

CORRESPONDENCE

Help with Protein

SIR,—“How to help with protein” (*Nature*, 236, 1; 1972) is an excellent review of the protein problem, but it misses the important point, that perhaps protein deficiency is not the most serious problem after all, even for young children. The United Nations document “Feeding the expanding world population: international action to avert the impending protein crisis” (*UN Publication E. 68, xiii, 2*) appeared just at a time when many workers in the field of nutrition were beginning to question the almost exclusive concentration on protein which had characterized the previous decade. It is not accepted by all that kwashiorkor can be equated simply with protein deficiency; some believe that it represents a failure of adaptation to a generally poor diet, others that the precipitating factor is an infection. When energy rather than protein is the limiting factor in the diet, the end result is marasmus, which is less dramatic than kwashiorkor, and has therefore received less attention. Broadly speaking, kwashiorkor may perhaps be regarded as a disease of rural communities, marasmus of urban ones. With rapidly increasing urbanization in most developing countries, marasmus is becoming an ever more serious problem. Since it tends to occur at an earlier age than kwashiorkor, the permanent effects on development are more likely.

Your article was stimulated by the *Eighth Report of the Joint Expert Committee on Nutrition of FAO and WHO*, which it describes as disappointing. Certainly the report does not give a clear picture of the problem, because there are too many gaps in our knowledge. The report poses two questions: first, can one diagnose the nature and extent of the dietary deficiency from the clinical pattern protein-calorie malnutrition? Second, how can one assess the prevalence and type of protein-calorie malnutrition in any community? The answer to these two questions is essential for the proper planning of preventive measures. It may well turn out that concentration on the “protein gap”, and on the practical difficulties of what you describe as artificially fortified foods is leading us down a blind alley. It is necessary to keep an open mind.

In fact your article does this admirably in its closing paragraphs. “It follows that the mere provision of protein supplements is not sufficient and that developing countries . . . have no way of meeting the social need which confronts

them except by developing the kinds of child care services which can ensure that suitable foods are available at the critical times. . . . Such programmes . . . will have to deal not merely with protein supplements but with the health care of children in the round”. These words deserve to be quoted again and again because, as you rightly point out, it is useless to treat these problems in isolation.

Yours faithfully,

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Rothschild

SIR,—Winding up for the Government in the House of Lords debate on the Rothschild Report on February 29, Earl Jellicoe said (about the Research Councils vis-à-vis executive departments) that he saw “no reason why there should be a confrontation . . . between opposing forces” and that he thought he could predict with some confidence that we shall follow “the evolutionary path which I think many noble Lords have suggested we should tread on this particular matter”.

What evolutionary path can we tread? The best answer is to replace the blunderbuss of Table IV of Lord Rothschild's Report by inter-governmental machinery in which the research councils would be included.

All parties are agreed on the transfer to executive departments of the financial responsibility for most of the tactical science done by the research councils, once the departments have fully competent Chief Scientist organizations. This could in due course transfer funds of £7 or £8 million. The work involved can be fairly easily defined and identified, and the transfer would not raise problems of accountability or threaten the stability or integrity of the councils.

To go beyond transfers of this magnitude involves arguments about strategic and basic research where different but equally valid definitions give different answers. Moreover, the transfer to executive departments of funds with which the research councils finance basic and strategic research cannot but cut across their ability to discharge their primary responsibilities, which are to the Secretary of State for Education and Science.

In any case, the objective of organizations is that executive departments

should be able effectively to influence the programmes of the councils so as to take account of national responsibilities of the departments in the same or related fields of research. In that case, there are two steps on which all the parties are agreed—the creation of powerful Chief Scientists' organizations within departments and the granting of full council membership to the heads of those organizations. These two developments would go a long way to securing the main objective. True, such an arrangement would entail partnership and joint involvement, as opposed to the method of confrontation which would almost certainly result from giving departments the power of the purse, and good will and good sense by all would be required, but that is not a weakness—rather a necessity for any organization to run well.

But what will happen when a department and a research council disagree about a programme or series of programmes, or where a department considers that too much money is being allocated to the research councils as a whole? At present there is no machinery for catering for this kind of situation. The advice of the Council for Scientific Policy goes straight to the Secretary of State for Education and Science, who deals directly with the Treasury. There is no place, and no point in time, at which departments can quarrel with the amount of money being allocated to the DES vote. This is the one remaining gap in the machinery which must be closed.

So should not departments have the right to take major disagreements on research council programmes or financial allocations direct to the CSP? The CSP should make its own judgment of such a dispute in framing its advice to the Secretary of State, making clear in every case what the respective arguments are and why they have decided the one way or the other. It is to ensure that the CSP should not automatically rubber stamp the views of the research councils in such a situation that this institution, while wishing the CSP to remain advisory, has supported the proposal of the Dainton Working Party that the Chief Scientists of Departments should also be members of the CSP. The Cabinet would, of course, be the ultimate referee.

