LETTER TO THE EDITOR

The nature of Biochemistry

At the beginning of this century, progress in chemistry, especially in organic analysis, enabled some of those who wanted to find out how organisms worked, to establish biochemistry as a science. As with most divisions between adjacent sciences, the distinction between biochemistry and physiology was arbitrary. Hopkins was the most influential exponent of the new science. He succeeded in banishing purely verbal 'explanations' (e.g. inogen, protoplasm and *vis insita*) of phenomena; he replaced them by the study of definable enzyme actions between identifiable molecules. As a result, most of the work in his laboratory in Cambridge was on enzymes. There was no general agreement until the 1930s that enzymes were proteins, but proteins dominated biochemical thinking because of their presence in all the organisms that had been studied. There is a grain of truth in Engels' dictum 'Life is the mode of existence of protein molecules', and it was reasonable for proteins to be the central theme at a symposium on the origins of biochemistry.

The reviewer of 'The origins of modern biochemistry: a retrospect on proteins'[1] seems unaware of these points. The review accuses biochemists of lacking interest in 3-dimensional protein structure. Until the mid-30s there was no technique for studying that structure. Biochemists, in Cambridge at any rate, ridiculed attempts to guess structures—we compared them to the notions Hopkins had banished. Guesses for which there is no experimental evidence are sterile. Thus Medicus' correct guess at the structure of uric acid did not advance understanding of purines: that depended on Fischer's evidence.

As soon as X-ray crystallography had progressed beyond the level of showing that there was structure in such things as hair and silk, we pestered crystallographers for structural information. First with vitamins and hormones, then with proteins. For example: I took crystals of glutathione (then as now thought of as an important biochemical intermediate) to my friend Bernal in 1932, and tobacco mosaic virus in the liquid-crystalline state four years later.

Every science has fashions. I have commented elsewhere[2] on the fickle way in which attempts were made to give exclusive roles to certain types of molecule as the vehicles for specificity—first carbohydrates, then proteins, now nucleic acids. This process will probably continue. Nature seems opportunistically to use whatever is available. It is reasonable to predict that, as soon as physicists devise techniques for studying the conformation of lipids *in vivo*, there will be a flurry of excitement about 'informational lipids' which will equal if not surpass today's nucleic acid flurry.

Biochemistry is concerned with the behaviour of every type of molecule which occurs in, or is introduced into, organisms. The reviewer was right in commenting adversely on the slow recognition by some biochemists of the roles of nucleic acids. For 40 years I have ridiculed those who wrote of 'virus protein' when all that was meant was 'virus'. That solecism still mars one article in the symposium. In 1936, Bawden & I[3], after finding that potato virus X probably contained protein, wrote '— —there is no evidence that other equally important substances may not also be present.' Later[4], although we wrote '— —it may well be that the study of virus multiplication will shed more light on protein synthesis than *vice versa*.' We argued that it would be as reasonable to discuss virus multiplication as an example of nucleic acid, as of protein, synthesis. However, the unfavourable light in which the reviewer sees biochemistry, as opposed to molecular biology, overlooks the point that biochemistry is concerned with *all* that is going on in organisms. No one disputes the importance of nucleic acids, nor the importance of systems which transmit information. What thoughtful biochemists dispute is the overriding importance of these aspects of the subject. It is as if what purported to be the description of a country, amounted to no more than a description of how its newspapers were printed.

There is a basic difference in objective: there is less difference than the reviewer seems to think between what is actually done by many of those who work in biochemical or molecular biological laboratories. Instruments for measuring radioactive tracers twinkle as merrily in one as the other, and the published papers contain similar photographs of the bands into which molecules are separated by electrophoresis.

These somewhat personal comments on the inadequacies of the review are not prompted by pique. The comments on my own paper were sympathetic. Historians have to depend on what is written. I have tried to give an impression of the outlook of one of the more influential departments of biochemistry. We discussed endlessly such themes as the nature of viruses and genes: were the former just the latter delocalised as Muller suggested? Haldane was our teacher, and we met Garrod who was an old friend of Hopkins, so we knew the one-gene-one-enzyme idea and wondered whether genes were enzymes. But we disseminated our musings in pubs—not publications.

42 Leyton Rd. Harpenden, Herts. N. W. Pirie

[1] Abir-Am Pnina, 'Modern Biochemistry', *British Journal for the History of Science*, 1982, 15, 301–305.
[2] N. W. Pirie, 'Patterns of Assumption about Large Molecules', *Archives of Biochemistry and Biophysics*, *1962*, Suppl. 1, 21–29.
[3] F. C. Bawden & N. W. Pirie, 'Experiments on the Chemical Behaviour of Potato Virus "X" ', *British Journal of Experimental Pathology*, 1936, 17, 64–74.
[4] F. C. Bawden & N. W. Pirie, 'Virus Multiplication Considered as a Form of Protein Synthesis', in 'The Nature of Virus Multiplication' Eds. P. Fildes & W. E. van Heyningen, 1953, Cambridge University Press, 21–41.

https://doi.org/10.1017/S0007087400019816 Published online by Cambridge University Press