

## The gold standard editorials of *Current Science*

It is not all that easy to write scientific editorials every fortnight. It is even harder to explain complicated concepts and theories in an easily understandable fashion eloquently so that the readers can digest the information without hiccups. Recent responses from readers, especially leading scientists like M. S. Swaminathan on the stepping down of P. Balamram from the editorship of *Current Science* have shown the impact of his writings<sup>1</sup>.

Balamram has written numerous editorials on diverse science subjects in a balanced way targeting more on the subject matter rather than interjecting personal biases. Practising science is after all a selfless business and so he has done his job modestly with enthusiasm, which has been duly recognized by the readership. As a matter of fact, during his tenure as editor, Balamram has set a golden standard on how to write flawless scientific editorials. Even the editorials published in popular journals such as *Science* and

*Nature* are often brief and to the point. Hence they lack thoroughness that the common science readers often expect. Balamram's editorials have always maintained clarity, straightforwardness and attention to detail on various complex science matters. He even told readers bluntly on the problems of finding topics and collecting materials to meet the editorial deadlines<sup>2</sup>.

Scientists at times forget to acknowledge those who play a supporting role such as secretarial services, but Balamram has dutifully acknowledged with gratitude all those who assisted him in providing even simple ideas using search engines and literature materials. He even dedicated a tearful editorial to one of his assistants, Riki Krishnan, who passed away so young and it vividly shows the humble nature of a great scientist<sup>3</sup>.

In 2004, Balamram wrote about the reality of ageing and retirement by quoting what General Macarthur once said, 'just

fade away'<sup>4</sup>. Even if Balamram fades away as an editor into history, the scientific editorials that he left behind will shine evermore to dispel ignorance. So it is about time for the journal management to compile all of his editorials and publish it as a volume of progressive thoughts on science to benefit the next generation of science students.

1. Swaminathan, M. S., *Curr. Sci.*, 2013, **105**, 429.
2. Balamram, P., *Curr. Sci.*, 2013, **104**, 1591–1592.
3. Balamram, P., *Curr. Sci.*, 2010, **98**, 731–732.
4. Balamram, P., *Curr. Sci.*, 2004, **87**, 1163–1164.

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## Peer review: then and now

The opinion piece by Gupta<sup>1</sup> is interesting and would be useful for reviewers as well as authors. While reading about the changing philosophy and process of peer review, I remembered several of my earlier experiences relating to the review process and the way editors handled the same. I would like to share some of these to illustrate how reviewers and editors can be constructively helpful to authors.

As a PhD scholar in late 1960s, I submitted a single-author manuscript to *Genetical Research, Cambridge*. One of the anonymous reviewers was unbelievably helpful and considerate to the young author and took the pains of retyping all the pages of the manuscript to improve its scientific communicability and readability. It may be noted that those were pre-soft copy days and one had to manually type the entire hard copy using the now almost extinct typewriter. The editor wrote back to me, asking if I found the reviewer's retyped version appropriate. This extra work on part of the reviewer was indeed of great help and a valuable lesson for me as a beginner. The paper

finally appeared in *Genetical Research, Cambridge* in 1970. In another case, my PhD supervisor, A. S. Mukherjee, and I submitted a rather long manuscript to *Journal of Cell Biology* in 1970, wherein we questioned the conclusions drawn in a paper on same topic published earlier in the same journal. While one of the reviewers was positive, the other one had many critical comments amounting almost to rejection. The editor reverted to me (as I was the corresponding author) to reply to the queries of the critical reviewer which we did by pointing out why the reviewer's comments were not applicable in the context of our paper. The revised version was accepted the same day it was received by the editor without being referred back to the reviewer. In yet another case in the early 1970s, we (Lakhotia and Jacob), using electron microscopy and EM autoradiography, reported that a major part of the classical heterochromatic chromocentre in polytene nuclei of *Drosophila*, carried typical ribonucleoprotein particles and was as active in transcription (shown

through <sup>3</sup>H-uridine incorporation followed by EM autoradiography) as the typical euchromatic regions. This study was among the first to demonstrate the transcriptional activity of classical heterochromatin *in situ*, but was completely out of sync with the then 'well-established' belief that heterochromatic regions were 'devoid of genes' and were transcriptionally silent. While one of the two reviewers was excited and recommended its publication with the addition of more data, the other was critical and concluded that transcriptional silencing of heterochromatin had been established beyond question and therefore, our observations were wrong and possibly an artifact of electron microscopy. The editor of *Experimental Cell Research*, however, did not reject the paper but wanted us to respond to the critical comments. My response, as the corresponding author, to the detailed critical comments of the reviewer was rather brief to say that if the kind of artifacts that the reviewer thinks can happen in electron microscopic studies, the use of EM in biologi-

cal studies should be stopped! The editor accepted the revised manuscript on the day of its receipt without reverting to the reviewer. The paper appeared in *Experimental Cell Research* in 1974.

