Expectations from a Ph D degree

We come across varied comments and observations regarding the Ph D degrees awarded by different universities. A Ph D has now become, so to speak, the minimum qualification for a number of academic positions. Earlier, it was considered to be an entry-level degree for a research job or for a university position. But now a Ph D is practically a must for a faculty position in a UG college, and teachers in schools also feel more comfortable with a Ph D degree. Thus Ph Ds are more visible in various academic institutions, nowadays.

A Ph D degree as an academic qualification has a unique feature. Advertisements for academic positions are particular about the marks/grades obtained by a candidate at the UG or PG level. Since no marks, class, division or grade is awarded for Ph D degree, apparently all Ph Ds are treated as equals. But, can we accept all Ph Ds to be equivalent? Well, our perception becomes the deciding factor in such cases. For example, if we find that a Ph D degree has been awarded by a university that is reasonably old and has already produced a good number of Ph Ds who are doing well, it draws more respect. Scientists/academicians working in the relevant field or in the same branch/discipline try to obtain information on the supervisor under whom the work has been carried out, or the name of the research institute where the work has been done. This helps them to mentally grade the degree. Another factor is the number of publications by a Ph D student and the journals where these have been published. Taking all these into consideration, we again tend to grade the Ph D work, though the subjectivity in the gradation cannot be denied. This is because one cannot just compare two Ph Ds on the basis of marks/grades as it is done for other degrees.

Is there a way of making the assessment of a Ph D degree less subjective? Well, some things can be tried out in this regard. A wider database of laboratories, units or groups may help one to put a Ph D in the right perspective. One can also find out what sort of course work a Ph D student has taken to identify his/her area of interest, and strong or weak academic aspects. However, if we accept the Ph D degree as one indicating that the awardees have undergone some academic training, have come to know where to look for information for a specialized field, and have developed the capability to look at things with a deeper insight, I think the degree will be judged per se and not by the name of the awarding university.

BHUPATI CHAKRABARTI

Department of Physics,
City College,
Kolkata 700 009, India
e-mail: bhupati2005@yahoo.co.in

Journal impact factors – essential primary quality indicators – surely not so!!

In a recent issue of *Current Science*, Sharma[1] describes journal impact factor (JIF) as an essential primary quality indicator. This is incorrect because inherent flaws in the determination of the JIFs preclude their use as a quality indicator. JIFs are neither 'essential' nor 'primary' indicators, nor do they reflect any specific 'quality'. The impact factor (IF) is a flawed metric because it does not use a comprehensive database, is centric to a smaller number of journals and the citation of papers is not a foolproof process[2]. The IF and citation of a paper are at best indicators of the 'visibility index' or 'cosmetic' descriptors of research quality and of the papers and journals influenced also by commercial considerations and marketing strategies of the publishers. This is also a modern phenomenon because, not just good science but also 'path-breaking' science was published long before the IF metrics were developed.

For the IFs, to have value as motivators and impetus providers[1], they should not be based on the arbitrarily calculated metrics but more by the relevance of the journal to the context of the work to be published as well as by the most important consideration that the author seeks, namely approbation of his/her work from peers and not a generic audience. Here we will cite three contrasting examples. In the same issue of *Current Science*, Srinivasan and Glover[3] report on the traditional skill at highly successful mirror-making in Kerala using non-sophisticated technology, organic and everyday materials to get a sophisticated, high-technology end-product and that has merited a GI patent. As a second example, Kumar[4] has shown how the cultivation of betel vine in subtropical India is attributed to what can only be described as the world's earliest (if not the first) example of anthropogenic regulation of plant microclimate. Finally, Zong *et al.*[5] have shown how more than 7000 years ago the first paddy cultivation in east China was enabled by fire and flood management practices. The first example is of a paper published in *Current Science* (a journal with IF less than 1.0 presently), the second is from a journal that is not listed in *Science Citation Index* (*SCI*) for computation of IF and the third is from *Nature*, a journal with a very high IF. In all these cases, the reports are a retrospective account of technology or science that was practised much before the concept of scientific impact was putforth. Ignoring the source where the paper was published and reading it for its contents, it will be obvious that the work reported is profound. If the mirror-making skills were poor science, they would not have survived this long; if betel vine cultivation was serendipitous, it would not be growing so widely in the country as it is currently and if the Chinese paddy cultivation was poor agronomy, we doubt if there would have been any great significance to rice as a crop plant. These three examples strongly support the argument that the context of the paper or of the R&D work involved in the paper as well as the appropriateness of the peer group, who will be interested in a given paper, should at all times be the sole criterion for deciding the publication outlet and not any arbitrarily calculated metric.

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[2], as well as several arguments for or against the IFs and the several alternative means of calculating similar or related metrics[6]. Finally, Eugene Garfield[7] (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. Evolut.*, 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.

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3. Srinivasan, S. and Glover, I., *Curr. Sci.*, 2007, **93**, 35–40.
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5. Zong, Y. *et al.*, *Nature*, 2007, **449**, 459–463.
6. http://en.wikipedia.org/wiki/Impact_factors; (Last accessed on 8 February 2008).
7. Garfiled, E., *Unfallchirurg*, 1998, **101**, 413–414 (cited in: 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[1] 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[2] 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[3,4]. Several alternatives to replace this system that might cope with its drawbacks have been proposed[5].

Ranade and Kumar[1] 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[6].

As the double-blind review system has its own drawbacks, the main being the reviewers' anonymity, Ranade and Kumar[1] 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[6], 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.

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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