



We'd like your views – write to: The Editor, *Bone & Joint*³⁶⁰,
22 Buckingham Street, London WC2N 6ET or email editor360@boneandjoint.org.uk

Less is beautiful

Dear Sir,

In the August 2012 issue of *Bone & Joint*³⁶⁰ I found the articles on fraud in the medical literature very interesting.^{1,2} I completely agree that fraud in written medicine, or the particular way in which some researchers write and present their data, is normally biased by the fact that they actually expect to discover something that can lead to a change in clinical practice. An incredible and unique discovery seems very attractive to any ambitious and fame-seeking researcher.

Unfortunately, fraudulent behaviour, I believe by only a small group of researchers, can bring all research into question. Sometimes this may even be quoted by insurance companies that are asked to pay for new therapies.

The use of mannitol is a good example, presented by Marcovitch in his feature article. This reminded me of the different levels of recommendation on the use of high doses of cortisone after spinal cord injury concluded by the National Acute Spinal Cord Injury Study (NASCIS), I, II and III. These were not fraudulent, but moved from being highly recommended to a situation where there was doubt of their real benefit, and ended up as an uncertain recommendation where clinicians were asked to decide for themselves.

One of the issues that I encountered during my time as a researcher at the laboratory for tissue engineering and cartilage repair, led by Shawn O'Driscoll at the Mayo Clinic, was the difficulty when one tried to report results that were not as good as those normally seen by the peer reviewers. There is also the perceived problem that publishing negative results is normally more difficult than publishing positive findings.

I was personally surprised by the number of retracted articles quoted by Marcovitch, up to 788 PubMed articles in ten years. Following his finding, I may try and confirm that by myself. Alas, you might see me copying Marcovitch's own sentences, so I trust this letter will not be regarded as plagiarism.

As Rajasekaran³ argues in his feature article, it is probably the pressure and the number of publications that are pushed to completion in order

to obtain success, promotion, grants, or an increase in salary and professional status that form part of the causes of this corruption. Hwang^{3,4}, on stem cell research, is a sad and unfortunate example of this. Because of this, however, he is today more famous, and quoted more widely than his original false data. Indeed, it appears that he is still publishing on the same topic.

If I seek a grant, I would rather have papers published on the grant's subject area; the more there are, the better my chance of receiving the grant. I would disagree, however, that a large number of articles implies a bad quality of publication. Perhaps there needs to be a smaller number of journals. I am a reviewer for several journals which frequently press me to review as soon as possible articles that might need more detailed attention. My problem is that I also need to use that time to run my busy practice and to undertake academic activities, a situation that can make me very overburdened. Furthermore, being a reviewer does not give me much academic credit and helps barely at all when I apply for a grant.

I like Rajasekaran's solution – less is beautiful. I am pretty sure my family would be happy with that, too.

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4. **Hwang WS, Roh SI, Lee BC, et al.** Patient-specific embryonic stem cells derived from human SCNT blastocysts. *Science* 2005;308:1777-1783. Erratum in *Science* 2005;310:1769. Retraction in **Kennedy D.** *Science* 2006;311:335.