Journal Watch

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Authorship, ghostwriting, and tips on making scientific writing more enjoyable to read

In this issue five papers are discussed covering the subjects of authorship and authorship criteria, ghostwriting and guest authorship, and adding style to scientific writing.

Standards in authorship

In a short editorial in the BMJ, Baskin and Gross[1], editors at the journal *Neurology*, returned to the matter of authorship. They discussed a number of issues that have recently come up regarding the authorship criteria of the International Committee of Medical Journal Editors (ICMJE), and whether they are the correct standards with which to measure 'appropriate' authorship. *Neurology* has gone as far as to develop their own authorship policy that departs from the ICMJE criteria and focuses more on a contributorship model; i.e. identifying everyone who contributed to the study, wrote the report, paid for the research, etc. The journal hopes that this will foster greater transparency and disclosure and help avoid honorary and ghost authorship. Briefly, *Neurology's* criteria for authorship are: design or conceptualization of the study, or analysis or interpretation of the data, or drafting or revising the manuscript. In addition, all authors are required to acknowledge all versions; those who do not qualify as authors should be listed as co-investigators or contributors; any paid medical writer who wrote the first draft or responded to the reviewer's comments must be included in the author byline; and finally, all authors must complete and sign authorship forms with roles and contributions, disclosure forms listing all sources of potential bias, and copyright transfer agreements.[2] Baskin and Gross suggest that 'Identification of professional writers as authors is transparent, fair, and anti-discriminatory: credit is given where credit is due.' The authors put forward that scientific research is gradually becoming a more complex and collaborative process, which means increased challenges regarding transparency in authorship and disclosure. They offered that *Neurology's* policy is a starting point in the effort to improve transparency and suggested that more journals should adopt the contributorship approach in their instructions for authors.

Three more articles on ghostwriting

Rachel Hendrick[3], in a feature in the BMJ, suggested that ghostwriting in medical publishing on behalf of drug companies has a long history. She gave a few examples, historical (going back to the early twentieth century) and recent, of when large pharmaceutical companies have used professional medical writers to anonymously write articles that portray their product in a favourable light, and then have also paid academics to be named as authors. Hendrick says that this is an issue because of the potential influence on the content and conclusions of the article and leads to problems with data integrity and accountability for the reported research. Hendrick did talk about the possible benefits of using a professional writer; they fill a needs gap, they are able to write well and can increase efficiency. However, she seemed quite dismissive of the value of professional organizations, such as European Medical Writers Association (EMWA), and their codes of practice and qualifications to promote working standards and respect for the profession. There was also the suggestion that even if a writer is acknowledged, this still could be considered ghostwriting, which of course goes against EMWA's current position.

Following on from a 2011 article by Stern and Lemmens[5] (previously mentioned in journal watch[4]) about the possibility of imposing fraud liability for ghostwritten articles, Bosch *et al.*[6] outlined specific models of legal liability that could apply to medical ghostwriting in the USA. Briefly, these areas were: (1) when an injured patient's physician relies on a journal article containing false or manipulated data, the authors could be held legally liable for the injuries; (2) authors of articles used as clinical evidence for indications for off-label uses may be liable as a conspirator under the federal False Claims Act for inducing the US government to reimburse prescriptions under false

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pretences; (3) both physicians and sponsor companies may be liable under the federal Anti-Kickback Statute if patients are put at risk by misrepresenting the risk–benefit of a treatment; and (4) although defendants may argue that they have a First Amendment (freedom of speech) right to participate in ghostwriting, the US Supreme Court holds that the First Amendment does not shield fraud. Overall, the authors suggested that the current responses to ghostwriting are unsatisfactory and argue that the only remaining option is the legal system in order to ensure that guest authors take more responsibility for the work they put their names to. How realistic or practical this would be is debatable, especially considering that taking legal action can be both extremely expensive and time consuming.

