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## From the Editors' Desks

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**2010 Annual General Meeting**

We hope that as many of our Italian members as possible will make the effort to join us at Hotel Genio, Turin, on the evening of Saturday 3rd July, as well as any EASE members who are attending ESOF. The speaker will be Philip Campbell (see p37).

**Social networking sites**

Our accounts on Twitter, Facebook, and LinkedIn are attracting followers from within the membership and beyond. These should prove an excellent way of disseminating information about our activities to a wider audience.

**Science Editors' Handbook**

Petter Oscarsson is commissioning updates and new articles for the Handbook. Anyone with any ideas for topics they would like to see covered should contact Petter or the EASE Secretary.

**EASE website**

The latest new feature is an area where members may promote their editorial services for free (and non-members may do so for a small fee). Emma Campbell is also working on a complete

facelift and new content management system: a technical feat for which she has our gratitude and admiration!

**EASE author guidelines**

These have been approved by Council and are now available via the website. Our thanks to Sylwia Ufnalska for suggesting and writing these. Sylwia will now translate these into Polish and we welcome volunteers to translate them into other languages. Please contact Sylwia or the Secretary.

**Goodbye and good luck**

This issue is the last in which NewsNotes is compiled by Richard Hurley. Richard has decided to step down from the Publications Committee to allow him to concentrate 100% on his full-time job. He has managed the NewsNotes column for more than three years, collecting news items relevant to editing and publishing and of interest to our members. We thank Richard for his unflinching productivity, and wish him well for the future.

**Contributions for next issue**

The copy date for the August issue is 15 June.

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## Editorial

### England's libel laws and scientific censorship

On 15 March 2010 the British Chiropractic Association (BCA) dropped its libel case against the science writer Simon Singh.<sup>1</sup> Singh could have settled earlier but spent two years and more than £200,000 defending himself so this must be a huge relief for him—but it was also likely a huge relief for his many supporters. These scientists, academics, and freedom of speech campaigners say that English law is being used inappropriately to stifle free speech and scientific debate, not just in the United Kingdom but around the world. They hope that Singh's win will accelerate the reform of libel laws in the United Kingdom.

Libel is the publication of a defamation that might damage someone's reputation. The costs of defending an accusation of libel can spiral to hundreds of thousands of pounds. Even if the defendant wins, the costs that the judge awards rarely cover the actual outlay. For example, in 2008 the doctor and journalist Ben Goldacre successfully defended a libel charge brought against the *Guardian* newspaper by the vitamin seller Matthias Rath.<sup>2</sup> The case cost the newspaper £535,000 to defend, but only £360,000 was recovered.<sup>3</sup> Fear of these costs mean that many articles do not make it onto page, and pre-emptive editing by lawyers—that is, self censorship by journal editors—is a necessity. This impedes scientific debate and is ultimately a disservice to the public, which has a right to properly scrutinized information.

English libel laws have also been criticized for being used by parties based outside of England to sue non-UK citizens because it is easier for the complainant to win. This may be because under English libel law the burden of proof is on the defendant to prove what they have said is true, and rigorous defences of fair comment and public interest are lacking. This “libel tourism” can be brought even if the alleged defamation is published in a work that sells just a few copies in the UK or on websites accessible in the UK but not intended for that audience.

An example is the case of Peter Wilmshurst, a consultant cardiologist in the United Kingdom who is being sued by the US medical device manufacturer NMT Medical. Wilmshurst was a principal investigator in a trial of a device, and he made comments about it that the manufacturer disagreed with on a US website. It is suing him for libel in London. Attempts to settle have failed, and even if he wins, a trial could ruin Wilmshurst financially.<sup>4</sup>

The BCA brought the action against Singh after he wrote an article in the *Guardian* comment pages in April 2008.<sup>5</sup> In the piece Singh criticized the BCA for “happily promoting” spinal manipulation for diseases in childhood such as asthma and colic—a treatment that he called “bogus” and for which he said there is “not a jot of evidence”. Clearly this matter of public interest should be scrutinized.

In an initial hearing in London on 7 May 2009, Mr Justice Eady ruled that Singh's article was a statement of fact that the BCA was being “consciously dishonest”.<sup>6</sup> This meant that

Singh would not have been able to claim a defence of fair comment and would have had to prove that the BCA was being “consciously dishonest”. This ruling might have also set a precedent, making it difficult for anyone to question claims made by companies or organizations.

The *Guardian*'s lawyers recommended settling out of court. But Singh maintained that Justice Eady's interpretation was not what he had intended, and he took the case to the Court of Appeal. On 1 April 2010 the Lord Chief Justice, Lord Neuberger, and Lord Justice Sedley ruled that Eady had “erred”. They allowed Singh the defence of fair comment, which has led to the BCA dropping the case.<sup>7</sup>

Sense About Science has campaigned for Singh ([www.senseaboutscience.org/freedebate](http://www.senseaboutscience.org/freedebate)). And the Index on Censorship and English PEN have presented a list of suggested legal reforms (<http://libelreform.org>). These include a cap on costs; strengthening the defences of fair comment and public interest; and exclusion of large organizations from bringing libel claims unless they can prove malicious falsehood.

Scientific debate should occur in journal pages and not the courts. With these and other cases in the spotlight, libel reform is likely soon. A recent Ministry of Justice review has led to reduced success fees that lawyers can charge in defamation cases, from 100% to 10%,<sup>8</sup> and a working party at the same ministry has reported on the statute of limitations and libel tourism.<sup>9</sup> All three main UK political parties have committed to libel reform in their manifestos for the election in May, although they are short of detail.

**Richard Hurley**

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## Articles

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### Diversity begets diversity: an analysis of relationships between author, reviewer, and editor populations

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Presented at the 10th EASE Conference, Pisa, Italy, September 2009

#### Abstract

*Background:* Peer review is critical in the objective evaluation of research, yet biases have been demonstrated against females and by US nationals. One means to mitigate these effects is to increase the diversity of the reviewer and editor populations.

*Methods:* Using manuscript-handling data from papers submitted between 2006 and 2007 to nine ecology and evolutionary biology journals, I examined the geographical and gender diversity of authors, reviewers, and editors and the relationships between them.

*Results:* Geographical diversity of accepted authors increased with increasing diversity of reviewers. There was no relationship between editor and referee diversity. The reviewer population was more male biased than the author population, and editor and reviewer sex ratios were positively related; having more male editors in a journal resulted in more male reviewers.

*Conclusion:* Low geographical diversity of reviewers selects for reduced diversity in the population of publishing authors, indicating a need to maintain a geographically diverse pool of qualified reviewers. The proportion of females holding editorial positions reflects the population of submitting authors across journals. An effort to solicit more reviews from qualified females, or to seek increased participation by females, would result in a reviewer population more representative of the research community.

#### Introduction

Peer review is arguably one of the most important tools used in science to assign credibility to research. We rely on it as a means both to filter research into appropriate venues and to provide a judicious appraisal of the quality of research.<sup>1</sup> As such, it is necessary that the reviewers selected are experts within their given discipline and objective in their assessments.

The potential for bias to exist in the peer-review system has been well researched in the biomedical sciences with respect to both grant application and manuscript publication, and there is evidence that factors other than the intrinsic quality of the work affect review outcome. For example, both gender and national affiliation of the primary author or applicant have been shown to affect the reviewer's recommendation. In a classic study, Wennerås and Wold

showed that in the review of postdoctoral grant proposals, females needed to be 2.5 times as productive as males in order to receive an equivalent evaluation, suggesting value in masking the identity of authors to a reviewer.<sup>2</sup> Although not all studies have found such an effect, a meta-analysis of grant decisions shows that overall women fare less well than men.<sup>3</sup> Other studies have revealed country of origin to be a significant predictor of final outcome of manuscripts<sup>4</sup> and conference abstracts,<sup>5</sup> with authors from North America and Europe receiving more positive outcomes. Again, masking author's affiliation can reduce this effect.<sup>5</sup>

In addition to authors' characteristics, review quality and recommendation can vary by reviewers' attributes. One study showed that male reviewers were more extreme in their recommendations ("accept as is" or outright "reject") than females and worked with male editors more frequently than with female editors.<sup>6</sup> Similarly, a survey of ecologists revealed that male reviewers recommended rejection of manuscripts more frequently than females.<sup>7</sup> Reviewers' recommendations have also been shown to vary by reviewer's origin. US reviewers for a medical journal were more likely to recommend acceptance of manuscripts submitted by US authors than were non-US reviewers.<sup>8</sup> In ecology, US authors were more likely to cite papers from the US than from the EU, whereas European authors cited both EU and US papers.<sup>9</sup> If this nationality bias extends beyond citations to reviewers' behavior, as in medicine, then ecology may benefit from a geographically diverse pool of reviewers representative of the population of researchers in the field.

Diverse participation across all levels in academia is essential for the development of a discipline through the integration of new ideas and perspectives. However, diversity may also help to mitigate biases that may exist within one or other demographic sector. Here, I explore the geographical and gender diversity (sex ratio) of author populations across nine ecology and evolutionary biology journals, the demographics of the corresponding reviewer and editor populations, and the relationships therein. Due to different "sampling effort" across the populations (large sample sizes of self-selecting (submitting) author populations in comparison with small populations of appointed editors), measures of diversity cannot be compared directly. However, given published evidence of nationality biases, I predict a positive relationship between reviewer diversity and the diversity of accepted authors but do not predict significant relationships by sex ratio.

## Methods

Manuscript submission and handling data from nine ecology and evolutionary biology journals published by Wiley Blackwell between January 2006 and September 2007 were examined. Data were extracted from Manuscript Central and included name and contact information of the authors, reviewers, and editors, in addition to outcome. All submissions were examined, and all original research articles were included in the analyses presented here. When a manuscript was resubmitted and was therefore represented in the database multiple times, demographic data from the first submission were extracted, in addition to the outcome of the final iteration. This resulted in a total of 7615 manuscripts.

Gender was assigned to first authors and reviewers using Muse Names, a database of names for babies.<sup>10</sup> Gender was considered “uncertain” if the individual used initials, when the name was not in the database, or where it was listed as both male and female at a ratio of between 0.2 and 0.8 (the database lists the occurrence of the name across multiple countries). Where the database sex ratio of a name was 0.2 or lower the name was assigned the gender female and where the ratio was 0.8 or above it was assigned the gender male. In the case of editors, gender was assigned directly to individuals following evidence from internet searches.

