom to discuss molecular weights of proteins without being drawn into the quicksands of allosterism. My only comment is that he has apparently read both my mind and my article incorrectly. It was not concerned with allosteric theorizing, only with the number of sub-units in the enzyme, which Changeux, Gerhart and Schachman stated to be six (four regulatory and two catalytic), and Weber found to be eight (four of each kind).

The last point is of a subtlety which is, I fear, beyond my grasp. The breathtaking assertion that the octamer should "of course" be regarded as a tetramer leaves me defenceless. To the unprepared mind an octamer is an octamer, and the species which Professor Monod observes in his guanidine hydrochloride solutions are monomers, rather than hemimers or even demihemimers. My perusal of the elementary biochemistry textbooks which Professor Monod recommends has not so far led me to an explanation of his dialectic. Is it, I wonder, Professor Monod's real purpose to acquaint us with a new definition of a monomer—or monodmer perhaps—elusive, but adaptable?

Urban Pollution

SIR,—The yellow lines denoting parking restrictions in our cities contain a chromate pigment, as can be shown by simple qualitative tests. According to Browning[1], hexavalent chromium in dust is a cause of dermatitis, ulceration of the skin, inflammation of the nasal mucosa and larynx and of lung cancer. It is obvious that the rate of pulverization of the material used for yellow lines is rapid in conditions of heavy traffic. The purpose of this note is to draw attention to this new source of pollution of the urban atmosphere, and to hope that some less harmful pigment can be substituted for chromate.

Yours faithfully,

H. J. M. BOWEN

Department of Chemistry,
The University,
Whiteknights Park,
Reading.

[1] Browning, E., in *Toxicity of Industrial Metals* (Butterworths, 1961).

ERRATUM. The following changes should be made in the article "Markovian Models of Dialogic Time Patterns", by J. Jaffe, S. Feldstein and L. Cassotta (*Nature*, **216**, 93; 1968): the expression $1-q_0$ in the first row, first column of the matrix in equation (2) should be enclosed within parentheses. The last line of the second paragraph following equation (2) reads: . . . estimates for the q_1 and r_1 are . . . This should read: . . . estimates for the q_i and r_i are . . . The equation following should therefore read: $q_i = p_{i1} + p_{i3}$ and $r_i = p_{i2} + p_{i3}$ $(i = 0, 1, 2, 3)$.

ERRATUM. In the article "Control of Growth in Two Populations of Elephant Seals", by M. M. Bryden (*Nature*, **217**, 1106; 1968), reference 4, Bryden, M. M., *Austral. J. Biol. Sci.* (in the press), should read Bryden, M. M., thesis, Univ. Sydney.

ERRATUM. In the communication "Erythropoietin Enhancing Factor in Serum of a Calf with Primary Familial Polycythaemia", by D. Van Dyke, M. L. Nohr and B. Tennant (*Nature*, **217**, 1027; 1968), it was not made clear that the incubation of erythropoietin with renal erythropoietic factor was carried out by Dr E. Zanjani.

ERRATUM. In the note "Where to put it" (*Nature*, **217** 1199; 1968) describing the CERN site evaluation panel report, the overall ratings of the Doberdò and Le Luc sites were reversed. In fact, Le Luc, not Doberdò, is the most suitable site.