

elementary physics. It is impossible to regard as rational understanding a mental condition involving simultaneous acceptance of directly contradictory ideas. Yet this is what is exhibited by very many of our university science entrants, and also by some of those who write the sixth-form textbooks. Thus a student may be shown by a simple test to be acquainted with a particular concept, but it may well be that his ideas are so confused that the concept is useless to him.

Yours faithfully,  
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### The Future of Botany

SIR,—We have recently gone through the process of appointing a lecturer to this department. In advertising the post, we made clear that although we were interested in candidates from a broad field there were certain specific requirements, possibly somewhat out of the ordinary. We had a number of applicants for the post and were very surprised to find that a good majority of these made no effort to indicate how they might fit in with our requirements. Furthermore, a good number of the candidates who were either just finishing, or had just finished, their PhD work seemed to think that their only mission in life was to further their postgraduate research and seemed in no way interested or anxious to broaden their interest in botany.

We found this all very disturbing and indeed it seems to us to echo some of the problems which have been discussed about the training of biologists in this country, particularly those made by the Royal Society Committee on Postgraduate Training in Biology and in the Swann Report. The feeling is that the training of PhDs in biological subjects is leading far too frequently to an inflexibility of outlook and a narrow-mindedness which is inherently bad for the progress of the subject.

The situation is becoming critical. Something needs to be done to discourage departments, and those within them who are responsible for training postgraduate students, from allowing these students to have their interests focused on very specialized problems to the neglect of their general biological education. Specialization of work may be a necessary part of achieving a proper scientific training, but if it leads to a narrow outlook then it is very bad training.

Yours faithfully,  
D. H. JENNINGS  
A. D. BRADSHAW

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University of Liverpool.

### Simplified Notation for Peptides in Computer Compatible Format

SIR,—Without wishing to comment one way or another on the proposals advanced by Revesz<sup>1</sup>, it should be pointed out that the IUPAC-IUB tentative rules *A One-Letter Notation for Amino Acid Sequences*<sup>2</sup> do not contain the letters J, O, U (which she allocates to the also not included Cit, Orn, CyS-SCy) and allocates X to "unknown or other", and not to HyPro. These differences from the IUPAC-IUB tentative rules are presented as if they were part of the latter, in which respect the article is misleading.

Yours faithfully,  
O. HOFFMANN-OSTENHOF

IUPAC-IUB Combined Commission  
on Biochemical Nomenclature.

<sup>1</sup> Revesz, G. S., *Nature*, **219**, 1113 (1968).  
<sup>2</sup> *Arch. Biochem. Biophys.*, **125** (3), 1 (1968); *Biochemistry*, **7**, 2703 (1968); *Biochim. Biophys. Acta*, **168**, 6 (1968); *Europ. J. Biochem.*, **5**, 151 (1968); *J. Biol. Chem.*, **243**, 3551 (1968).

SIR,—In answer to Dr O. Hoffmann-Ostenhof's letter I would like to point out that the changes made by *Index Chemicus* in IUPAC nomenclature were limited to the use of unused or undefined letters, such as J, O, U and K. To the extent that these changes were not pointed out clearly, there may have been room for misunderstanding.

My article (*Nature*, **219**, 1113; 1968) clearly stated, however, that IC-IUPAC notation is considered an open-ended system to which additions can be made as the need arises for effective computer storage and searching of long chain peptides. Indeed, the following additions have already been made:

- A Cysteic acid
- B  $\beta$ -Aminobutyric acid
- C Homoserine
- D Pyroglutamic acid
- E  $\beta$ -Aminovaleric acid
- F Sarcosine
- G  $\beta$ -Alanine

The addition of these symbols, together with twenty other symbols for substituents, was prompted by their actual occurrence in the peptide literature which the *Index Chemicus Registry System* (ICRS) stores on computer tape. A significant and increasing percentage of the over 150,000 compounds encoded by ICRS each year involve peptides. To deal with such a large amount of new information, the IUPAC IUB system must be open-ended.

Yours faithfully,  
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### SI Units and Dimensionless Quantities

SIR,—It is probably not generally realized that, because the electromagnetic SI units are associated with a rationalized system (Giorgi), the values of some dimensionless quantities, such as electric and magnetic susceptibilities, are  $4\pi$  times greater than in the (unrationalized) cgs systems. There is thus a real danger of errors in interpretation if it is wrongly assumed that for dimensionless quantities the units system adopted is irrelevant. So long as rationalized and unrationalized systems coexist it is important that in reporting such quantities the system used should be stated.

Yours faithfully,  
J. K. BECCONSALL

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### Policies for Pollution and its Cure

SIR,—Your article on "Policies for Pollution and its Cure" (*Nature*, **222**, 1013; 1969) is a timely reminder of the ineffectiveness of international legislation in controlling oil pollution. Oil pollution is, however, only one aspect of coastal pollution, but one which has great emotional impact. The use of the sea for the disposal of unwanted industrial waste material is an increasing threat to sea fisheries. Yet in Britain pollution of sea fisheries is controlled by the Sea Fisheries Act of 1888. In order to stop polluting discharges, the Sea Fisheries Officers have to "prove that pollution has occurred". This is, of course, exceedingly difficult to do because long term natural fluctuations in marine communities have been little studied. Thus the effect of the effluent can be put down to "natural fluctuations" in the community or species decimated.

Clearly Britain cannot alone strengthen her laws on effluent disposal and require home industries to control discharges if overseas competitors are not required to do

so. What is needed is effective international legislation to control effluent discharges.