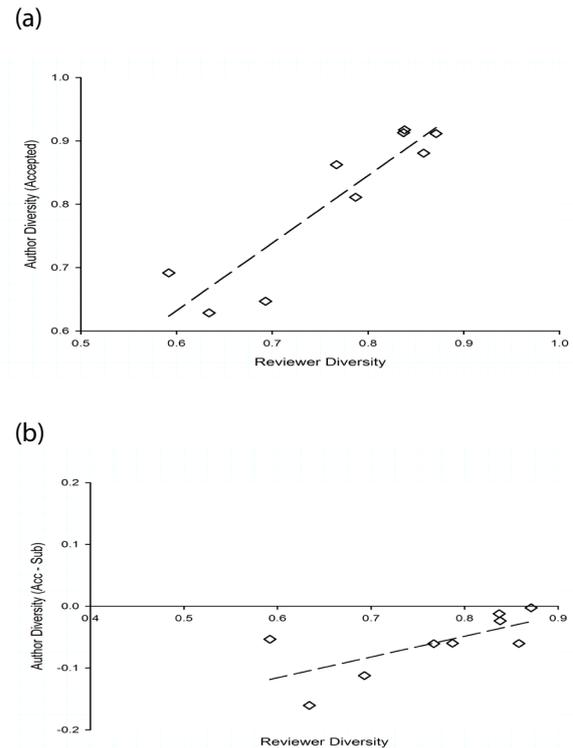
The Simpson's Index of Diversity,  $1 - (\sum n(n-1) / N(N-1))$  (where  $n$  is the number of individuals within each category (country) and  $N$  is the total number of individuals) was calculated for geographical diversity of first authors, reviewers, and editors at each journal, using the country of their host institution. This index takes into account the richness (number of countries) and evenness (number of individuals within each country) of the samples. The sex ratio of submitting (first) authors, reviewers, and handling editors was also calculated for each journal. Since individuals (primarily editors) are represented multiple times in the database, the indices of diversity and sex ratios calculated reflect the manuscript handling statistics as opposed to the actual diversity or sex ratio at a journal, except where noted.

Pearson correlation analyses were used to explore relationships between diversity indices (Simpson's and sex ratio) across groups of individuals. A one-way within-subjects (journals) ANOVA was used to test for differences in sex ratio and the number of countries represented. All tests were two-tailed, with alpha set at 0.05.

## Results

### Geographical diversity

The mean ( $\pm 1$  SD) number of countries represented among submitting authors, reviewers, and editors across journals was 44.2 (14.6), 33.1 (10.5) and 8.1 (5.5), and this differed significantly between groups ( $F_{2,16}=45.698$ ,  $P<0.001$ ; linear contrast  $F_{1,8}=51.676$ ,  $P<0.001$ ). The mean ( $\pm 1$  SD) Simpson's Index of Diversity was 0.87 (0.08), 0.76 (0.10) and 0.57 (0.22) for authors, reviewers, and editors. There was no relationship between editor diversity and diversity of the



**Figure 1: Relationships between geographical diversity of reviewers and (a) first authors of accepted manuscripts and (b) change in first author diversity between accepted and submitted manuscripts, for nine ecology and evolutionary biology journals.**

reviewers used ( $r=0.312$ ,  $P=0.412$ ), but there was a strong positive correlation between reviewer diversity and the diversity of published first authors ( $r=0.912$ ,  $P<0.001$ ; fig 1a). Since the diversity of submitting authors was positively correlated with the diversity of accepted authors ( $r=0.948$ ,  $P<0.001$ ), I also examined the change in author diversity between the submitted and accepted pool of manuscripts. Again there was a positive correlation between reviewer diversity and change in author diversity ( $r=0.689$ ,  $P=0.040$ ). Those journals with higher reviewer diversity incurred little or no change in author diversity while journals with lower reviewer diversity selected for lower diversity in the population of accepted papers (fig 1b).

### Sex ratio

The mean ( $\pm 1$  SD) *actual* sex ratio of authors, reviewers, and editors was 0.69 (0.03), 0.77 (0.05), and 0.71 (0.06), with the reviewer pool significantly more male biased than the author pool ( $F_{2,16}=5.642$ ,  $P=0.014$ ; quadratic contrast  $F_{1,8}=14.086$ ,  $P=0.006$ ).

There was a positive correlation between the editorial *handling* sex ratio and the sex ratio of the reviewers across journals ( $r=0.713$ ,  $P=0.031$ ). There was no relationship between reviewer and author sex ratio ( $r=0.449$ ,  $P=0.225$ ) or between reviewer sex ratio and the change in sex ratio between submitting and accepted authors ( $r=0.199$ ,  $P=0.609$ ).

There were no significant relationships between the sex

ratio or diversity of accepted authors and the impact factor of a journal ( $P=0.83$  and  $0.94$  respectively).

### Discussion

The peer-review process depends on the largely unpaid participation of editors, reviewers, and authors in order to function. These individuals represent multiple disciplines, institutions, countries, and levels of scientific experience, and it would be unrealistic to assume that all reviewers will act the same. Indeed, the peer-review system relies, in part, on the diversity of the reviewer pool.

#### *Geographical diversity*

The data presented here indicate that the geographical diversity of authors and reviewers is higher than that of editors. This is a consequence of the lower number of countries represented by journal editors, arising from a more limited population size (there is a finite number of editorial positions in comparison with the seemingly limitless potential for manuscript submission). Hence, while it might not be statistically meaningful to compare geographical diversity across groups, examining the relationships between them nonetheless provides insight into the potential for nation-level biases.

The geographical diversity of reviewers and authors was positively correlated (fig 1a). Variation in the status of the journals, with some having an international scope and others more regional, might explain this result. Removal of the two journals with the lowest diversity values for reviewers (reflecting a possible small scale geographical focus) did not change the significance or direction of results. This suggests that the relationship is not due solely to journal scope and may instead reflect an amelioration of nation-level bias.

Since the pool of accepted authors is a subset of the submitting author population, it is not surprising that the diversity of accepted authors reflected the geographical diversity of submitted papers. What is more important is that the difference in diversity between these groups reflects a process of filtration arising from peer review. Those journals exhibiting a greater difference, or a more homogeneous set of accepted authors, utilized a less diverse pool of reviewers. Such an effect may arise from differential reviewer recommendations across countries, independent of author's origin, or as a result of author-reviewer interactions.<sup>8</sup> For example, a non-native English speaker might more readily accommodate issues of grammar in a manuscript and have a higher probability of recommending acceptance than a native English speaker. If this practice occurs, the result in journals with low reviewer diversity would be an apparent nationality bias, reducing the acceptance rate of papers by non-native English speakers relative to others and thereby decreasing geographical diversity.

To fully answer this question, data on individual recommendations are required, but it appears that a broad selection of reviewers begets a high diversity of published authors. Interestingly, there was no relationship between editor diversity and reviewer diversity, hence an important

finding is that the editorial board does not necessarily need to maintain high diversity to ensure geographical diversity in its published authors.

#### *Sex ratio*

It is encouraging that the sex ratio of editors reflects the genders of submitting authors, in contrast with reviewers, who were more male biased. Recent discussion has highlighted the lack of female scientists in high level positions,<sup>11-13</sup> arguing the benefits of integrating multiple perspectives and presenting editors as role models for junior female researchers.<sup>12</sup>

The data presented here suggest a gender preference by editors (given the significant correlation between editor and reviewer sex ratios), yet overall the population of reviewers is significantly more male biased. Given that the ratio of females at high levels (editors) is representative of the sex ratio across all levels (authors), why is the reviewer pool significantly more male biased?

One possibility is in the differential selection of male and female reviewers by editors of different genders.<sup>6</sup> Another is that females are less likely to accept invitations, as has been suggested for female editors in evolutionary biology,<sup>14</sup> or that individual females are over-utilized. The latter interpretation would explain the apparent discord between the male biased reviewer pool and the positive association between editor's and reviewer's gender. Regardless of how the reviewer sex ratio arises, there is no relationship with the sex ratio of authors eventually published, which suggests that in ecology and evolution, same-sex preferences in reviewing are not present.

#### *Beyond bias?*

Preference for, or leniency towards, a particular national group or gender may not constitute a conscious bias. Rather, our objectivity may be shaped by cultural and environmental influences that vary by nationality, ethnicity, gender, etc. Gender schema theory proposes that the perception of gender roles learnt throughout childhood results in a set of implicit, or non-conscious, hypotheses about sex differences held by both men and women<sup>15</sup> that scale up to create large disparities in salary, promotion, and prestige.<sup>16</sup> Schemas can apply to groupings beyond gender, and while the influences may be non-conscious, the way they are manifest in the peer review process may be non-trivial and result in apparent bias.

Evidence for bias in peer review is mixed<sup>1</sup>; however, there is widespread support for implementing double-blind review. A comprehensive survey revealed that double-blind review was the preferred and most effective form of peer review, due to the objectivity associated with this method and its perceived fairness.<sup>17</sup> Perhaps not surprisingly, double-blind review was more favoured by women, individuals from Asia, and younger (junior) researchers<sup>17</sup> (also surveys in ecology,<sup>18</sup> medicine,<sup>19</sup> and weather forecasting<sup>20</sup>). While limitations include the potential to infer the author's identity, previous studies indicate that reviewers were correct on only 25-42% of occasions,<sup>21</sup> and degree of certainty may yet prove to be important.

It has been suggested that in the absence of strong evidence demonstrating a benefit of double-blind review, the logistical challenges outweigh the benefits.<sup>22, 23</sup> For editors in ecology and evolution, increasing the diversity of the reviewer population in terms of both nationality and gender does not impose such a cost and, quite simply, may further ameliorate bias in a single-blind reviewing process.

Clearly we do not need to promote diversity for diversity's sake or compromise our standards of expertise. However, to make certain that the best material published reflects the full breadth of ideas and perspectives held by our diverse and changing research community, we need to consciously ensure that our reviewer pool is both broad and balanced in gender and geography.

I would like to thank Wiley-Blackwell Publishers, Liz Fergusson, and the participating ecology and evolutionary biology journals for access to their submission data. Thanks also to the NCEAS Publication Bias working group and John Parker for comments on this manuscript, and to the European Association of Science Editors for the speaker invitation that this paper complements. This work was conducted while I was a Postdoctoral Associate at the National Center for Ecological Analysis and Synthesis, a Center funded by NSF (Grant #EF-0553768), the University of California, Santa Barbara, and the State of California.

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### EASE at World AIDS Congress in Vienna

Shirin Heidari has led a team of EASE members, including Tom Babor, Karen Shashok, Edith Gruslin, and Kerstin Stenius, who also represent the International Association of Addiction Journal Editors, AuthorAID, and the Journal of the International AIDS Society, in submitting proposals for three workshops to be held during the World AIDS Congress in June. We are pleased that one was accepted, "Scientific integrity and ethical issues in publishing in the field of HIV and AIDS". This will be led by Tom Babor, with contributions from Kerstin Stenius, Ana Marusic, Shirin Heidari, and Elise Langdon Neuner.

## Essays in Editing

### Citing oneself

**James Hartley**

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One of the many pleasures an author can have is to see his or her name in print, especially if it is cited (in a positive way) by someone else. However, perhaps an easier way of ensuring your name appears in print is to cite yourself in an article you are writing...