In a recent commentary, Bosch and Ross[7] debated whether ghostwriting and guest authorship should be seen as research misconduct. They suggested that there are many reasons why academics, sponsors, and medical writers engage in ghostwriting; for example, enhancing professional standing, product promotion, and employment, respectively. They suggested that, at the moment, ghostwriting is perceived as a slight failing or a little bit naughty, rather than as an unethical practice. They went on to say that in this culture, ghostwriting and guest authorship are fool's gold or 'an unspoken permission to fatten curricula with redundant reviews and, predominantly, lower-impact clinical research studies'. Bosch and Ross argued that guest authorship could be seen as a form of plagiarism because using someone's name implies credit for work done by someone else. But the same probably cannot be said for ghostwriting as a ghostwriter 'willingly creates text for attribution to others'. The authors think ghostwriting and guest authorship should be considered acts of research misconduct, as they consider both situations clearly perpetuate a fraud on an unsuspecting public and profession; and feel that professional organizations, such as The Office of Research Integrity, should include ghostwriting and guest authorship in their official definitions of misconduct.

Style and scientific writing

Advice on how to incorporate style into scientific writing, to make it more enjoyable for the writer and the reader, was given in an editorial by Franzblau *et al.*[8] The authors said that the communication of study findings is at the core of scientific research; however, medical writing is still often seen as quite dry and formulaic. The authors offered a number of tips on improving the quality and readability of scientific writing. Some of the best ones were: shorter articles are easier to read, most could be considerably shorter without losing the overall message, so authors should edit an article several times to condense the text; try to write in an unambiguous, logical, succinct fashion; use the active voice rather than passive phrases (the grammar tool in word processing programmes can be helpful to highlight passive phrases); reduce repetition by using a thesaurus to provide alternative words; authors should be allowed to use and develop their own personal style of writing; and, there is room for imaginative composition in the introduction, discussion, and conclusion sections of a manuscript, even if the methods have to adhere to strict formats. The take-home message from this article is that the quality of scientific writing needs to improve in order to establish a new, higher standard of literary quality in scientific communication.

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Open science as a replacement for peer review of scientific articles?

There have been complains about the peer-review process, but what is the solution? A recent article by Thomas Lin in *The New York Times* (16 January 2012), 'Cracking open the scientific process', discusses 'open science' as a possible solution. Open science means making the results of scientific research freely available and using the power of social networking to replace peer review.

According to the article, some scientists feel that the peer-review system is 'hidebound, expensive, and elitist' and that it should be replaced by open science. These criticisms are probably reasonable. Peer-reviewed journals are expensive to run. However, open science does not completely resolve this problem because professional curation and perseveration of data are time consuming and expensive. The 'hidebound' and 'elitist' criticisms are a bit vague and are not really addressed in the article. Probably they mean that that there can be a political side to getting published – who the authors are and their institutions they are affiliated with can affect the ability to be published, when it is the quality of the science alone that should really be the deciding factor. The criticism is also probably made because reviews are sometimes insufficient so that bad science gets published, whereas good, innovative science is sometimes blocked because it contradicts existing dogma. Although improvements are being made, publishing research results *should* be better, faster, and cheaper and should take better advantage of electronic media.

As examples of open science, the article mentions online-only journals like *Nature Communications* (www.nature.com/ncomms/), and the PLoS journals (www.plos.org/). These journals simply do not charge for access, although they charge the authors for submitting an article. These are faster to print, and open access is a great way to catalyse the sharing of scientific information, but these journals do not eliminate peer review and therefore do not truly constitute open science.

The article also mentions ResearchGate (www.researchgate.net/), an interactive website, where scientists can pose and answer questions from other scientists. This website is great for sharing ideas, but it is not currently a site for publishing the results of research or for peer review of those results. Moreover, sharing of ideas is not the same as a true in-depth critique of study results. On the other hand, using the Internet for post-publication review of research is a great idea.

Unfortunately, the *New York Times* article does not explain or provide examples of how open science could replace professional peer review, nor does it address whether eliminating peer review is a good idea. Even with many people critiquing an article through a social networking site, whether it is possible to attain the same depth of review as using two or three dedicated peer reviewers is not yet clear. Also, experience with Wikipedia® shows that using social networking in place of true peer review carries certain risks for abuse and misinformation.[1–3] So it is not yet clear, at least from the *New York Times* article, how or why open science would be better than professional peer review.

