

EDITORIAL

Comment from the Editor-in-Chief on correspondence in this issue on immuno-techniques

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Linked Articles

Visit <http://dx.doi.org/10.1111/j.1476-5381.2010.01139.x> to view the correspondence from Ashton and <http://dx.doi.org/10.1111/j.1476-5381.2010.01140.x> to view the reply from Atwood and Mackie. Visit <http://dx.doi.org/10.1111/j.1476-5381.2011.01262.x> to view the correspondence from Sandow and Grayson and <http://dx.doi.org/10.1111/j.1476-5381.2011.01284.x> to view the reply from Edwards *et al.*

In this issue of *BJP* we publish two letters commenting on the use of immunolabelling techniques. The first, from JC Ashton (Ashton, 2011) reinforces the need for strict criteria in assessing immunohistochemistry data as set out in a review by Atwood and Mackie (2010). The second, from SL Sandow and TH Grayson (Sandow and Grayson, 2011), is more concerned with Western Blots and comments on a paper by Weston *et al.* (2010). The latter paper was also the subject of an invited Commentary by Garland (2010).

Both pieces of correspondence concern the need for inclusion of rigorous control data when reporting experiments employing immunotechniques. Similar factors apply whether the technique in question is histological or biochemical and, where possible, the use of both approaches can reinforce the finding made by one of them.

The gist of the letter by Sandow and Grayson is that insufficient control data were included in a part of the study that reported Western Blots. After peer review of the letter and of the response by the authors, the scientific points raised were judged to be valid and, equally, the authors had most of the answers to hand. However, the fact that the answers were available does not mean that the points should not have been raised. It means that our system somehow failed to ensure that these details were available to the reader. Indeed, in responding to the comments, Edwards *et al.* (2011) point out that several other papers in the same issue could similarly be taken to task.

Whether the submitting authors, the peer reviews or the journal's rules for submission or review are responsible for the omission of these details is not really relevant. The editorial procedures employed by *BJP* are as comprehensive as those employed by other leading life-science journals, although that is no reason to be complacent. By including the data requested by Sandow and Grayson, the Weston *et al.* paper has become more rigorous – why was it not there to begin with? It was only this 'extra review layer' of post-publication comment to the Editor that produced the more complete study.

So we should take two lessons from this.

First we should be pleased that the availability of the Letter to the Editor mechanism has stimulated scientific debate and helped obtain a satisfactory outcome. Perhaps we should be thinking about reinforcing the armamentarium available for readers' comments, given the technology developed for social media.

Secondly, we should recognize that the application of immunological techniques to the detection and quantification of proteins in heterogeneous tissue carries many hazards that have been well highlighted elsewhere. One problem is that when antibodies are made available commercially there is often not as much data on quality control provided as should be expected, sometimes because it does not exist, although it should be emphasized that in the Weston *et al.* study the experiments were done, just not included in the original manuscript. Scientists employing these products are expected to ensure the quality of their own work although they might not be equipped to provide evidence for it themselves. Indeed, why should they when they have purchased a product in good faith from a recognized supplier? A recent useful series of articles in Naunyn-Schmiedeberg's Archives of Pharmacology appear to highlight this problem for a large number of commercially available antibodies for GPCRs (Michel *et al.*, 2009), pointing out that many of them could be shown to be non-selective when appropriate controls were carried out and making recommendations for minimum data required for publication of work using antibodies.

Clearly, we need to specify more clearly what information and what controls are required when authors publish work based on antibodies, whether this be for Western Blots or immunocytochemistry. Fortunately, we do not have to re-invent the wheel. In addition to Michel *et al.* (2009), excellent editorials on this topic have been published in the *Journal of Comparative Neurology* (Saper and Sawchenko, 2003; Saper, 2005).

We will be refining our instructions to authors in this regard.

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