One would imagine that this would be relatively simple to do. However, there are at least two hidden rules about how to proceed in scientific writing that seem to make life difficult. The first of these – which is often spelled out – is that authors should not identify themselves or their colleagues when submitting their manuscripts for peer review.<sup>1-3</sup> Here are two representative quotations:

Authors must completely blind the manuscript before submitting it. A title page and/or other text that could identify the authors to reviewers (such as an Acknowledgements paragraph) must be removed from the main document, but may be uploaded separately...<sup>2</sup>

Information in text or references that would identify the author should be deleted from the manuscript (e.g., text citations of “my previous work,” especially when accompanied by a self-citation; a preponderance of the author’s own work in the reference list). These may be inserted in the final draft.<sup>3</sup>

This procedure, where the author is anonymous to the reviewers, and vice versa, is known as double-blind reviewing, and the value of this procedure, and variants of it, is subject to much debate. One way of staying anonymous as an author is to refer to yourself in the third person in the hope that referees will not realise that this person is you. Such strategies, of course, do not always work well: in some research areas it is relatively easy for referees to identify successfully the authors of blinded manuscripts,<sup>3-6</sup> and in any case – anticipating the second rule – writing in the third person leads to ugly and undesirable prose.<sup>7,8</sup>

The second rule is that scientific writing should be objective, and impersonal. Consider, for example, the following advice from Smyth:

Good scientific writing is characterised by objectivity. This means that a paper must present a balanced discussion of a range of views.... Moreover, value judgements, which involve moral beliefs of what is “right” or “wrong” must be avoided.... The use of personal pronouns is unnecessary, and can lead to biases or unsupported assumptions. In scientific papers, therefore, personal pronouns should not be used. When you write a paper, unless you attribute an opinion to someone else, it is understood to be your own. Phrases such as “in my opinion” or “I think,” therefore, are superfluous and a waste of words.... For the same reasons, the plural pronouns *we* and *our* are not used.<sup>9</sup>

This paper is concerned with the second of these “rules” rather than the first. I have been looking at examples of how people try to be objective and impersonal, and then seeing if their text could be made easier to read if this advice was ignored. I consider three sets of possibilities: a single author citing him or herself; two joint authors citing one, both, or one and someone else; and three joint authors citing one, two, or all three, one or two of them and someone else, etc.

The examples are based on texts that I have been reading while writing this article. I give them first in their original form – as published – and then rewrite them to suggest how they can be made easier to follow.

The convention I have used here to anonymize these examples is to give the surnames of the authors of the original papers as A, B, C, etc, and then to use D, E, etc, for the surnames of other authors that they may cite in addition to themselves.

#### Category 1: Where the single author cites himself/herself/others

##### *One (A) cites himself/herself (A)*

*Original:* In an earlier study A reported that in most respects the writing of men and women did not differ (A, 2006).

*Revised to be easier to follow:* In an earlier study I reported that, in most respects, the writing of men and women did not differ (A, 2006).

*Original:* A (2004) proposed that the interplay of student motivation and identity development could be usefully understood within a co-regulation (CR) model. *Co-regulation* refers to the relationships among cultural, social, and personal sources of influence that together challenge, shape and guide (“co-regulate”) identity (A, 2004).

*Revised:* In an earlier paper I proposed that the interplay of student motivation and identity development could be usefully understood within a co-regulation (CR) model. *Co-regulation* refers to the relationships among cultural, social, and personal sources of influence that together challenge, shape and guide (“co-regulate”) identity (A, 2004).

##### *One (A) cites another (B)*

*Original:* A has spent more than thirty years trying to locate B’s (1926) raw character scores.

*Revised:* I have spent more than thirty years trying to locate B’s (1926) raw character scores.

**One (A) cites two (A and B)**

*Original:* In an earlier study A and B reported that men's writing was more adventurous than that of women (A and B, 2001).

*Revised:* In an earlier study my colleague James B and I reported that men's writing was more adventurous than that of women (A and B, 2001).

**Category 2: Where pairs of joint authors cite themselves (separately or together) and others****Two (A and B) cite one (A or B)**

*Original:* A downloaded 100 abstracts from 53 journals published in the social sciences between January and November 2008.

*Revised:* The first author downloaded 100 abstracts from 53 journals published in the social sciences between January and November 2008.

*Original:* Using this overview as a guide, the interviewer (A) then concentrated on particular stages in their biography.

*Revised:* Using this overview as a guide, the interviewer (initials of A) then concentrated on particular stages in their biography.

*Original:* The overall structure of the *Handbook* follows the analytical framework developed by B in her article (title) published in (journal reference). This structure reflects B's scholarly analysis of the wide range of work undertaken...

*Revised:* The overall structure of the *Handbook* follows the analytical framework that the second author devised in her article (title) published in (journal reference). This structure reflects her scholarly analysis of the wide range of work undertaken....

**Two (A and B) cite two (B and C)**

*Original:* However, the previous interests of the project directors were also a factor. B and C (1998, 2001) had researched and theorised classroom assessment in primary schools.

*Revised:* However, the previous interests of the project directors were also a factor. One of us, together with C, had already researched and theorised classroom assessment in primary schools (B and C, 1998, 2001).

**Two (A and B) cite one, two, three and four authors (A, B, C, and D)**

*Original:* One of the guest editors (A) was undertaking his graduate work in the United States within the motivational framework of Achievement Goal Theory ... the young scholar was again fortunate to find a willing colleague in the other guest editor (B) – an identity researcher ... already engaged in exploring theoretical processes between identity processes and motivation (e.g., B and C, 2000). The collaboration has been fruitful, leading ... to a study

... (A, B and D, 2009), and to the theoretical exploration of the relationships between achievement goal orientations and identity formation processes (B and A, 2006; A and B, 2009).

*Revised:* One of us (James A) was undertaking his graduate work in the United States within the motivational framework of Achievement Goal Theory ... the young scholar James was again fortunate to find a willing colleague in the other guest editor (John B) – an identity researcher ... already engaged in exploring theoretical processes between identity processes and motivation (e.g., B and C, 2000). Our collaboration has been fruitful, leading ... to a study... (A, B and D, 2009), and to theoretical explorations of the relationships between achievement goal orientations and identity formation processes (B and A, 2006; A and B, 2009).

**Category 3: Where three joint authors cite themselves (separately or together) and others****Three (A, B, and C) cite one (A or B or C)**

*Original:* C (2007) also argued that ... (C, 2007)

*Revised:* One of us also argued that ... (C, 2007)

*Original:* In particular, this study aims to verify if the higher average male performance in research, revealed by A et al. in a preceding study (2007), can be largely ascribed to the subpopulation of the academic universe known as "star scientists".

*Revised:* In particular, this study aims to verify if the higher average male performance in research that we found in earlier study (A et al., 2007), can be largely ascribed to the subpopulation of the academic universe known as "star scientists".

**Three (A, B, and C) cite two (A and B, or B and C, or A and D, etc)**

*Original:* However, respondents also commented on the review's inappropriate methodology, emotional tone, and biased use of language (A, 2004; D, 2006; E, 2005). B (2007a, b, c) also argued ....

*Revised:* No changes suggested.

**Three (A, B, and C) cite two and three (A, B, and D)**

*Original:* Many judges have difficulty in identifying methodologically-flawed expert testimony (B and A, 2000a). Attorneys also struggle to evaluate effectively expert evidence (B and A, 2000b). Their ability to make and successfully argue ... may be limited as a result (B, D, and A, 2002).

*Revised:* No changes suggested.

**Three (A, B, and C) cite three (A, B, and C)**

*Original:* In an earlier article A, B and C (2008) described the results of 2,324 surveys from 206 usability tests collected over a ten-year period.

*Revised:* In an earlier article we described the results of 2,324 surveys from 206 usability tests collected over a ten-year period (A, B and C, 2008).

*Three (A, B, and C) cite three (A, B, and D)*

*Original:* For an exhaustive description of the development and function of the observatory and the listing of the names for Italian research workers, see A et al. (2007).

*Revised:* A, B and D (2007) provide an exhaustive description of the development and function of the observatory and the listing of the names for Italian research workers.

### Concluding remarks

The most irritating original examples occurred – in my view – when a single author used his or her surname to refer to an earlier publication within, rather than at the end of a sentence. However, these sentences were the easiest to revise. I found it much more difficult to change the text when I got into the realm of trying to cite the authors of a relevant publication when a mixture of the original and different authors wrote the initial text. There may well be a case here for citing particular authors' surnames to ensure that it is clear which of them are involved in a particular citation.

Of course, this discussion only applies to papers published in the name/date (APA) style – where the names of the cited authors are given in the text. Using the MLA style (or similar), as here – where the citations are numbered in the text and the reference list – may make things more complicated for the reader when the original authors cite articles with two or more additional authors.

I should perhaps conclude by observing that Smyth has rather softened his views with the passage of time. In 2004, he wrote:

When writing a paper the use of first person personal pronouns is usually superfluous ... Sometimes students use *the writer* or *the author* to avoid using the first person personal pronoun *I*. This style should be avoided because it divorces the author from his or

her paper. A similar problem sometimes arises when authors refer to themselves when citing themselves as a source ... The same reasoning applies to the use of the first person plural pronouns *we* and *our*... (pp 17-18).<sup>10</sup>

Taking this further, it is interesting to note that Hyland, using a much larger corpus of examples than the present study, found greater variation in the use of personal pronouns within disciplines rather than across them, suggesting that this “may be an area where experienced users of the genre may be permitted a degree of freedom to manipulate discourse conventions”.<sup>11</sup>

I am indebted to Marcin Kozak for helpful suggestions during the writing of this paper.

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### AGM speaker Philip Campbell, Editor-in-Chief of *Nature*

Philip Campbell began working at *Nature* in 1979 and was appointed physical sciences editor in 1982. In 1988 he left to start *Physics World*, the membership magazine of the Institute of Physics, returning to *Nature* as Editor-in-Chief in 1995.

He heads a team of about 90 editorial staff around the world, ensuring that quality and integrity of *Nature* publications are maintained. He takes direct editorial responsibility for the content of *Nature's* editorials, writing some of them.

After degrees in aeronautical engineering and astrophysics, Philip Campbell's doctoral and postdoctoral research was on the physics of the ionized upper atmosphere and effects on radio propagation. He is a Fellow of the Royal Astronomical Society and of the Institute of Physics, and has been awarded honours by several universities.

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## Editing around the World

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### Brazilian medical journals are now facing a big challenge

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A significant proportion of the medical research produced in Brazilian institutions by national investigators comes from PhD and postdoctoral programmes across the country and is published in Brazilian medical journals. A federal government agency named CAPES (Coordenação de Aperfeiçoamento de Pessoal de Nível Superior) regulates (and rates) these programmes. Scientific productivity is analyzed both quantitatively and qualitatively. The federal government – the hand behind CAPES – provides academic and financial support based on performance during the previous three years.

