

## CORRESPONDENCE

The 'golden rule' of Sharma that we should consider only 'like with like', is however, not followed by the journal and citation IFs themselves. By this rule the best journals in all scientific fields should have the same high IF; for example, if the best review journal for immunology, say the *Annual Reviews of Immunology* has an IF of 50, then the best plant science review journal or the best veterinary science review journal must also have the same impact factor of 50. In reality it is not done or is not feasible, and this is the reason why the IFs should not be given any undue importance as 'essential, primary or quality' indicators.

It is educative to refer to Seglen's article on why IFs are not a good measure<sup>2</sup>, as well as several arguments for or against the IFs and the several alternative means of calculating similar or related metrics<sup>6</sup>. Finally, Eugene Garfield<sup>7</sup> (who Sharma has cited heavily from) himself has warned about the 'misuse in evaluating individuals as well as journals' because there is 'a wide variation from article to article even within a single journal'.

*Note added in proof:* We recently came across an earlier paper (Boero, F., *Trends Ecol. Evol.*, 2001, **16**, 266) that has categorically shown how over-indulgence with the IF metric has actually harmed

the discipline of taxonomy. Since most of the taxonomy journals have very low or no Impact Factors, the taxonomists faced several hardships in performance appraisal as well as securing competitive research grants for their work. This factor has been recognized to be amongst the most important factors for the decline in taxonomy science in the USA.

1. Sharma, O. P., *Curr. Sci.*, 2007, **93**, 5.
2. Seglen, P. O., *Br. Med. J.*, 1997, **314**, 497.
3. Srinivasan, S. and Glover, I., *Curr. Sci.*, 2007, **93**, 35–40.
4. Kumar, N., *Indian J. Hist. Sci.*, 1999, **34**, 19–32.
5. Zong, Y. et al., *Nature*, 2007, **449**, 459–463.
6. [http://en.wikipedia.org/wiki/Impact\\_factors](http://en.wikipedia.org/wiki/Impact_factors); (Last accessed on 8 February 2008).
7. Garfield, E., *Unfallchirurg*, 1998, **101**, 413–414 (cited in: [http://en.wikipedia.org/wiki/Impact\\_factors](http://en.wikipedia.org/wiki/Impact_factors)); (Last accessed on 8 February 2008).

SHIRISH A. RANADE\*  
NIKHIL KUMAR

*National Botanical Research Institute,  
Lucknow 226 001, India  
\*e-mail: shirishranade@yahoo.com*

### Response:

I have described JIF as the primary quality indicator. This obviously implies

there would be a couple of other factors or opinions which ought to be taken into consideration. The limitations of JIF have been pointed out pretty often and must be taken into consideration. JIF is used along with citations which reflect the visibility of the publication to the peers. Certainly, some criteria other than the subjective assessments are needed to stem the 'publishing rot' all of us see around and careful use of JIF would be a step forward in that direction. In the absence of any quantitative parameters, the field remains wide open for manipulations and subjectivity. A comparison of 'like with like' means comparing the workers within a field and not normalizing the IFs within fields. In addition, researchers in different areas are pretty aware of which journals in their area of specialization have rigorous peer-reviews and command reputation. The examples cited by Ranade and Kumar are worthy of attention. I am happy that a positive discussion has been set in motion in order to adopt quantitative parameters for selection and appraisal, as well as minimize subjectivity.

OM P. SHARMA

*Indian Veterinary Research Institute,  
Regional Station,  
Palampur 176 061, India  
e-mail: omsharma53@yahoo.com*

## Peer review of manuscripts

Ranade and Kumar<sup>1</sup> have touched upon an important problem of the peer reviewing of manuscripts in scientific journals. To their discussion I would like to add the following. Campanario<sup>2</sup> has presented a discussion on rejecting papers that later are highly cited, an issue that certainly supports Ranade and Kumar's criticism of the peer review and shows its inadequacy. Interesting insights into the peer review process have also been given<sup>3,4</sup>. Several alternatives to replace this system that might cope with its drawbacks have been proposed<sup>5</sup>.

Ranade and Kumar<sup>1</sup> mention the double-blind review system in which both the authors and the reviewers are anonymous. However, in some research fields, for example agricultural sciences, the authors' anonymity is almost impossible to be kept: when an experiment is described and its details are given, it is no real

problem to figure out who the author is and from where he or she wrote the paper<sup>6</sup>.

As the double-blind review system has its own drawbacks, the main being the reviewers' anonymity, Ranade and Kumar<sup>1</sup> propose what we could call the reversed blind review system, in which authors are unknown to reviewers and reviewers are known to authors. I do not know of any journal that would apply such a system<sup>6</sup>, but maybe Ranade and Kumar's proposition will stimulate some journals to try this. Needless to say, this might be an efficient system and still it would be a peer review, not suffering from lack of professional evaluation of papers published. Still, the problem of authors' anonymity in chosen research fields remains unsolved. In other fields, though, it might work. Let us hope that, in the near future, there will be a journal

that will try the reversed blind review system.

1. Ranade, S. A. and Kumar, N., *Curr. Sci.*, 2007, **93**, 1659–1660.
2. Campanario, J. M., *Social Stud. Sci.*, 1993, **23**, 342–362.
3. Campanario, J. M., *Sci. Commun.*, 1998, **19**, 181–211.
4. Campanario, J. M., *Sci. Commun.*, 1998, **19**, 277–306.
5. Campanario, J. M., *The Sci.*, 1997, **11**, 9.
6. Kozak, M., *Eur. Sci. Edit.*, 2007, **33**, 56.

MARCIN KOZAK

*Department of Biometry and  
Bioinformatics,  
Warsaw University of Life Sciences,  
Nowoursynowska 159, 02-787,  
Warsaw, Poland  
e-mail: m.kozak@omega.sggw.waw.pl*