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EDITORIAL

Scientific Publishing: Eroding Trust

Publishing papers in the best of scientific journals appears to be getting harder. Papers authored by groups of researchers may soon need detailed declarations of the contributions made by each author, a feature already introduced by some journals. In a recent editorial, Bruce Alberts, the editor-in-chief of *Science* outlines changes of policy, designed to discourage 'honorary authors'. He cites a 1994 report of the US National Academy of Sciences, which states that 'just providing the laboratory space for a project or furnishing a sample used in the research is not sufficient to be included as an author' (*Science*, 2010, **327**, 12). Chemists and biologists, among others, who often labour long and hard to produce samples may wonder how they will be rewarded for their efforts. The 'importance of intellectual contributions' is emphasized, presumably making it difficult to have papers with dozens, sometimes hundreds, of authors; a common feature in papers in areas like experimental high energy physics, genome sequencing and multi-centric clinical trials. Interestingly, Alberts announces a new requirement: 'The senior author for each laboratory or group' will be asked to 'confirm that he or she has personally reviewed the original data generated by that unit, ascertaining that the data selected for publication in specific figures and tables have been appropriately presented. Thus, for example, a researcher who prepares a digitally processed figure displaying an assortment of electrophoretic gel separations will need to present all of the original gel data to a specified senior author, who must certify that this has been done when the manuscript is returned for revision.' Figures containing 'gel bands' have, of course, acquired considerable notoriety in many high profile misconduct cases. Alberts' editorial entitled 'Promoting Scientific Standards' appears to have been motivated by a spate of recent retractions of papers published in the 'highest impact' scientific journals and by the unusual step taken by the editor in publishing an 'Editorial Expression of Concern' (Alberts, B., *Sciencexpress*, Dec 17, 2009, 10.1126/science.1186078).

Journals and editors are increasingly under pressure to curb the rising tide of papers which attract adverse attention. A recent paper with an apparently catchy title, 'Reactome Array: Forging a Link Between Metabolome and Genome' (Beloqui, A. *et al.*, *Science*, 2009, **326**, 252), has been widely discussed on the Internet, resulting in the journal's unusual step of 'expressing concern'. The paper describes 'a sensitive metabolite array for genome sequence – independent functional analysis of metabolic phenotypes and networks, the reactomes of cell populations and communities. The array includes 1676 dye-linked substrate compounds collectively representing central metabolic pathways of all forms of life'. The task of chemically synthesizing nearly 1700 dye conjugated substrates for specific cellular enzymes seems forbidding. Unfortunately, the introductory figure in the paper does not appear to have engendered a great deal of confidence in the chemistry, leading to a public discussion on the validity of the 'reactome strategy' (Travis, J., *Science*, 2010, **327**, 22). The rise of 'high throughput' methodologies, systems biology, where the individual reactions of classical biochemistry fade into anonymity under the cloak of networks, and the growing need to develop 'arrays' have driven away the conservatism that characterized biochemistry in its formative phases. Efraim Racker's oft quoted dictum, 'never waste clean thoughts on dirty enzymes' is unlikely to influence a new generation of biological scientists. The need to characterize substrates, especially when synthesized by the hundreds, poses a considerable challenge. Whether the 'reactome' study will stand the test of time remains to be seen. There is more trouble in a field, structural biology, that has long been thought to be largely free of deliberate data fabrication. The recent admission by the University of Alabama, Birmingham that as many as ten papers reporting protein crystal structures may have to be retracted, has been a rude shock to the community of structural biologists (Borrell, B., *Nature*, 2009, **462**, 970). The extensive discussion on these structures on the Internet raises important questions on the review processes at the best of journals. An editorial comment entitled 'Black sheep among the flock of protein structures' notes that while 'mistakes do occur', in the current case 'it appears that the retracted structures were deliberately fabricated and there is no evidence that any experimental data were actually collected' (Dauter, Z. and Baker, E. N., *Acta Crystallogr.*, 2010, **D66**, 1). The papers reporting the rogue structures appeared in *J. Biol. Chem.*, *J. Mol. Biol.*, *Cell*, *Proc. Natl. Acad. Sci. USA*, *Nature*, *Biochemistry*

and *Acta Crystallogr. D*. All of these journals have a high reputation and some are amongst the most sought after journals by ambitious authors. Curiously, data fabrication has also surfaced in the area of small molecule crystallography; long considered as an area made routine by automated data collection and structure determination methods. As many as 70 papers in *Acta Crystallographica Section E*, a journal used as a structure report repository, have been retracted. The papers emanate from two different groups in Jingganshan University in China (Zhang, H. *et al.*, *Acta Crystallogr.*, 2010, **E66**, e11–e12; Liu, T. *et al.*, *ibid*, 2010, **E66**, e13–e14). An editorial in the journal acknowledges that the 70 structures are 'demonstrated to be falsified' (Harrison, W. T. A. *et al.*, *Acta Crystallogr.*, 2010, **E66**, e1–e2).