For the moment, peer review is the only system for the in-depth evaluation of research and the conclusions made from it. Open science seems to be a great way to improve information sharing and access to published research, but whether it is a good replacement for peer review remains to be seen. Changes are coming, but what they will look like is not yet obvious.

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Cause: Its effect on biomedical research

Jonah Lehrer contends that we jump to conclusions about causation too quickly and explains his reasoning in his article 'Trials and errors: Why science is failing us'.[1]

He challenges the assumption that ever deeper research of a system to discover subtle correlations

will reveal how the entire system works. The article is certainly a thought-provoking read for anyone interested in biomedical research, especially against the background given by Lehrer that R&D costs of discovering a new drug are about 100 times higher (adjusted for inflation) than in the 1950s and development takes three times as long. Even more disheartening is that 'According to one internal estimate, approximately 85 percent of new prescription drugs approved by European regulators provide little or no benefit'.

Lehrer illustrates that causes are inferences rather than facts by referring to experiments conducted in the 1940s by the Belgian psychologist Albert Michotte which showed how humans observe a series of events and form conclusions that one thing causes another. For example, if one rolling ball touches another and the other ball moves, the first ball is assumed to have caused the second one to move. People thus translate perceptions into causal beliefs.

In scientific research, statistical correlation has been developed to show associations between measurements and the assumed cause. But Lehrer points out that reliance on correlations has entered an age of diminishing returns. The easy causes have been found and scientists are forced to search for the tiniest of associations but too often rely on simple correlations and fail to make the effort to search for secondary and tertiary interactions in these systems. Lehrer gives a number of examples of this failure starting with Pfizer's withdrawal of the drug trocetrapib after it had entered phase III clinical trials. The withdrawal was announced 2 days after the company's CEO had stated that this new cholesterol-lowering drug would be 'one of the most important compounds of our generation'. Instead of preventing heart disease it was found to lead to a 60% increase in mortality. Lehrer concludes that because the individual steps of the cholesterol pathway were well understood false assumptions were made about how the pathway functions as a whole.

Another example he gives is back pain from which 80% of us will suffer at some point in our life. Doctors used to tell their patients to take time off and rest in bed. Ninety per cent of patients with lower back pain recovered within 6 weeks. However, magnetic resonance imaging was introduced in the 1970s and showed a strong correlation between back pain and seriously degenerated spinal discs. Doctors changed tack to prescriptions of epidurals and surgical removal of the damaged tissues. Subsequent research found disc abnormalities were just as likely to be correlated with no pain and a recent study found that a small subset of non-spinal factors such as smoking and depression were more closely associated with serious back pain. Another illustration he gives is biomarkers where a study has now found that 83% of supposed correlations become weaker with further studies.

The readers' comments on the article variously accuse Lehrer of being provocative, anti-science, and praise him for being brave. He is charged with promoting holistic medicine, the prospect of which seems to be like a red rag to a bull for many medical practitioners, and then defended against having done so. A few comments from readers are worth quoting:

> The nature of publishing has also changed such that scientists are encouraged to publish piecemeal rather than wait for Ultimate Certainty before submitting a study for publication. On the plus side this keeps a good flow of information rolling, but on the minus side it means the likelihood of being inaccurate, or downright wrong, proportionally increases.

> If anything your examples only reinforce the point that sufficiently powered, double-blind studies are the only check we have against our frequently incorrect assumptions and intuitions about causality.

> The pharmaceutical industry is looking for answers, but is starting from the wrong place. Without understanding the mind's effect on the body we'll never come up with consistently effective therapies.

> Science is, in fact, not failing us at all; rigorous experimental design (e.g. Phase III clinical trials) are defeating the poor initial research.

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The higher the income the lower the morals

Medical writers are it is to be hoped concerned about ethics. For this reason, an article published in PNAS earlier this year should be of some interest to us. The article reports five studies undertaken in naturalistic and experimental settings with social class as the major variable. The studies found that upper-class people are more unethical than lower-class people.