It is, surely, along these lines that a solution must evolve. Instead of arbitrarily wresting from the councils a large part of the finance they have already secured to discharge their responsibilities to the Secretary of State for Education and Science, machinery must be created

to provide a framework within which departments and research councils can work in partnership while allowing for the occasional major difference of view to be discussed and determined at every level.

Yours faithfully,

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Publication Speed

SIR,—I am pleased to see that Mr St Aubyn has, in responding to my letter, taken the opportunity of giving us some good advice on how to get our papers published (*Nature*, 235, 447; 1972). I am unsure about the sin that I and a number of other contributors have committed. I had always believed that while it was quite true that I could not eat my cake and have it I could certainly have it and eat it. I agree, I think, with the substance of every point that he makes in his letter, but disagree considerably about their importance.

Journals must, of course, have stringent refereeing procedures, but there is no need for this to take longer than, say, three months. Drs Rose-Innes and Lipman (*Physics Bulletin*; September 1971, 527–8) tell us that a good PhD thesis (longer than Mr St Aubyn's already excessive 8,000 words) takes about the pre-electricity time from Manchester to London to read and assess. All working scientists read research reports and critically evaluate them for their own purposes in very short periods of time. Three months seems a maximum for these decisions.

I would agree that there are likely to be some cases where the subject matter of submissions is so technical that a generalist editor cannot know whether it suits his journal or not. This surely cannot be the case on other than an insignificant number of occasions; surely it is this matter of relevance which is above all the responsibility of the editor.

Finally, I'm sure that the majority of readers of my original letter will know that I never suggested that authors should adopt the practice of multiple publication. I merely stated that in the case in which many journals are clearly so inefficient the editors should not be too surprised if

this device was used as a tactic to ensure speedy and full publication.

Yours faithfully,

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Knuckle Walking

SIR,—Russell Tuttle and Benjamin B. Beck state (*Nature*, 236, 34; 1972): "Man, too, often rests his flexed fingers in facultative knuckle-walking postures, as may be witnessed in certain public speakers and football linesmen". But surely the distinctive hand flexure of linesmen is due to the fact that they must hold a stop-watch in their non-flag-waving hand, the face of which must be easily visible? Is it different in Chicago? Does this affect the evolutionary inferences?

Yours faithfully,

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Obituary

Dr John Dennis Carthy

JOHN CARTHY, who died on March 13, was a man of many talents and much of the success and enjoyment which he achieved was brought about by the ease with which he combined them. His main scientific interests were in animal behaviour, but his knowledge and lively enthusiasm soon led him into activities where his ability to understand and communicate this work became appreciated by many. Those who met him found a warm and friendly personality which quickly made him a most approachable and rewarding colleague.

John Carthy was born in Plymouth in 1923 and educated at Bedford School and Christ's College, Cambridge, where he took the Natural Sciences Tripos in 1947. For the next three years he was engaged in postgraduate work under Professor W. H. Thorpe on problems of the orientation of ants and their use of odour trails. He was awarded the degree of PhD in 1950 and left Cambridge to join the academic staff of Queen Mary College, University of London, where he remained for the next seventeen years. During this period he extended his behavioural interests to include research on sensory physiology and cell ultrastructure. The

research published on these problems included investigations on the electrophysiology of the eye of the "whillig" beetle, the fine structure of the sensory pegs of scorpion pectines and field observations on the behaviour of a variety of arthropods. A number of successful postgraduates were trained under him at this time on topics as diverse as the significance of olfaction and vibration in termites to the social behaviour of the Red Avadaat. He was also involved in the establishment of a successful biological electron microscopy unit at Queen Mary College.

In 1952 John Carthy went on an expedition to the Azores and wrote a number of papers on the insect fauna of the isles of Pico and Fayal. An excellent entomologist and field worker, he blended these interests with his behavioural work so that he was eminently suited to be appointed in 1967 as the first Scientific Director of the Field Studies Council. In this capacity he travelled around the country a great deal, and soon became a familiar figure at the field stations, where he encouraged a whole range of research interests among the staff. He was particularly proud of the Oil Pollution Research Unit which was set up at Orierton and shared the concern for the environ-

ment expressed in the joint publication on the *Biological Effects of Oil Pollution on Littoral Communities*.

To many John Carthy will be remembered as a scientist who carried his subject to a wide audience by his radio and television appearances. Others will have read his many books, of which *Animal Navigation*, *An Introduction to the Behaviour of Invertebrates*, *The Behaviour of Arthropods*, *Animal Behaviour* and *The Study of Behaviour* are perhaps the best known for the clear and enjoyable style which appeared in these and later publications and which attracted a whole generation of biologists. The extent of the impact which these books had can be judged from the fact that they were translated into Spanish, Italian, Dutch, Swedish and German. These popular achievements were, however, based on the solid foundation of an understanding of scientific research and its implications and it was for the same reason that he was in heavy demand by the scientific community. He served as Secretary to the Society for Experimental Biology and the Association for the Study of Animal Behaviour and was a section secretary for the British Association for the Advancement of Science, of which he was also a Council member. He was