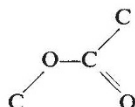


CORRESPONDENCE

Molecular Models

SIR,—Atomic model building (or, as some might say, guessing) is a powerful tool in determining the structures of molecules. In the cases of the α -helix and DNA, for example, the structures would not have been proposed without the aid of accurate model building. In both cases, however, the model building was backed up by sound stereochemical knowledge. We would like to draw your attention to the most important stereochemical error that occurs in the recent article in which W. H. Beers and E. Reich use model building to relate the structure of acetylcholine to its activity as a neurotransmitter¹.

All five atoms of an ester group



are coplanar due to the partial double bond character of the O—C bond (ref. 2). The group has a resonance energy of about 24 kcal per mole (ref. 2) and this energy must be added for any large rotation to take place about the O—C bond.

Beers and Reich do not describe their model for acetylcholine in the text of the article, but inspection of Figs. 2 to 6 shows the molecule, $(\text{CH}_3)_3\text{N}^+\text{CH}_2\text{CH}_2\text{OCOCH}_3$, with a rotation about the O—C bond of approximately 90° from the coplanar conformation. Such a conformation, at 24 kcal per mole above the ground state, would occur for less than one molecule in 10^{15} . As less than 10^8 molecules are released for the transmission of one nerve impulse³ it seems doubtful that this model of acetylcholine is relevant to the molecule's role as a neurotransmitter.

Yours faithfully,

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PETER PAULING

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University College London,
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¹ Beers, W. H., and Reich, E., *Nature*, **228**, 917 (1970).

² Pauling, L., *The Nature of the Chemical Bond*, 3rd ed. (Cornell University Press, Ithaca, 1960).

³ Katz, B., *Nerve, Synapse and Muscle*, 120 (McGraw-Hill, London, 1966).

Electrical Plant

SIR,—May I comment on the statements concerning the unreliability and breakdown of 500 MW and 660 MW generating units and the way the Central Electricity Generating Board manages its affairs (*Nature*, **228**, 1126 and 1246; 1970).

The Select Committee on Science and Technology (HMSO, 1970) concluded that there were welding defects and hair-line cracks in the boilers. The CEGB states¹ that "the problem in this country is accentuated by the high proportion of new plant on the system".

Could it be that too much attention has been given to designing plant which will operate at an excessively high steam temperature without an adequate regard for plant reliability?

In the United States there are 25 units² under construction of between 500 MW and 1,380 MW with a maximum steam temperature of $1,010^\circ\text{F}$ which is some 40°F below the CEGB units which are under construction.

Yours faithfully,

F. H. DENNIS

12 Cintra Park,
London SE19

¹ CEGB Annual Report and Accounts, 1969-70, 3 (1970).

² *Electrical World*, **174**, No. 8, 35 (1970).

Acanthaster in the Indian Ocean

SIR,—Having dived off the east coast of Africa since 1959, we very rarely saw *Acanthaster planci* (crown of thorns starfish) until 1968. One specimen was seen in 1961 at Bazaruto Island.

In January 1968 we spent some time in the Bay of Nacala ($14^\circ 25' \text{S}$, $40^\circ 40' \text{E}$), northern Mozambique. This is being developed as a harbour but we saw no evidence of blasting or dredging¹. Large areas of the reef were dead or apparently dying. In retrospect we have thought that this was probably attributable to *A. planci*² of which we saw and photographed a great number. At the time we were unaware of the threat that this starfish was presenting in the Pacific Ocean¹⁻³.

The following year, while visiting several islands in the Indian Ocean, we saw vast areas of devastated coral and many specimens of *A. planci* at Juan de Novo (17°S , 42.5°E). This is a small, totally isolated island with a Melanesian

population of about fifteen and with fewer than this number of visitors in the last two years. A passageway through the reef had been blasted on the leeward side of the island to allow small boats through the coral to load phosphates mined on the island. This activity had stopped about three years previously.

We are happy to report that we saw no evidence of *A. planci* or coral damage at either Mayotte in the Comores or at Aldabra.