CAPES elaborates the criteria for scientific and academic productivity that are used to rate Brazilian graduate programmes, and these are also based on a three-year performance model. Other factors taken into consideration include international collaboration, patent registrations, production of software for medical education and patient monitoring, the encouragement (and teaching) of graduate students in the first steps of research, and also initiatives directed towards the improvement of social, cultural, and economic aspects of the general population. Together these factors qualify this system as a reference guide for the next generation.

#### Impact factors are critical

A new set of criteria for rating periodicals has been posted for the 2010 evaluation procedure. For scientific journals, eight levels of rating have been established. The top four are rigidly linked to impact factor, and to nothing else. This has led to concern and complaints from the editors of Brazilian medical journals that the newly adopted cut-off values predict a cloudy future for all Brazilian journals.

The counter-argument from CAPES is an old – *déjà vu* – claim that editors and journals should plan to improve their impact factors. However, since the first official announcement of the new criteria, editors of Brazilian medical journals have disagreed with this view: the entire collection of Brazilian periodicals is excluded from the top two levels, and only three medical journals are included in the third rank. Even though at least 20 Brazilian medical journals have been included by ISI during the last three years, the fact that none of them has a posted impact factor means that none of them qualifies for the top four of the eight ranks.

Editors argue that, especially for Brazil, impact factor is not a realistic variable with which to evaluate and compare

journals. There is a consensus that the culture of citation has been designed in the first world and that many elements – citation tracker, references organization, referees and reviewers search, keywords-based reference search systems – are not appropriate for developing countries. There is also a clear differentiation between English-speaking and non-English-speaking first-world countries. French and Italian journals fare poorly in comparison to equivalent collections from English-speaking countries. To Brazilian editors it seems very clear that English-speaking editors and researchers – and, unfortunately, many of our own authors – are blind to our publications and do not cite or even read them.

Splendid initiatives such as SciELO ([www.scielo.br](http://www.scielo.br)) have been able to mitigate the chasm between English speaking and Latin-American – and Iberian – journals. There is no question that SciELO is mainly responsible for the observed upgrade of Brazilian journals. In addition, a very substantial effort has been invested in journal visibility and accessibility, including simultaneous complete online English versions, reduction of the decision process time, internationalization of editorial boards, indexing, and, of course, a determined effort to increase impact factor by strictly ethical means. However, as of February 2010, four Brazilian journals (three of them medical) have impact factors (2008) greater than 1.0, while several others are expected to reach this level later in the year, when the 2009 impact factors are published. Even though these new impact factors refer to the three years under evaluation (articles published in 2007 and 2008, cited in 2009), it seems that CAPES will simply ignore these in its 2010 evaluation.

#### Modified criteria

It thus appears that all of the Brazilian players in journal production are doing their best, and that the foundation for a competitive set of Brazilian journals has been created. What, then, is the problem? The trouble is, as mentioned above, that the CAPES criteria were recently upgraded and Brazilian medical journals were correspondingly downgraded.

There is a lot to be said in favour of modifying the CAPES criteria. As a consequence of the improvement of the quality of Brazilian graduate programmes, it was certainly necessary to increase the cut-off value for the impact factor in order to better differentiate and classify these programmes in terms of the quality of their publications.

From the data collected it was found that many programmes had more than 50% of their scientific output published in journals with the highest rank according to the old CAPES cut-off points.

All players agree that a new set of cut-off values was needed in order to generate a more discriminating scale for the evaluation of Brazilian scientific production. However, virtually all journal editors and many of the graduate programmes believe that the increased cut-off was excessive. The new cut-off values are based on the median of the impact factor of all world journals and create eight ranks: A1, A2, B1, B2, B3, B4, B5, and C. If the median had been used, all players would have been happy and a far more discriminating set of cut-off values would have been created. Unfortunately, the median was understood by CAPES as excessively permissive. Consequently, CAPES introduced a percentage increase to the median, apparently disregarding the fact that a percentage rise of the median would generate a cut-off point that includes a very small fraction of journals in the top (A1) category. For instance, cut-off impact factor values for A1 of clinical subject categories was raised from 1.0 to 4.2, a value that includes only 10% of all the ISI-listed journals.

In addition, an equivalence factor was created – applicable to the four higher levels. According to this factor, a number of articles published in journals ranked in the lower levels would be equivalent to a smaller number of articles published in journals ranked in the higher levels. Therefore, for example, two articles B1 would be equivalent to 1.2 article A1; one article B1 + one article A2 would be equivalent to 1.4 article A1; three articles B2 would be equivalent to 1.2 article A1.

But the true cornerstone of the disagreement is to consider the Journal Citation Report impact factor as the only and universal index to assess the quality of the journals. AMB, the Brazilian Medical Association, publisher of *Revista da Associação Médica Brasileira* ([www.ramb.org.br](http://www.ramb.org.br)) organized a series of meetings which provided an in-depth discussion of the problem and pointed out the major constraints in the relationship between editors, authors, and CAPES.

#### **Box 1. Suggestions from the WAME internet list**

- Protest against classifying journals only by impact factor
- Consider data in terms of how many times an article has been read or downloaded
- Lobby at a regional level to ignore impact factor as an assessment criterion
- Aim to publish articles that are useful to Brazilian doctors regardless of impact factor
- Publish fewer citable articles but ensure that those published are of excellent quality
- Publish more comprehensive review articles which will increase impact factor
- Avoid boosting a journal's impact factor at any level (country or editorial)

#### **Box 2. Improving the evaluation and development of Brazilian medical journals**

- Qualitative analysis of Brazilian journals should be carried out; it should include more than just impact factors published by the JCR
- The specific characteristics of each area of interest or each specialty should be taken into consideration and respected
- The Brazilian publishing industry (unlike those in other countries) is supported by public and private universities and scientific associations
- Strong stimulation of citations made in the source of scientific production – namely, PhD programmes. For example, those programmes classified at the top should, in addition to being required to have a percentage of articles published in journals with high impact factor, should also have a percentage of articles published in Brazilian journals
- An editorial signed by more than 50 editors should be written and forwarded to investigators, authors, editors, and institutions

In addition, we asked members from WAME (World Association of Medical Editors) to give their opinions on this situation and hopefully show previous successful experience, if any. The WAME list initiative brings out some interesting points and main results are listed in box 1.

WAME could lead an initiative, together with ICMJE and CSE, to persuade the relevant government officials to drop the requirement of publishing in journals with higher impact factors.

At the meetings sponsored by AMB it was clear that for both editors and CAPES the health of Brazilian journals is important for the scientific growth and development of Brazil. To stimulate this virtuous circle, it is necessary to foster growth and development of the culture of citation in Brazil. This means: facilitating and increasing the citation of articles published by Brazilian authors; intensifying the efforts of editors, reviewers, and authors to increase the quality of articles; ensuring that governmental agencies – especially CAPES and the Brazilian Federal Research Council – provide adequate support for the management of the financial resources and qualitative classification of journals. The main results and ideas of these meetings are depicted in box 2.

The Brazilian Association of Scientific Editors (ABEC) devoted a lot of time to these concepts during its national meeting of scientific editors in November 2009. Members of the staff of CAPES and editors of all areas of scientific knowledge held long discussions on this topic. The suggestions disclosed above were delivered and discussed. A proposal for a periodical evaluation of the entire system was produced and will be sent to all of the Brazilian sponsoring agencies.

In the meantime, all participants should consider making their contributions to the formulation of solutions to overcome this publication obstacle.

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## Viewpoints

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### Acknowledging the funders of research

Most major funders of research, in the UK as elsewhere, include in the terms and conditions of their grants a requirement for researchers to acknowledge in any publication the support they have received from the funder in question. Similarly, many publishers include provision in their XML protocols for the acknowledgement of funders.

The problem has been that, at least until recently, there has been no standard wording or guidance from funders on the form that the acknowledgement should take; and from publishers there has been no consistency in the tagging of the information that authors provide. The result is that it is difficult for funders to discover what publications have arisen from the research they have funded, and for publishers to provide relevant information to readers in a consistent way.

With a view to tackling these deficiencies, in 2007 the Research Information Network (RIN) convened a working group of representatives of major funders of research and publishers based in the UK. The key aim was to find a way of enabling both funders and publishers to link journal articles to specific funders and specific research grants. The result of their work was a brief report published in 2008, *Acknowledgement of Funders in Scholarly Journal Articles*,<sup>1</sup> with guidance for research funders, authors, and publishers.

The working group did not tackle all the issues that may arise in seeking, for instance, to establish linkages between publications and funders' databases of grants. Rather, it adopted a pragmatic approach, seeking to provide a simple way of ensuring that published journal articles are, wherever possible and appropriate, accompanied by standardized, high-quality metadata providing information about the organizations that funded the research. Essentially, the report provides two things: first, a standard sentence for the acknowledgement of the funder, named in full and with a grant number reference included; and second, recommendations to publishers as to good practice in the tagging of that information. The Research Information Network also provides a list of the authorized titles of all the major UK funders on its website ([www.rin.ac.uk](http://www.rin.ac.uk)).<sup>2</sup>

The guidance was endorsed in the UK by the Research Councils, the Wellcome Trust, and the Association of Medical Research Charities. Within a year, all of the major UK research funders amended their terms and conditions

of award to require researchers to use the standard recommended sentence. The guidance was also endorsed by the UK Publishers Association and the Association of Learned and Professional Society Publishers. Progress among publishers has been slower. A small survey of a range of UK-based publishers in 2009 showed that only a third require authors to provide an acknowledgement, with another fifth leaving the decision up to the editors of individual journals. Moreover, of those publishers that did require acknowledgement, only a minority had a procedure to check that one is actually provided, or provided a standard format for the acknowledgement, or tagged the acknowledgement in any way. Only two publishers were following anything like the Research Information Network's guidance.

The working group recognized that adoption and implementation of the guidance would take an extended period of time, as publishers amend their policies and, in particular, their DTDs. It is clear, however, that there is work to be done before the guidance achieves a wide degree of take-up.

The working group also acknowledged that both research and publication are international in scope, while its guidance is targeted at researchers and publishers in the UK. The Research Information Network has been in discussion with the International Association of Scientific, Technical and Medical Publishers about ways in which the guidance might be made more international in scope, and we hope to issue a revised, international version of the guidance in 2010.

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### Major editorial guidelines need to be translated and popularized

For over 15 years, I have been a freelance translator and copy editor, cooperating with Polish biological journals published in English. I have edited manuscripts submitted by authors from various countries. In my opinion, a major problem for the integrity of scientific publications is that many scientists are not fluent in English and are not

aware of the standards of scientific communication in this language. Consequently, those scientists are often unable to comply with the standards set by international journals and to communicate their research results effectively in English.

Every day we experience situations showing that human communication is inefficient, often leading to

misunderstanding. This inefficiency and misunderstanding is increased when people from different cultures try to communicate. Non-native speakers of English make many typical errors in scientific writing – for example, they commonly write complicated sentences and then translate them literally, preserving the original sentence structure, so the outcome is incomprehensible. Moreover, they often do not realize that there are semantic differences between words that seem to be equivalent (the so-called false friends). Numerous manuscripts contain long digressions or repetitions, but fail to provide important details: these may be acceptable in some cultures but have no place in modern English science articles.