The Swedish legislation which you cite has been introduced because of the chronic pollution of some Swedish lakes and rivers and, indeed, of the Baltic. The pollution situation in the Baltic, where water takes thirty years to move from Stockholm through to the Skagerrak, is far more acute than the situation in the North Sea. The public awareness of environmental pollution in Sweden is, however, impressive. Newspaper articles and radio and television programmes have focused on the problem and indeed the Scandinavian Pavilion at the 1970 Osaka World Fair has a theme of "Environment Protection".

In addition to the banning of DDT, aldrin, dieldrin and lindane, the Swedish Nature Conservancy Office has a £20 million budget for a five-year period from 1969-70 to subsidize the installation of equipment to control pollution. A special board has also been set up to examine individual applications for effluent disposal systems, and to recommend effective waste-treatment systems. Industrial and municipal companies will have to obtain a pre-establishing franchise from the board. The board may prohibit construction of plants that would have a detrimental effect on the environment. It may also prohibit continued activity where anti-pollution measures have proved insufficient.

Such overall control of environmental pollution by a single board (as outlined in your article) is desirable in an increasingly industrialized society such as ours.

Yours faithfully,

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Wellcome Marine Laboratory,  
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Yorkshire.

### Molecular Biology in the UK

SIR,—In *Nature's* on the whole admirable survey of science policy in Europe (222, 845; 1969), I was astonished to see that it opened its remarks about the United Kingdom with the statement that "the boom continues . . . in molecular biology". What on earth is *Nature* referring to? I should have thought that one of the salient features in recent British biology had been the almost total stagnation of significant new funding for this subject, apart from a few dribs and drabs to support outfits consisting of one man and a boy. Of course, the great well established MRC laboratories at Cambridge and King's College London have made some progress; but there has not been any new addition to the only two other groups—the departments of molecular biology and genetics, both at Edinburgh and both relying mainly on MRC finance—which pass the minimal tests for activities which can rank as significant expressions of policy. The most important test is a super-critical number of independent scientific staff (above eight, I should say), deployed over a wide range of related topics, and if possible intimately involved in university teaching. So far as I know, no one is even seriously working towards the establishment of any new centre of this kind. Frankly, I am not surprised, because, having done the job myself, I don't see why anyone should be mug enough to expend the necessary time, energy and fortitude against the "slings and arrows".

The difficulties do not arise from the policy-making administrators in the research councils, but from the passive resistance of more classical biologists. I started seriously trying to set up a laboratory of molecular developmental biology in 1952. The idea was enthusiastically accepted by the MRC administrators; but when they came to refer it to an "expert" biology committee, it was turned down flat, much to their surprise. The plan was

revived again some years later, and this time the MRC took more care to steer it through hoops, so that it actually got authorized in 1962. Even then, they could not build anywhere to put it; but they gave me what help they could to raise elsewhere the money for building. Since then, far from booming, the financial resources have remained almost static; there has been a slight rise for under-estimated devaluation, but the impact of the sophistication factor has resulted in actual contraction in the number of scientists on the staff.

The basic difficulty is very simply identified. Molecular biology as a whole is more expensive than classical experimental biology; and this applies with special force to studies on the differentiation of eukaryotic systems, where one is trying to put tabs on the activity of small fractions of the total genome. The experienced molecular biologists who drew up the budget for the projected EMBO laboratory put a round figure of \$4,000 per annum, per worker (excluding graduate students), for consumables, and the Kendrew Report<sup>1</sup> states that in three out of four established British laboratories the figure is between £1,000 and £1,200. But there is no experienced molecular biologist on any research council in Britain, and I think only one on the next level of advisory committees which do the actual vetting of proposals. The classical experimentalists and biochemists of these committees accept about \$1,000 as the standard level of expenditure on consumables. The result can be seen in Kendrew's Report that much of the work "is at best dull and at worst trivial".

It is for these reasons that I have at last come round to whole-hearted support for what *Nature's* review has to say about EMBO: an international laboratory is needed (222, 836; 1969). I was responsible for first suggesting that EMBO should get started by a programme of fellowships, training courses, seminars and so on; and I have been no more than lukewarm about the establishment of a central laboratory, on the grounds that the harm this would do to universities by draining away their best people would not certainly be compensated by any advantages. The experience of the last three or four years has convinced me that significant decentralized advance is not actually a practical proposition in the present set-up in Britain. A central European laboratory (or perhaps a small number of regional laboratories) will in the short run almost certainly damage existing laboratories and university departments, but I now see it as the only practical way forward to adequate developments in the long term.

Yours faithfully,

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<sup>1</sup> Council for Scientific Policy, *Rep. Working Group on Molecular Biology*, July 1968, Cmnd 3675 (HMSO, London, 1968).

### Isaac Newton Telescope

SIR,—An editorial in *Nature* (222, 211; 1969) is critical of the management of the Isaac Newton reflector. We do not wish to comment on that, which is purely a national matter, but we cannot ignore the unrealistic description given by your anonymous editorial writer on the way successful observational astronomy actually proceeds. You stress that "expert and highly paid astronomers are taken away from their jobs to operate equipment which can be run equally well by proficient technicians". Although some routine observing is necessary in any collection of data, it is the experience of highly trained astronomers, themselves working at the telescope, which turns otherwise routine programmes into major advances of a fundamental kind. It is the insight of observers who are in nightly contact with the subject which carries the work above mere data collection. Do you imagine that