In December 1969 we saw about ten specimens in six half-hour¹⁻⁴ dives at Bazaruto Island (22°S , 36°E). Some of these were seen at Punto Dundo, a rocky outcrop with very little coral, between Bazaruto and Benghueria. At this point the tidal current reaches 5-7 knots. Others we saw at Gengareme. This is a sheltered bay on the lee of the island where there are mainly large brain corals (lobophyllia) isolated by sand. Shortly after this, another expedition reported collecting sixty³ specimens in one dive from the reef two miles NNE of Punto Dundo. This reef has abundant and varied coral growth, and is subjected to perpetual breaking water. There was no evidence of coral damage in any of these three areas.

Another threat to the life on coral reefs is becoming more and more evident in Northern Mozambique. The local fishermen have learnt to use goggles and spearguns and indiscriminately shoot all coral fish over about three inches long. This results in a dearth of fish above a depth of about thirty-five feet. The fish deeper than this are very shy. We feel that concerted effort is required to protect life on coral reefs, such as at Watamu, Kenya and Aldabra.

Yours faithfully,

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N. BLYTH

PO Box 809,
Johannesburg,
South Africa

¹ Cheser, R. H., *Science*, **165**, 280 (1969).

² Australian Academy of Science, Report No. 11 (1970).

³ Dixon, B., *New Scientist*, Oct. 30, 226 (1969).

⁴ Vine, P. J., *Nature*, **228**, 341 (1970).

Nonsense Fragments

SIR,—The December 19 issue of *Nature* contains two articles and an editorial review¹⁻³ concerned with the fate of fragments of proteins produced by *E. coli* nonsense mutants. The authors claim

to have demonstrated the existence of a mechanism, presumably enzymatic, causing the rapid degradation of fragments *in vivo*, and discuss at some length the physiological implications of this finding. Neither the authors nor the reviewer consider an alternative explanation, that the fragments might have been released from the bacteria into the medium and in this way have escaped detection by the methods used. There is in fact some evidence for this explanation, from earlier experiments with alkaline phosphatase nonsense mutants of *E. coli*; phosphatase fragments were found to be rapidly released into the medium, an average time of about 20 minutes being required for a fragment to appear in the medium after being synthesized intracellularly^{4,5}. The behaviour of phosphatase fragments could represent a special case, because the phosphatase enzyme in a standard strain of *E. coli* is localized in the region between the outer cell wall and the inner membrane (the periplasmic space); consequently phosphatase fragments might escape from a cell more readily than fragments of other proteins. Nevertheless, in the absence of evidence to the contrary, the intracellular disappearance of fragments of any protein might result, not from degradation, but rather from the passage of the fragments into the surrounding medium. Until this possibility has been tested, the conclusion that *E. coli* protein fragments are degraded *in vivo* must be regarded as speculative.

Yours faithfully,

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and Biochemistry,
Yale University,
New Haven, Connecticut

¹ Goldschmidt, R., *Nature*, **228**, 1151 (1970).

² Platt, T., Miller, J. H., and Weber, K., *Nature*, **228**, 1154 (1970).

³ *Nature*, **228**, 1137 (1970).

⁴ Suzuki, T., and Garen, A., *J. Mol. Biol.*, **45**, 549 (1969).

⁵ Natori, S., and Garen, A., *J. Mol. Biol.*, **49**, 577 (1970). Reference to the loss of phosphatase fragments from cells is specifically made on page 587 of the second article, and the experimental details have been submitted for publication in *J. Mol. Biol.*

This letter has been shown to our correspondent, who replies:

The idea that cells might extrude unwanted nonsense fragments is appealing, but seems unlikely to apply for β -galactosidase and lactose repressor mutants. Neither of these proteins is located in the periplasmic space; this must imply that a special (enzyme) system would be needed to transport the nonsense fragments out of the cell. This seems more complicated than merely degrading the fragments, and would in any case demand the type of recognition mechanism which the authors discussed to discriminate mutant from normal proteins. An extrusion mechanism does not, therefore, seem to possess any theoretical advantage, and experimentally Goldschmidt was able to

detect and identify a protein fragment which is probably produced by the postulated endopeptidase degradation of the β -galactosidase nonsense fragment.

Neuromythology?

SIR,—Is the large daily loss of neurones from the brain, which figures in all the textbooks and provides the basis for Dawkins's¹ ingenious model of memory, as well established as he suggests? I write because the matter, which was the subject of a fair amount of controversy at a recent American research course on ageing, is of considerable importance for models of psychological and psychiatric ageing as well.