Many authors ask translators for help, but in all too many situations the translators have insufficient scientific knowledge to convey the message properly.<sup>1</sup> The authors lack sufficient knowledge of English to be aware of this, which closes the vicious circle. Journals publish guidelines for authors, and these may be short or extensive. Short instructions assume authors have experience in preparing scientific papers. Long instructions are hard for authors whose English is poor to understand. In both cases, authors may submit poorly prepared articles that require frequent revisions, most of which are concerned with the writing style and organization of the paper rather than the science itself. This wastes the time of authors, editors and reviewers.

For these reasons, I came to the conclusion that in order to promote research integrity successfully, we need to publicize the most important editorial guidelines for authors and translators of scientific articles to be published in English in many different languages. Authors and translators should fully understand and be familiar with the guidelines before they start writing.

Popularization of the guidelines can bring many benefits:

- researchers will understand editors better and spend less time on revising manuscripts;
- translators will be able more effectively to translate/edit scientific texts to be published in English;
- science editors will have more time to focus on the accuracy and scientific validity of submissions;
- editors and translators will be able to refer to the guidelines if authors object to their corrections (this is

particularly important in countries where editors are not duly respected).

All this should facilitate and standardize the editorial process – consequently, international scientific communication will be more efficient. Last, but not least, the visibility of EASE will increase, particularly in non-Anglophone countries, giving more editors the opportunity of joining and of benefiting from all EASE's educational and networking activities.

In April 2009, I presented draft guidelines for authors on the EASE Forum. Some EASE members discussed the draft on the Forum, and we continued the discussion at the EASE conference in Pisa. All comments were very welcome and appreciated. The final version of the guidelines was approved by the EASE Council in February 2010. These are now displayed on the EASE website.

We have started to send links to the guidelines to scientific institutions worldwide and to promote the guidelines in the scientific community in other ways. All EASE members can help in their popularization, by including links to the guidelines on their journals' websites and asking authors to read the guidelines before submission. In the future, feedback from the scientific community may aid in refining and updating the guidelines. We are also planning to add appendices and useful links to the guidelines, to explain selected issues more precisely (within the Author Toolkit being prepared by EASE).

The next step is for these guidelines to be carefully translated into other languages. I encourage all volunteers interested in participating to get in touch with me. I would like to thank everyone who has contributed to the guidelines or supported this idea in other ways. I hope that you will continue to support the project and help to popularize the EASE guidelines in your countries. This will be crucial for their effectiveness.

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## Effective presentation of data

In this age of mass communication and debate of complex scientific issues, it is vital to present data clearly, accurately, and so to provide maximum impact. Ehrenberg claimed almost 30 years ago that most data were badly presented,<sup>1</sup> and his words are just as relevant today. A common fault is the use of too many digits in tabulated data, which makes the numbers difficult to compare. For example, the three decimal digits in 214.465 and 241.645 neither facilitate comparison of the two numbers nor take any part in it; these are non-effective digits for this comparison. Ehrenberg therefore proposed that all numbers for presentation be rounded to just two or three effective digits – that is, those governing the major variation in the data.<sup>2</sup>

Sometimes, however, more exact values are desirable, for example when presenting data on official statistics, but such data are difficult to assimilate when they consist of numbers with many digits. To provide a clear data presentation and precise reporting of numbers, we need to emphasize the most important digits while still presenting some of the less important digits, albeit in a less prominent way.

The method we propose involves two font amendments: to reduce the size of the less important (“minor”) digits while keeping unchanged the size of the most important, (“major”) digits; and to italicize the minor digits. Font reduction has been used to present standard errors and confidence intervals<sup>3,4</sup>; we are here extending and

strengthening it for more general comparison of data values. Consider the four numbers in column on the left below, which we wish to represent using two major digits. The middle column shows the minor digits with reduced font, and the column on the right shows them with both reduced and italicized font.

987.2	987.2	<i>987.2</i>
3563.2	3563.2	<i>3563.2</i>
3599.9	3599.9	<i>3599.9</i>
4563.0	4563.0	<i>4563.0</i>

We believe that italicizing minor digits improves the presentation because of the additional distinction between the minor and major digits, so we consider this to be the optimal presentation. Note that “major digits” include any leading zeros implied by the size of the other data values. Thus 987.2 is in effect 0987.2, so has just the one major digit 9 – but had all data values consisted of three non-decimal digits then it would have the two major digits 9 and 8.

There is a particular advantage when there are no non-zero major digits, as for example with the two numbers: 0.099 and 0.012. Since the difference between the numbers lies in the minor digits, for comparison purposes the values may be considered roughly equal. However, they both differ from zero, and this information is easily caught by eye from the minor digits. We believe this to be preferable to rounding, in which for example 0.051 and 0.049 would respectively become 0.1 and 0.0, suggesting a bigger difference than actually exists.

By way of illustration, consider the populations of Poland’s provinces on 30 June 2007, taken from the Polish Central Statistical Office web page (<http://www.stat.gov.pl>), shown in the table below. The values are presented in four ways: as exact numbers; in millions rounded to two decimal digits; in millions presented with three major digits (Version 1 of the proposed technique); and in millions rounded to two major and two minor digits (Version 2).

The numbers in column 1 are large, and difficult to

compare, despite being arranged in decreasing order. The values in column 2 are much better for comparison purposes, but up to 5000 units of information on the populations can be lost (4942 units for the Łódzkie Voivodship, for example). Up to twice this amount of information can be lost when comparing two numbers (7926 units for the difference in the populations of the Łódzkie and Pomorskie Voivodships). Note that using only two effective digits (as Ehrenberg claimed was best<sup>2</sup>) gives potential losses of 50,000 units in a single number and 100,000 units when comparing two numbers! We would therefore favour using Version 1, as this is the presentation that provides exact values while still facilitating the number comparison, but for a simpler summary Version 2 would be acceptable.

There is, of course, much room for subjectivity with any visual presentation, but we would argue that there are better ways of presenting data than just by rounding values, and what we propose is one possible way of doing this.

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Province	Population				
	In units	In millions	Using technique proposed (in millions)		
			Version 1	Version 2	
Mazowieckie	5,170,786	5.17	5.170786	5.1708	
Śląskie	4,662,615	4.66	4.662615	4.6626	
Wielkopolskie	3,377,725	3.38	3.377725	3.3777	
Małopolskie	3,260,358	3.26	3.260358	3.2604	
Dolnośląskie	2,877,519	2.88	2.877519	2.8775	
Łódzkie	2,565,058	2.57	2.565058	2.5651	
Pomorskie	2,202,984	2.20	2.202984	2.2030	
Lubelskie	2,175,325	2.18	2.175325	2.1753	
Podkarpackie	2,106,259	2.11	2.106259	2.1063	
Kujawsko-Pomorskie	2,066,440	2.07	2.066440	2.0664	
Zachodniopomorskie	1,691,059	1.69	1.691059	1.6911	
Warmińsko-Mazurskie	1,429,670	1.43	1.429670	1.4297	
Świętokrzyskie	1,285,101	1.29	1.285101	1.2851	
Podlaskie	1,197,483	1.20	1.197483	1.1975	
Opolskie	1,038,204	1.04	1.038204	1.0382	
Lubuskie	1,009,381	1.01	1.009381	1.0094	

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## Reports of Meetings

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### Crystal ball gazing: the future of medical publishing

Oxford, 10 February 2010

Medical communications professionals from around Oxford gathered at the hugely successful regular networking event hosted by Peter Llewellyn of NetworkPharma. This particular event attracted rather more than the usual number of attendees to hear Richard Smith (former editor of the *BMJ* and now editor of *Cases Journal*) share with us his vision of “The Future of Scientific Communication” – or as it was billed, “a spot of crystal ball gazing”.

For those of you who have not had the privilege of hearing Richard speak, he is highly entertaining, managing to weave together hard facts and comic anecdotes seamlessly, and I can honestly say that he kept his audience fully engaged throughout.

He started by pointing out the inherent difficulties in looking to the future – many things just cannot be predicted, others are predicted wrongly, and events that no one ever considered do happen. For instance, Lord Kelvin, president of the Royal Society in the 1890s, predicted that radio would have no future, and no one could have foreseen the tragic events of 9/11. Richard expressed that this failure to correctly predict future events could be due to our tendency not to consider possibilities in the context of their associated probabilities, and also to our inclination to think linearly – extrapolating in only one direction. Perhaps we all need to develop our imaginations to their full and four-dimensional potential?

*The future belongs to the unreasonable ones, the ones who look forward not backward, who are certain only of uncertainty, and who have the ability and the confidence to think completely differently.* (Adapted from George Bernard Shaw)

Of course, the whole point of conjecture is not simply to know what might happen, but rather to be prepared for whatever comes, and also to be able to influence the shape it takes.

How does this apply to the world of medical publishing? We are living in an age in which information is being accumulated at a phenomenal rate, so fast in fact that we cannot use it effectively – a quote from Al Gore sums this up rather nicely: “Our current information policy resembles the worst aspects of our old agricultural policy, which left grain rotting in thousands of storage silos while people were starving. We have warehouses of unused information rotting while critical questions are left unanswered and critical problems are left unresolved.”

Is this a simple case of information overload, or lack of good publishing practice?

Richard described the effects of the information paradox in the field of medicine. For instance 40% of doctors read 1–10% of all the medical information they are bombarded with, and a further 40% read 11–50%. A shocking 8% read

less than 1%. When asked how their information supply makes them feel, a sample of 41 doctors almost all gave negative answers, with “impossible” and “overwhelming” heading the list of adjectives.

Clearly, there is something wrong with the way that medical information is offered to those who need it. Among the criticisms of medical journals were that they are “too much rubbish”, “boring”, “expensive”, “biased”, “pompous”, “awful to look at”, “don’t add value”, and “slow everything down”. Perhaps most damning was the comment: “and anyway, nobody reads them”. Clearly the system is broken, and we need to fix it. But how?

Overcoming resistance to change – challenging the status quo – is an issue, so drivers for change need to be identified. Not least, there needs to be a vision of something better, itself driven by the failures of the present system, such as slowness and Balkanisation of the literature, and of course there is the usual suspect – money.

Open access advocate Stevan Harnad has such a vision – a resource utopia: “It’s easy to say what would be the ideal online resource for scholars and scientists: all papers in all fields, systematically interconnected, effortlessly accessible and rationally navigable, from any researcher’s desk, worldwide for free.”

Richard has his own vision of the future of publishing, the overriding themes being accessibility and transparency. Scientific papers should be posted on the web in databases, with the raw data being fully accessible; the software used to analyse them should be named; and full use of multimedia should be possible. Instead of blinded peer review, an open system should be adopted, with post-publication discussion rather than a “filter and publish” system. And everything should be open access. These principles should be applied also to clinical trials, where increased transparency would almost certainly result in reduced bias and more rapid dissemination of the data.