The investigators concluded that abundant resources and elevated rank give upper-class people the freedom and independence from others which causes them to prioritize self-interest over the welfare of others. Furthermore, rich people perceive greed as positive and beneficial, which the authors contend flows from economics education with its focus on maximizing self-interest. These upper-class attitudes result in a higher tendency to unethical behaviour among the rich than among the poor. The relative independence from others and increased privacy in their professions result in fewer constraints and less perceived risk associated with committing unethical acts, added to which such people have a feeling of entitlement.

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Recommendations for improving the reporting of industry-sponsored studies

A commentary recently published in the Mayor Clinic Proceedings[1] by The Medical Publishing Insights and Practices (MPIP) will be of interest to medical writers working with publications in the pharmaceutical industry. It makes the following 10 recommendations for closing the credibility gap in reporting industry-sponsored clinical research:

1. Ensure clinical studies and publications address clinically important questions.
2. Make public all results, including negative or unfavourable ones, in a timely manner, while avoiding redundancy.
3. Improve understanding and disclosure of authors' potential conflicts of interest.
4. Educate authors on how to develop quality manuscripts and meet journal expectations.
5. Improve disclosure of authorship contributions and writing assistance and continue education on best publication.
6. Practices to end ghostwriting and guest authorship.
7. Report adverse event data more transparently and in a more clinically meaningful manner.
8. Provide access to more complete protocol information.
9. Transparently report statistical methods used in the analysis.
10. Ensure authors can access complete study data, know how to do so, and can attest to this.
11. Support the sharing of prior reviews from other journals.

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We like your article, but there's one small thing you could do for us … The problem of coercive self-citation

Have you heard of editors trying to get authors to cite more articles published in their journals? US researcher Eric Fong had not, until it happened to him.

Teaming up with fellow academic Allen Wilhite, he decided to investigate the scale of the problem, which the two of them refer to as 'coercive self-citation' and define as a request to add unspecified citations from the editor's journal. Their findings were published in the February issue of *Science*.[1]

Wilhite and Fong invited over 50,000 academics in business, economics, sociology, and psychology to participate in a survey[2] to find out how many had heard of this practice and how many had themselves been affected by it.

Of the 6672 people who responded, some 40% were aware of coercive self-citation and 20% had personally encountered it.[1] Further analysis showed that junior researchers were more likely than senior ones to give in to an editor's demands, and that journals with commercial publishers were more likely to coerce than those published by academic societies.

Contributing to a follow-up piece on the *Nature* website,[3] publishing consultant Phil Davis highlights possible sources of bias in the study – e.g. responders potentially being more likely than non-responders to be aware of coercion by journal editors – but ultimately accepts that the problem exists.

The editors of the two journals that were most commonly named by responders as engaging in coercive self-citation unsurprisingly deny involvement in this kind of activity.[3]

Citations are the basis for journal impact factors. Referencing a couple of articles published in *Journal of X Y* to keep the demanding editor happy may seem trivial to the author who does it, but impact factors are a big deal. Academic careers depend on them.

Earlier studies have highlighted serious impact factor abuses. In one notable incident, a journal managed to increase its impact factor by 18 ranks by publishing a single article that cited a jaw-dropping 303 of the journal's previous papers.[4]

In an earlier case,[5] authors who submitted a manuscript to the journal *Leukemia* received a letter containing the following request: 'We have noticed that you cite *Leukemia* [once in 42 references]. Consequently, we kindly ask you to add references of articles published in *Leukemia* to your present article'.

It is by no means the only such example.[6]

Marie McVeigh, director of Thomas Reuters' Journal Citation Reports, feels that the figures reported by Wilhite and Fong are higher than she would have expected based on her own data. Nonetheless, Thomas Reuters has taken steps to address the problem. It now publishes impact factors with *and* without self-citations and temporarily delists journals that have used self-citation to boost their impact factors.[7]

But is this enough? Should self-citations be removed from impact factors altogether? While Wilhite certainly advocates this change, he does acknowledge the need for studies of other disciplines (including biological sciences). And he is yet to secure McVeigh's support.

For the record, I can honestly say that I have never been put under pressure to cite *TWS* articles when writing for this journal!

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