The spate of problems at high profile journals seems unending. Two papers emanating from the Scripps Research Institute, describing the use of the methods for incorporating non-coded amino acids, for preparing glycosylated proteins have been retracted (Service, R. F., *Science*, 2009, **326**, 1610; Hayden, E. C. and Dalton, R., *Nature*, 2009, **462**, 969). The experiments described in the papers published in 2004 in *Science* and *Journal of the American Chemical Society* appear to have been difficult to replicate, by researchers following up on the initial work, in the same laboratory. Curiously and unhappily, the case has been complicated by an e-mail extortion attempt; clearly a sign of the times. Even classical chemistry has been affected by the virus of doubtful science. A paper with a chemically provocative title, 'Reductive and Transition-Metal-Free Oxidation of Secondary Alcohols by Sodium Hydride' (Wang, X. *et al.*, *J. Am. Chem. Soc.*, doi:10.1021/ja910615z) has been quietly withdrawn after publication, 'on scientific grounds'. For the uninitiated, I might add that sodium hydride is invariably used as a base or reducing agent by synthetic chemists. A claim that the reagent does the opposite – oxidation, was predictably met with skepticism. The expected questions about the process of peer review have been raised in the examples that I have cited. For a long time, misconduct and exaggerated claims seemed more common in the biomedical literature. The widely discussed Jan Hendrik Schon case in physics and the recent examples in structural and chemical biology and chemistry suggest that no discipline can be really free of doubtful science and inappropriate practice. For the last few years there has been a great deal of discussion on plagiarism. This is the easiest form of misconduct to detect. Fabrication and falsification can be much harder to spot.

Are there any lessons to be learnt and are there environmental pressures and influences that promote aberrant behaviour? These are questions that must be considered by scientists in India, even as the pressures to be competitive build up. In the United States, the pressures to obtain research support, which often includes salary support, can be stifling. In countries like China and India the drive to increase the number of publications by offering incentives can be dangerously corrupting. Some institutions offer monetary incentives for publishing papers, scaled by journal impact factors. There cannot be a better catalyst for promoting dubious practices in science. In India, a bloated reward system offers monetary incentives of various kinds, ostensibly for enhancing research performance. The monthly bonus offered to scientists who are elected to fellowships of two academies or those who receive CSIR's Bhatnagar awards are examples of schemes that will promote a scramble for these accolades. The DST's J. C. Bose Fellowship, which should really provide stable research support, now carries an enhanced monthly bonus. This profusion of monetary incentives for 'performing scientists' has distorted the view of many researchers. A long list of publications, preferably in journals with high impact factors, is sometimes desirable in order to enter the 'circle of recognition'. For a privileged few, pedigree and connections may suffice. Some years ago none of this may have really mattered. Unfortunately, attaching a regular monetary bonus to Academy fellowships, the J. C. Bose fellowship and Bhatnagar awards distorts the view of many practitioners of science. Research must be enjoyable, satisfying and intellectually stimulating. Publications must be a measure of the enthusiasm that scientists have for their disciplines. The pursuit of recognition and reward cannot become an end in itself. Genuine mistakes and errors are unavoidable. Disagreements between collaborators are not uncommon. These are a part of the human element in science. Fabrication and falsification erode the base of trust on which science is built.

January is the time of the year when science in India is in the public eye, with the inauguration of the Science Congress by the Prime Minister. As in the past, the need for minimizing bureaucracy and increasing institutional autonomy has been highlighted at the Thiruvananthapuram Congress. These are matters that government can act upon. There are many other issues on which the scientific community alone can have an influence. There needs to be some level of introspection in institutions, academies and government science departments, if the environment for research is to be improved in India. The cursory survey of recent problems in international science that I began with in this column, suggests that the importance of promoting good practices in science cannot be overstated. Mentoring at all levels may be valuable. Alberts notes: 'Effective mentoring is critical to the future success of science. . . . Scientists everywhere can and should do more to promote it'. This is a sentiment that we should all endorse.

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