As another example of generous discretion by the editor of a journal, I remember reading a personal incident recorded by Curt Stern, a leading *Drosophila* and human geneticist of the pre-molecular biology era, in an article he wrote on the raging controversy between R. B. Goldschmidt and H. J. Muller and other 'natural selectionists'. Goldschmidt had strong difference of opinion with Muller, which reflected in his Presidential address entitled 'Two philosophies of genetics' at the 1956 Genetics Congress. Stern submitted one of his papers to a journal edited by Goldschmidt and criticized Goldschmidt's views while interpreting his own results. As Stern reminisced, Goldschmidt wrote back after reviewing the manuscript that Stern had read his paper, but rejected Goldschmidt's views without understanding, while he (Goldschmidt) read Stern's manuscript, understood it and rejected Stern's interpretation. The significant point, however, was the decision made by Goldschmidt as the editor, to allow Stern's paper to appear in the next issue. Stern, in spite of his disagreement with Goldschmidt's views on 'philosophies of genetics', admired him for this 'greatness'.

These instances reflect the positive roles that reviewers and editors can play in promoting scientific progress. A constructive criticism is always helpful in improving the quality. When combined with some out-of-the-way help, as

experienced in my own case, it can become a turning point in a young author's career. The editor, being a scientist, should be able to take a balanced and informed view, remembering that only the author/s is/are responsible for interpretations offered in the paper and posterity alone can decide on their validity or otherwise.

The contemporary experiences with the review and editorial decisions are, unfortunately, different from my past experiences. Today, it appears that the main responsibility of reviewers and editors is to find the slightest pretext on which a paper may be rejected. Nowadays editors of most journals seem to be only sending the reviewers' comments and authors' replies to each other, without even reading what the two parties are actually stating. This may be a consequence of the exponential increase in the number of manuscripts being submitted to a journal, most requiring rejection rather than acceptance. With rejection having become the primary goal, the possibility of a reviewer extending a 'helping hand' becomes remote.

A basic tenet of progress in science is that as we move along, we shed or modify the current hypothesis/theories. However, the current trend in the reviewing process is that new findings should be in conformity with the current trends/dogmas. 'Junk DNA' is a typical example of the conformist approach. The 'non-coding' RNA/genome, which has become an extremely fast-proliferating theme in recent years, remained as 'junk' for several decades because the so called 'central dogma of molecular biology' had no place for its function, in spite of the fact that the non-coding part is always

a major component of the eukaryotic genome! During the 1980s and 1990s reviewers, editors and granting authorities snubbed or even 'killed' attempts to look for functional significance of the so called 'junk' DNA. Obviously, reviewers, editors and other decision-makers exceeded their briefs.

A reviewer taking the trouble of retying an entire manuscript is obviously a rare event. The editor taking a more proactive role in accepting a manuscript for publication, even if not agreeing to the views of the author/s also seems to have more or less disappeared in today's fast-paced publication process. This needs to change. Peer reviewers must remember that they are also authors. The authors must believe their findings and should not become unduly compliant with the reviewers' observations. The editors should make good use of their own wisdom and give the authors some freedom of interpretation, as long as the reported work is technically sound. We must remember that our understanding of nature progresses only when the new information lets us know what we still do not know.

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1. Gupta, M. N., *Curr. Sci.*, 2013, **105**, 159-161.

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## Open access: publish with caution

The internet and electronic publishing have changed the scholarly communication, where it both complements and challenges the traditional systems. It has greatly accelerated the speed of publishing, increased the overall distribution and number of global audiences and has made it possible to think of new publishing systems like open access (OA) system, where, generally, authors pay for peer review, editing and website maintenance.

However, the major challenge remains quality of publication through the OA system.

Publishers are increasingly opting for an OA route and it is perhaps becoming more popular and diverse. It does include traditional publishers like Springer (Germany), which now publishes about 300 OA titles and a larger number of new powerhouses<sup>1</sup>. However, there are some serious concerns which researchers

need to keep in mind while opting for OA publishing scheme. There are a number of predatory publishers, who are spying on researchers and trying to motivate them to publish in their journals<sup>2</sup>. These journals are fraudulent and work just for money<sup>2,3</sup>. Jeffrey Beall, a Scholarly Initiatives Librarian at the University of Colorado Denver, USA, discusses this significant problem in one of his articles published in *Nature*<sup>2</sup> and on his blog