

EDITORIAL

Arguments for Acceptance

Recently I received a long and very vigorous letter from a group of authors whose paper had been rejected. They had clearly been stung by the comments in the Editorial Report, which they believed did not recognize the importance of their paper which they, quite naturally, thought was worthy of publication. I went back to the original paper and the Editorial Report and re-read them alongside the letter from the authors. As I did this I was immediately struck by the contrast between the arguments presented in the letter, of three pages, compared with the paper of many more. The letter contained, succinctly, the major reasons why the work described in the paper should be published. These were not evident in the paper, and this example raises some wider issues upon which I would like to expand.

Firstly, the authors in their letter gave a very clear and cogent reason why they had carried out the work. They argued that the evidence from the literature was conflicting in regard to the requirement for a particular nutrient and that they believed that it was important to resolve this issue. Few nutritionists, especially anyone who has been involved in debates on recommended dietary amounts, allowances, or whatever term one decides to use, would disagree with the importance to the nutritional sciences of understanding quantitative nutrient requirements.

The authors then went on to describe what seemed to them to be lacking from the existing work in the field: this was that the concepts regarding the factors that determine nutritional requirements had not been developed adequately. They proposed, in effect, a different 'model' of the determinants of requirements and argued that 'if this was true then the following should be observed' in a particular experiment. They therefore defined the hypothesis which they proposed to test. Purists of the Popperian school would argue that they should have devised an experiment to attempt to falsify their ideas: but I believe that one can, and should, attempt to analyse nutritional hypotheses in a theoretical, conceptual fashion because many central nutritional hypotheses cannot be tested experimentally for ethical or other reasons, but extensions along the lines 'if this is true then the following should be true' are amenable to experimental evaluation.

One of the reasons for rejecting the original paper was that the authors had used a particular statistical technique which the statistical editor did not think appropriate – a common experience for many authors I believe. In their letter, the authors argued why they had adopted this technique, and why they believed that it gave sounder estimates of requirements than those used by previous authors. In this they showed that they were aware of the need to defend the methods they had used and explain why their findings advanced nutritional knowledge.

Finally their paper was criticized as being 'confirmatory'. I am conscious that this word is often associated with the weasel word 'merely'; I believe that we need to rethink our views on confirmatory studies, recognizing how important confirmation of experimental findings is for the development of understanding. How often do we see in a review of a new or unexpected observation that this 'must await confirmation' before one attempts to alter one's views of a mechanism or relationship. In the case in question, the use of a different 'model' and experimental approach added significance to the findings in the paper.

This episode emphasizes the benefits of peer review in that the authors were forced by

the comments of their peers – which we should not forget means equals – to examine why they did the work and the hypothesis they set up to test, and to defend the validity of their findings. In developing arguments for acceptance I think that they will have improved their paper and shown how one can make the scientific literature convey the sense of challenge that scientific research involves.

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