Far-reaching visions indeed.

Richard’s last slide presented a sobering thought: “When the future comes through you’re either part of the roller or part of the road.”

I know which I would prefer. What about you?

**Moir Johnson**

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For more details about MedComms Networking, please contact Peter Llewellyn via <http://www.medcommsnetworking.co.uk>

To download Richard’s presentation (not *entirely* plagiarised in this brief report) go to [http://www.medcommsnetworking.co.uk/docs/smith\\_100210.pdf](http://www.medcommsnetworking.co.uk/docs/smith_100210.pdf)

## Science reporting: is it good for you?

Royal Institution, London, 16 September 2009

Science is badly reported in the British media. This was the subject of a debate at the Royal Institution on 16 September 2009. There to defend science reporting was Lord Drayson, the science minister, and his opponent was Ben Goldacre, author of *Bad Science*. The debate arose from a Twitter exchange between Lord Drayson and readers of *Times Higher Education*, who took exception to his claim at the World Conference of Science Journalists that UK science journalists were “the best in the world”.

Lord Drayson started off the debate by praising science journalists, who, he feels, are doing a great job. He said that a lot has improved since the controversy about whether the triple vaccine against measles, mumps, and rubella is associated with autism. We only need to look at the great coverage of swine flu and the Large Hadron Collider this year compared with MMR, BSE, and GM foods to see how much science reporting has changed. It is crucial not to rubbish all science reporting and focus only on the bad because scientific journalism is hugely important to the health and happiness of our country. People’s readiness to accept changes and new technologies is affected by their trust in science reporting. Lord Drayson also spoke in support of sensationalism, as long as it is accurate. It is what grabs people’s attention and puts science on the front pages, he thinks.

Ben Goldacre’s argument was that a problem does not go away just because you pretend that it’s not there. Although he accepts that much has improved, the media are still full of scare stories and dodgy scientific journalism. This is eroding the public’s faith in common-sense health advice. He sees this as a systems problem because journalists

feel pressurised into writing stories they don’t want to, headlines are written by someone other than the author of the story, and press releases are often inaccurate. He argued that we need more “nerd capital”, by which he meant more facts, figures, and accurate data. He said he was jealous of sports fans who have reams and reams of information available to them about their sport of interest. What we need is more intellectual information available about science, he added.

Both speakers agreed that scientists themselves need to become more involved in science reporting. They spoke of a duty that scientists have to communicate the results of their publicly funded research. Ben Goldacre also pointed out that there are many people writing and blogging outside the mainstream media who are making very valuable contributions to scientific reporting, and who deserve more recognition, not least because many of them have a large readership. Technologies such as Twitter, YouTube, and blogs should be used by scientists to engage with the community. The fact that this debate took place as a result of a Twitter exchange, and was broadcast live online, shows the value of new forms of communication.

Overall I was more convinced by Ben Goldacre’s argument. There was no vote at the end, but the webcast is still available at <http://www.timeshighereducation.co.uk/webcast.html>, so you can make up your own mind as to who was most persuasive.

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## Measuring the health of science journalism

City University, London, 31 March 2010

“Science in the Media: Rude or Ailing Health?” compared the role of mainstream science journalism with that of blogs and other forms of science communication.

The aim of the event was to deliberate the findings of a report by the Science Media Centre and the Department for Business, Innovation and Skills: *Science and the Media: Securing the Future*.<sup>1</sup> Encouragingly, the expert working group behind the report “found more reason to champion specialist science reporting in the UK than to despair” and “judged science in the media to be in rude health”. Nevertheless, the report makes various recommendations on how to improve science journalism in the key areas of scientific training, science broadcasting, openness and transparency, and future science journalism.

The report didn’t involve a full public consultation, so this debate was a chance for comment. On the panel were Natasha Loder, science and technology correspondent for the *Economist*; Andrew Jack, pharmaceuticals

correspondent for the *Financial Times*; Ed Yong, author of the blog Not Exactly Rocket Science; and Fiona Fox, director of the Science Media Centre and author of the report.

Fiona Fox outlined the findings of the report—overall, the state of science journalism is good. The public has a huge appetite for stories on science, and plenty of good journalism is around to feed it. Also, editors now often defer to science reporters on specialist stories.

However, science journalism is being affected by changes in the wider world of media: “journalism is in crisis and the business model is collapsing,” stated Fiona Fox. Fewer jobs are available across the media, and the journalists who are left are being asked to squeeze in more and more work, leading to a rise in “churnalism”—the act of hurriedly producing news stories from press releases and wire stories without doing further research or checking.

On the panel, Andrew Jack thought that science journalism was in good health, but there is a crisis structurally

in the media, not just in science coverage. Natasha Loder likewise felt that the “crisis” in science journalism has been overstated. For example, a study published alongside this report found that the number of full time science journalists in the UK has almost doubled between 1989 and 2009: from 43 to 82.5.<sup>2</sup> The major problem is time: workloads are rising and a fifth of science, health, environment, and technology news journalists have reported not having to time to fact check or follow up stories adequately.<sup>2</sup>

Loder then raised the issue of whether “direct to the public” outreach, such as that conducted by charities or bloggers, constitutes journalism. She was of the opinion that journalists and groups that communicate directly to the public both take part in “truth telling”, so there’s no point getting fixated on the title “journalist”.

Blogger Ed Yong pointed out that the report, crucially, doesn’t cover online media such as blogs and direct to the public communication such as that done by universities and charities. He felt that mainstream media is just one of many channels available now. We are going through a “Cambrian explosion” of science journalism, with lots of new “species”—means of communication—coming out of the woodwork.

Yong’s comments sparked a lengthy and heated debate on whether blogging and other forms of science communication outside of the mass media count as journalism. The report deliberately omitted “the explosion of direct to the public science communication by way of websites, blogging, tweeting, etc” in favour of “science communicated through journalism in mainstream media settings”, suggesting that the experts who contributed to the report, most of whom are entrenched in the traditional media, think not.

Fiona Fox resolutely did not believe that blogs should be considered journalism. She thought that the role of the journalist is to provide objective standards. The more “noise” there is on the web, the more we need objective journalists to navigate and filter the material. Andrew Jack agreed and pointed out that journalists are trained to be objective, whereas blogs grew out of opinion writing.

Ed Yong countered that blogs have been stereotyped as being opinion, not journalism. Natasha Loder made another point against traditional media: no-one can be objective. As a journalist at the *Economist*, for example, she is subject to the political leanings of her organisation.

The difference between journalism and blogging seems to be objectivity, but then the issue of credibility is brought up. Yong suggested that traditional media sources aren’t as reliable or accountable as those online: bloggers link to their sources whereas journalists don’t. For example, Ben Goldacre of the Bad Science blog has been campaigning to get BBC News to provide hyperlinks from science and health stories to the source research,<sup>3</sup> but has been met with resistance so far.<sup>4</sup> Blogging encourages a culture of investigation and scrutiny, whereas the mainstream media works from a top-down authoritative standpoint.

Unsurprisingly, the people in the room who had made their careers in the mainstream media tried to defend the exalted position of journalists in the new science media ecosystem, whereas those who worked online argued that

other approaches should also be considered journalism. One of the more open minded was the *Guardian*’s science and environment correspondent, Alok Jha. He cited *Guardian* editor Alan Rusbridger’s Hugh Cudlipp lecture,<sup>5</sup> in which Rusbridger talked about “mutualisation” of the media. The mainstream media could improve stories by communicating and collaborating with the audience on social media such as Twitter. Journalists would still be needed, but as “gate keepers,” guiding audiences to interesting writing and mediating their responses. In the realm of science communication, the mass media can direct people who aren’t into science towards science blogs.

The fact that the Science Media Centre report ignored any forms of communication that weren’t mainstream press or broadcast was rightfully a point of contention in this debate. Furthermore, it got a bit tiring hearing people from traditional media trying stubbornly to defend their turf from other forms of communication, rarely willing to concede that other approaches might likewise be skilfully disseminating science news.

As far as I’m concerned, whether blogs “count” as science journalism is a bit of a moot point. Anyone interested in science and science communication should just care about getting information into the public domain in a clear and accurate way; the medium they use isn’t so important.

Science in mainstream media seems to be suffering, thanks not to shortcomings of professional journalists and their reporting but as a result of wider changes in the media, whereas other forms of science coverage, mostly online, are thriving. Overall, science in the media seems to be doing fine and, more pertinently, science *communication* seems to be growing exponentially thanks to the internet.

A version of this article was first published on [bmj.com](http://bmj.com).

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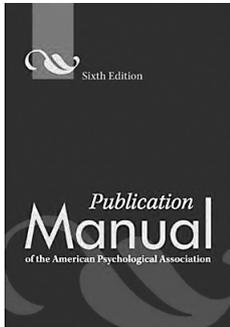
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## Book Reviews

**Publication Manual of the American Psychological Association. 6th edition. M L Skutley, G R VandenBos. Washington DC: American Psychological Association, 2010. £23.50. ISBN 13: 978-1-4338-0561-5.**



The introduction to this 272 page book describes its growth and evolution from its seven page ancestor, which appeared in 1929, as “of necessity”. It states that since the 5th edition was published (in 2001, but I could not find the publication history in the volume, and had to search online to find out) the population had changed from one that “reads articles” to one

that “consumes content”. Oh, brave new world!

This is a comprehensive manual. It occurred to me that it might enable a visiting Martian, who had never read (or consumed) a scientific paper, to write one. But I am not sure that Earth people in that situation should be called upon to write them. Nevertheless, everyone has gaps in his/her knowledge, and perhaps the provision of a detailed set of instructions is the only way to ensure that all permutations of ignorance are catered for.

The book, though many will dip in to check on style rather than read from cover to cover, follows a format that almost allows it to be used as bedtime reading. It starts with the range of articles that might be written and the ethical and legal frameworks in which they are conceived and goes on to describe the structure of articles, with 20 pages of examples of different parts of the manuscripts. This is fine, but would-be writers have for many years been advised simply to examine the publication for which they wish to write and to present their manuscripts accordingly.

Chapter 3, “Writing clearly and concisely”, is a pleasure to read. Its advice, if heeded, would make the editor’s job so much easier. I looked in vain in the subsequent chapter, “The mechanics of style”, for advice on the use of capitals for job titles. I was taught that only God and the Queen merited capitalization, but this issue seemed not to be discussed in a chapter that otherwise seemed compendious.

In a chapter on displaying results I discovered that the components of a table, which I had used and manipulated without terms to describe them, all had names as specific and defined (and unknown to me) as the positions of fielders in a game of cricket. The book contains numerous examples of tables and figures, which would be useful in comparing one’s intended presentation with what is likely to be acceptable. (Equally, one could browse the target journal for inspiration.) The chapter ends with an appropriately stern warning against digital manipulation of images.