Leaving aside insect work, the prime authority for a massive loss is Brodie², whom Dawkins quotes, though the original mammalian claim was made by Hatai³ for the rat, and repeated by Vogt and Vogt⁴. The loss of Purkinje cells from rat brain found by Inukai⁵ has not been discovered in ageing hamsters⁶. With regard to the cortex, which would appear to be the critical site for Dawkins's purpose, some loss is reported⁷ but in the rat it appears to be trifling⁸. The most recent title indicates no loss whatever with age from the human cochlear nucleus⁹.

In view of the importance of the subject and the categorical nature of the textbook statement, the matter of cell loss with age among fixed postmitotics is due for careful re-examination, in spite of the tiresome task of cell counting which it involves. It would be of interest to know whether studies of this kind in mammals are already in progress.

Yours faithfully,

ALEX COMFORT

Director of Research, Gerontology,
University College London

¹ Dawkins, R., *Nature*, **229**, 118 (1971).

² Brodie, H., *J. Comp. Neurol.*, **102**, 511 (1955).

³ Hatai, S., *J. Comp. Neurol.*, **12**, 107 (1902).

⁴ Vogt, C., and Vogt, O., *Nature*, **158**, 304 (1946).

⁵ Inukai, T., *J. Compar. Neurol.*, **45**, 1 (1928).

⁶ Wilcox, H. H., *J. Gerontol.*, **11**, 442 (1956).

⁷ Wright, E. A., and Spink, J. M., *Gerontologia*, **3**, 277 (1959).

⁸ Brizzee, K. R., Sherwood, N., and Timiras, P. S., *J. Gerontol.*, **23**, 289 (1968).

⁹ Konigsmark, B. W., and Murphy, E. A., *Nature*, **228**, 1355 (1970).

ESRO Satellite

SIR,—It was not our intention to imply that a satellite needs to be stabilized to the same accuracy with which one desires to locate the X-ray sources. There is, however, a relation between the two as illustrated by the example of Sco X-1 given by Dr Gursky¹. As he correctly states, the requirement is to know, with high precision, the attitude, at the same time maintaining a stability commensurate with the field of view of the

instrument. For the HEAO mission to which Dr Gursky refers (that carrying modulation collimators, not the large X-ray telescope planned for a later mission) he claims a limiting accuracy in location of several arc seconds.

If this can be achieved, the two missions would give complementary results in the location of sources since the occultation satellite, although in principle capable of higher precision, locates a smaller fraction of the observed sources. On the other hand, an important characteristic of the occultation method is that the angular dimensions and relative structure of extended sources can be obtained with even higher accuracy than that of location, down to small fractions of an arc second.

We should finally note that the terms "complex" and "expensive" are, of course, relative rather than absolute, and should be read in the context of the ESRO budget which does not at present envisage satellites of the HEAO type.

Yours faithfully,

J. ORTNER

The ESRO Mission Definition Group
for the Highly Eccentric X-ray
Astronomy Lunar Occultation Satellite

¹ Gursky S., H., *Nature*, **228**, 1121 (1970).

Neural Nomenclature

SIR,—In 1933, Dale¹ suggested the terms "adrenergic" and "cholinergic" for the two types of autonomic nerve fibre known at that time. He introduced these terms "to assist clear thinking, without committing us to precise chemical identifications, which may be long in coming".

In the early 1960s powerful nerves were found to supply that gut which were neither cholinergic nor adrenergic²⁻⁴. Since then, there have been a number of reports concerning details of their distribution, structure and function⁵⁻⁷, but their description as non-adrenergic, non-cholinergic nerves is clumsy and somewhat negative. Evidence has recently been presented that the transmitter substance released from these nerves may be ATP or some related purine nucleotide⁸. It would therefore seem suitable, for the same reasons put forward by Dale in 1933, to propose that the new nerves be termed "purinergic".

Yours faithfully,

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University of Melbourne, Parkville, 3052,
Victoria, Australia

¹ Dale, H. H., *J. Physiol.*, **80**, 10 (1933)

² Burnstock, G., Campbell, G., Bennett, M., and Holman, M. E., *Nature*, **200**, 581 (1963).

³ Burnstock, G., Campbell, G., Bennett, M., and Holman, M. E., *Intern. J. Neuropharmacol.*, **3**, 163 (1964).

⁴ Burnstock, G., Campbell, G., and Rand, M. J., *J. Physiol.*, **182**, 504 (1966).