Crediting sources, and reference examples, make up two further chapters. There are clear and useful statements about URLs and DOIs, the second of which crept into use while I was not looking. If you have performed a piece of original research and followed the book’s advice on writing it up, you might be interested in the book’s description of the publication process. This includes an example of a covering letter to use when submitting a paper, and sections on the peer review process and copyright issues.

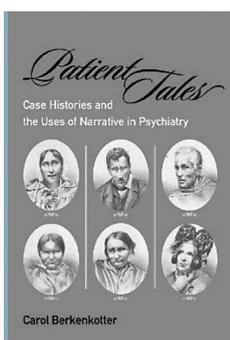
Beyond the book itself, the introduction draws attention to web-based resources at [www.apastyle.org](http://www.apastyle.org), with tutorials and other learning activities, some of which are free – for example, a slideshow entitled “Basics of APA style”, which describes manuscript structure, format, and headings; advises on use of the active voice and the avoidance of bias; and offers a guide to citation practice.

Style varies between journals and between countries. This manual is intended for those who plan to write for journals of the American Psychological Association. Some of its specific advice will be inappropriate elsewhere, but there is much within it that I found interesting and will be glad to refer to in the future.

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**Patient tales: case histories and the use of narratives in psychiatry. Carol Benkenkotter. Columbia, SC: University of South Carolina Press, 2008. 201 pages. £33.50. ISBN-13 978-1570037610.**



*Patient Tales* supplies another chapter in the evolution of the experimental research article, reporting the results of Carol Benkenkotter’s research on psychiatric case histories from their origin at the Edinburgh Medical School and the Royal Edinburgh Infirmary in the mid-18th century, to contemporary medical records in mental health clinics. It

convincingly illustrates how discursive changes occurring over time in this genre mirror evolving assumptions and epistemological commitments among those who used to be called “mad doctors”.

With her background in genre analysis and rhetoric of science, the author successfully integrates close reading (interpretative analysis) with a more systematic approach that involves analyzing narrative elements such as reported speech. The use of both techniques to analyze written texts enables her to adjust her research focus from macro (whole text/genre) to micro (grammatical, lexical, and syntactical)

levels. In an innovative and eclectic approach, the author uses techniques that range from discourse analysis to textual exegesis of primary texts, such as 19th century patient case histories and asylum superintendents' letters and diaries.

The volume is constructed in two parts. Part I (chapters 1 to 5) examines the historical, legislative, and institutional contexts of case history writing during the asylum era in Scotland and England, beginning with institutional record keeping at the Royal Infirmary of Edinburgh in the late 18th century. Part II (chapters 6 to 8) deals with the second half of the 20th century in the United States and the fate of the single subject case history during the rise of biomedicine in North American psychiatry.

In chapters 2 and 3, the author turns to the genre innovation of two of the best known British physicians in the 19th century: John Haslam and John Conolly. More specifically, chapter 2 presents a detailed description and analysis of the first book-length case history of a patient residing against his will in London's Bethlem Asylum.

Chapter 3 introduces the practice and concept of "capturing insanity". The author describes one of the earliest attempts by a Victorian psychiatrist to use an emerging technology, photography, to provide empirical evidence of the various kinds of "insanity" or mental illness. (The book cover is a reproduction of the frontispiece for an 1858 manual representing the belief that different types of insanity could be diagnosed by studying patients' facial expressions.) She reviews a series of 12 articles published by Conolly between 1849 and 1850 in the *Medical Times and Gazette*, illustrating the first examples of multimodal texts in psychiatry.

Chapter 4 provides the background and context of 19th century legislative asylum reform, resulting in a series of laws that forced asylum superintendents to standardize the form and content of asylum records. The Lunatic Act passed by the British Parliament mandated many reforms and required, among other things, all asylum keepers to keep in case books the weekly, and often daily, records of all patients, their treatment, and the course and outcome of their illness.

Chapter 5 examines the textual means through which Sigmund Freud made the psychoanalytic case history the centerpiece of his theory about the role that the unconscious plays in various neuroses (and psychoses). In her thorough reading of Sigmund Freud's *Fragment of an Analysis of a Case of Hysteria*, Berkenkotter shows how the account of Freud's famous patient Dora (a pseudonym) led to technical innovation in the genre through the incorporation of literary devices. Believing that using psychoanalytic techniques as the keys to unlock patients' memories of trauma or fantasies would lead to a cure, Freud argued that in-depth analysis of patients' utterances and the content of their dreams would lead to recovering significant memories stored in the unconscious.

Chapter 6 chronicles what happened to the published case history at the end of what Edward Shorter in his 1997 *A History of Psychiatry* called "the psychoanalytic hiatus" – the period in the 1950s and 60s when Freudian,

neo-Freudian, and psychodynamic theories became so profuse that psychiatry became a "tower of Babel". The author also refers to the publication of the *Diagnosis and Statistical Manual of Mental Disorders*, 3rd edition (DSM-III). This manual, which brought with it a new thought-style, with its subsequent revisions became the lingua franca of psychiatry and clinical psychology over the following 30 years.

Chapter 7 examines the outcome of the new reigning biomedical paradigm in the mid-1990s. Berkenkotter and her colleague and specialist-informant, Doris Ravotas, report their study conducted in the late 1990s of psychotherapists' practices in writing up the "psychosocial assessment" (also known as the "screening summary"), a text based on the first interview between therapist and client, as outpatients are now called. The "psychosocial" is the first document in the client's case history and is often based on the therapist's notes taken in session. The authors examine several clinical psychologists' uses of rhetorical devices and syntactic and stylistic features as they translate material from the session notes into the psychosocial assessment.

The closing chapter makes the case for the importance of narrative knowledge in both psychiatry and psychotherapy. Those interested in medical discourse analysis and in genre analysis in general will find the analysis of the clinical case history as a double narrative most interesting. The patient's "story", his or her narrative of personal experience, is indeed subsumed into the narrative pattern and thought-style of clinical psychiatry. From a rhetorical perspective, this narrative-within-a-narrative is noteworthy because of the linguistic and semantic devices the therapist uses to recontextualize the patient's narrative of personal experience into a more encompassing narrative framework that has been highly codified within mental health. Berkenkotter interestingly shows how, as a genre, the case history has acquired a conventional structure, style, and lexicon that, over the past 250 years, has become the standard form of reporting in clinical medicine and psychiatry.

This fascinating account of psychiatry's evolution to a knowledge-producing profession will appeal to a wide range of professionals: historiographers; medical historians; rhetoricians of science; medical anthropologists; applied and anthropological linguists; genre theorists; and psychotherapists (clinical psychologists). Some may find some chapters too detailed, but they will all undoubtedly find in this volume much more than what they are actually looking for. I highly recommend *Patient Tales*: it is a model of textual and historical research in the rhetoric of science.

A longer version of this review is available in *Ibérica* 19, 2010; reprinted with permission.

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## EASE-Forum Digest: December 2009 to March 2010

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You can join the forum by sending the one-line message "subscribe ease-forum" (without the quotation marks) to [majordomo@helsinki.fi](mailto:majordomo@helsinki.fi). Be sure to send commands in plain text format because only plain text is accepted by the forum software; HTML-formatted messages are not recognised. More information can be found on the EASE web site ([www.ease.org.uk](http://www.ease.org.uk)). When you first subscribe, you will be able to receive messages, but you won't be able to post messages until your address has been added manually to the file. This prevents spam being sent by outsiders, so please be patient.

### Editors publishing in their own journals: a good or a bad practice?

Moira Johnson asked forum participants for their views on whether it was good practice for editors to publish their research in their own journals. Was the practice viewed as indicating faith in the journal, or as an editor's attempt to enhance a personal bibliography? Should the practice be actively encouraged or discouraged?

Rhana Pike voted for discouraging the practice, as publication selection processes are anyway fairly opaque to the public. Consequently editors should send their papers to other journals and avoid the appearance of favouritism. Although Marcin Kozak thought that at first glance editors publishing in their own journal seemed somewhat unethical and editors-in-chief should avoid the practice, he queried whether editorial board members should be penalised by a publication prohibition, especially in a small field with few journals or where journals had problems finding good papers to publish and needed support from the editorial board. Either way the editorial board members would need to be treated like any other authors, or indeed be required to meet a higher standard.

The small journal perspective was considered further by Mary Ellen Kerans, who emphasised that editorial board members often represented the most active researchers in a small community, and might be the main source of submissions making their support essential for the journal's survival. The board members should also support the journal by publishing in major international journals because this might be the only source of citations for the small journal. Editors should take the trouble to make their contribution to their own journal as good as those they sent to other journals (which in her experience was not always the case), and their contributions should be critically assessed.

Angela Turner explained that the editors at her journal, *Animal Behaviour*, published both in that journal and elsewhere. Their failure to publish in *Animal Behaviour* might otherwise be seen as a lack of confidence in the journal. As the journal has a UK and a US office, editors associated with one office could submit to the other to ensure an independent decision. Smaller journals might

have more difficulties in achieving such independence. Jan Reedijk saw no reason for editors not to publish in their own journals as is universally the case in chemistry. There was always at least one co-editor who could handle the submission. Indeed there would have to be to comply with the Uniform Requirements for Manuscripts submitted to Biomedical Journals ([www.icmje.org](http://www.icmje.org)), which state that editors who make final decisions about manuscripts must have no personal, professional, or financial involvement in any of the issues they might judge. Will Hughes pointed out that where, as in his journal's case, the journal was the leader in its field, PhD students would resent being prevented from publishing in it because a co-author happened to be an editor of the journal. Electronic submission systems made it possible for co-editors to handle a submission completely blinded to the editor-author, and he felt there was a tendency to be harsher with co-editors to avoid accusations of favouritism. Both Will and Arjan Polderman recounted occasions where an editor's papers had been rejected by co-editors.

Nevertheless the practice of editors publishing in their own journal is viewed with suspicion in some quarters. Will Hughes added the observation that interview panels and promotion committees he had encountered had deleted those papers in his publications list that had been published in the journal of which he subsequently became the editor.

Between 1993 and 2008 El Naschie, editor of *Chaos, Solitons & Fractals*, published over 300 articles in his own journal. The matter came to discussion when the December 2008 issue of this Elsevier journal contained five of his articles. Philip Davis, writing in *The Scholarly Kitchen* (<http://scholarlykitchen.sspnet.org/2008/11/25/elsevier-math-editor-controversy/>), pointed out that editors who publish in their own journal are not explicitly doing anything wrong, but he questioned whether the practice broke implicit and unwritten norms in academia and could harm a journal's reputation. One possible harm could be associated with a suspicion that papers are being used to manipulate the journal's impact factor. An example is where an ophthalmology journal's impact factor jumped up 18 ranks as a result of its publication of a paper written by its editor containing 303 self-citations.<sup>1</sup> This incident resulted in some ophthalmology journals adopting a policy which prohibits editors-in-chief from being named as authors in their own journal (except book review and editorials).

### Writing Russian in English

The Cyrillic alphabet needs to be transliterated into the Latin alphabet when Russian words – authors' names and article titles, for example – are written in English journals. Sylwia Ufnalska pointed out that there are many systems of transliteration (as can be viewed at [http://en.wikipedia.org/wiki/Romanization\\_of\\_Russian](http://en.wikipedia.org/wiki/Romanization_of_Russian)). She wanted to know if the British Standard was commonly used in science. Helle Goldman replied saying that her journal, *Polar Research*, used the UNESCO system, which deviates only

slightly from the ISO system. The system works well with the original Cyrillic words, but when the author has already attempted a transliteration or has copied one from the internet, she needs help from Russian colleagues working in her institute.



PHOTO: SARAH JARMAN

Jaščerica? ... or is it ... yashcheritsa?

### Mothers-in-law causing apostrophe problems

Marge Berer was concerned not only about the view of her mother-in-law but of all mothers-in-law. She asked the forum if you should write “mothers-in-law’s views” or “mothers-in-laws’ views” or “mother-in-laws’ views”? The consensus was you should write “mothers-in-law’s views”. Curiously Norman Grossblatt had found almost the same example in the *Chicago Manual of Style* (14th ed, 1993), which states that the singular possessive is “daughter-in-law’s” and the plural possessive is “daughters-in-law’s”. But the manual recommends that the possessive of the plural of noun phrases should be avoided altogether and the construction with “of” used instead, which in our case would be “views of the mothers-in-law”. In any event the in-laws were causing too much aggro for David Mason, who mused that the French equivalent *belles-mères* (beautiful mothers) was far more charming.

To return to the apostrophe, Andrew Davis explained the rule, saying you add ‘s to make the genitive unless the word already has a terminal s, in which case you just add the apostrophe. No, said Rod Hunt, we don’t just add ‘s: “We only do this if the word not only ends in s but it also is a plural. Thus we should put ‘Andy Williams’s new single’ (not Williams’).” He regretted that this important distinction was not only being eroded but was being developed in the wrong direction, as in “Pierre Boulez’ new recording”. While Maeve O’Connor agreed that “Boulez’s” would be the correct possessive in his example, she was not convinced that “Williams’s” was correct. She pointed to *New Hart’s Rules* (2005, p 64), which states: “an apostrophe alone may be used in cases where an additional s would cause difficulty in pronunciation, typically after longer names that are not accented on the last or penultimate syllable” – to wit, Williams.

### “Risk of” vs “risk for”

Carol Norris was worried about whether “of” or “for” is the correct preposition to use with risk in relation to damage, disease or a poor outcome. She uses “risk of” only for death and other inevitable events because “risk of” sounded like lay language to her and prevents one writing “risk of smoking for heart disease”. The view that “risk of” is lay language was not generally supported.

Elisabeth Heseltine considered the risk was that of the person, not the event, and suggested “the risk of women for breast cancer”. Meanwhile Angela Turner pointed out that the *Oxford English Dictionary*, with no mention of “for”, says that “risk” is followed by “of”. Thus she would write “the risk of getting heart disease from smoking” on the basis that it is the smoking that is risky. Norman Grossblatt also favoured “of” where it refers to a likelihood of something after exposure to something, but he often encountered “the risk of exposure to something” where he considered “of” incorrect and possibly ambiguous. He proffered the version “women’s risk of breast cancer” as an alternative to Elisabeth’s example. “At risk for” would be correct in his view in phrases such as “People who smoke are at higher risk for heart disease than are people who do not smoke”.

Ed Hull questioned the preference for nouns in scientific writing. Why do we talk about “the risk” when risk is a perfectly good verb? Putting action into the sentence avoids the preposition problem and enhances readability: Smokers risk heart disease more than non-smokers.

Mary Ellen Kerans offered to concordance “risk of” and “risk for” using <http://www.collins.co.uk/corpus/CorpusSearch.aspx>, which is a “criterion referenced” corpus – meaning that Mary Ellen had chosen texts she wished to use as exemplars. She also suggested using an excellent corpus that is offered by Springer free of charge at <http://www.springerexemplar.com>, which is an “open” one probably added to every day. This corpus concordance allows you to search all Springer journals, specific ones which you feel you can trust, or within a subject area, but it does not provide information as to which hit comes from which article.

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## My Life as an Editor – Angela Turner

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I am currently managing editor of the UK Office of *Animal Behaviour*. The journal is owned by the Association for the Study of Animal Behaviour and is published by Elsevier in collaboration with the Animal Behavior Society. We have two journal offices, one based in the UK, at the University of Nottingham, and one in the USA, at Indiana University, Bloomington, which are

largely independent of each other. Each has its own office staff and processes its own manuscripts, although we are in frequent contact.

As a child I wanted to be a zoologist, but when I was doing my PhD, on the behaviour of swallows, I found that the bit I enjoyed the most was gathering information together and writing the thesis, which my fellow postgrads thought very strange. That was when my career path was diverted to writing and editing. Initially I did some freelance editing, in particular for the *Journal of Animal Ecology*, and some writing for popular magazines, while also studying birds in Venezuela and Malaysia. Then the post of assistant editor for *Animal Behaviour* became vacant, and I took over the role in 1986.

### Changes along the way

I run the UK Office, with one part-time assistant. I copy edit manuscripts and help authors, many of whom are not native English speakers, to convey their ideas more clearly. What I do has changed over the years, though.

When I started, there was one UK editor. We kept a card file of referees; I sent manuscripts out by post and kept details in a logbook. The journal, then published by Baillière Tindall, came out every two months. The UK Office received about 150 submissions a year, so two people could easily manage it. But the journal grew rapidly in the 1980s to over 400 manuscripts a year, with a long publication time (over 13 months), so in 1989 we decided to publish monthly, and in 1994 we changed our editorial system to have a board of editors, headed by an executive editor, to make decisions on submissions. I became “managing editor”, and we employed someone to assist me. We also changed the format of the journal and its cover. Last year, because of concerns over a possible sex bias in peer review, we adopted a double-blind peer review system.

My first office had just a typewriter; the arrival of a computer allowed us to invite people to referee papers instead of sending them manuscripts on spec. The first major change in office procedures came when we purchased a manuscript tracking system, enabling us to create a computer database of manuscripts, editors, and referees. Since 2003, we have used an online submission system,

which has revolutionized office procedures: authors submit online and most editorial correspondence is automatically sent by email; the journal’s editors choose the referees and routine procedures such as reminding late referees are taken care of. That has made my job much easier and also more settled.

### Work-life balance

In the early days, the assistant editor was in the same place as the editor. I would have to move house every few years when a new editor took over, until electronic communication and online manuscript tracking meant that the journal office could at last stay in one place. Moving every few years was a big disadvantage of the job but I enjoyed what I did and wouldn’t have wanted to give it up. Apart from the privilege of working in a subject area that I find fascinating, I am fortunate in being able to plan my own working day and do some of my work from home. I don’t stick to the usual office hours (I’m usually at my computer by 6:30 am) and try to incorporate a walk (I’m Secretary of my local Ramblers’ group) or some bird watching and gardening into my week; I find walking very helpful for creative thinking. I go to my office at the University of Nottingham two or three days a week when my assistant is there or to use the libraries. I also give an annual lecture on writing to postgrads at the University and am coordinator of the Behaviour Notes Panel of the journal *British Birds*. In my spare time I’ve also continued to write articles and books, mainly on swallows.

Nowadays I spend a lot of time at my computer editing papers online, but I must admit I was slow to embrace this new way of editing. I find online editing to be less accurate and it is more difficult to compare text, tables, and figures on different pages. It is useful in other ways, such as incorporating authors’ corrections.

Along with the electronic changes, I find there is more pressure among publishers, editors, and authors these days for fast, and cheap, publication. The journal is now typeset in India and there is regrettably less emphasis on helping authors write clearly. We no longer have time to read proofs in house and rely on authors correcting them.

### Lasting impressions

Some authors still appreciate editorial help with their writing and it’s very satisfying to receive feedback from them. One German author recently emailed me about a paper co-written with one of his students that I had copy edited; he thanked me for my help and told me that he still had the first paper he had had published, which I had also copy edited, apparently very heavily – he now shows it to his students so they can see how a scientific paper should be written. It’s nice to know that we sometimes leave a lasting impression.

**Angela Turner**  
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## This Site I Like

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### Scientific Red Cards: a collaborative website for better communication between scientists and institutions about misconduct

*Presented at the 10th EASE Conference, Pisa, Italy, September 2009*

Scientific publications are a basis on which to build scientific knowledge. They also constitute a platform for decision-making in policy formulation – for example, those concerning public health and the environment. The reliability of the scientific record is therefore critical both for the progress of science and for decision-making. However, the history of scientific research provides numerous cases of misconduct. How informed is the research community about the publications that result from misconduct, and how does it deal with this issue?

As PhD students concerned about some social/political issues related to science, we became interested in the debate about genetically modified organisms. When we confronted contradictory publications, we noticed that there were informal claims calling into question the integrity of some of these publications. We then realised that institutions exist that are able to investigate and identify fraudulent publications but that information about the reliability of publications is not readily available, as the official information from the institution is not connected to the publication itself.

It has been repeatedly noted that identification of publications that result from misconduct is difficult.<sup>1-6</sup> Once an institution has assessed a case of scientific misconduct, the corresponding articles can be retracted. However, not all fraudulent papers get retracted,<sup>2-4</sup> and retracted papers sometimes get cited after retraction.<sup>4-6</sup> This points to a lack of visibility of fraudulent publications.

On the one hand, this lack of visibility is a matter of connecting pieces of information together. Some institutions able to assess misconduct put their reports on line (the Office of Research Integrity, at the US Department of Health and Human Services, for example). However, this information is not well connected to the publication itself: while bibliographic content becomes easier to access day by day, such metadata about publications are still absent from most search engines.

On the other hand, this lack of visibility might be related to the reluctance of the scientific community to talk about issues of misconduct. Indeed, misconduct damages the image of scientific institutions.<sup>7</sup> The possibility to debate and become informed about misconduct within the scientific community could be beneficial.

#### Objectives

The objectives of the project are to help the identification of fraudulent papers in the literature, and to build an environment where scientific integrity is openly debated.

We propose to build a database available on a website; it will contain publications in which the content has been

shown to be incorrect, as a result of misconduct.

In addition, it is important that this database be presented in a context where people can become informed and discuss scientific integrity.<sup>8</sup> We propose an interactive platform to offer information and an exchange of ideas on this issue.

#### The project

The project is called Scientific Red Cards and is available at [www.scientificredcards.org](http://www.scientificredcards.org). It has four components: a database; a blog with the possibility to comment and debate cases; information about scientific integrity and links; and a form for reporting assessed cases of misconduct.

*The database* – Each entry in the database comprises the citation for the publication (title, authors, journal, volume, year), together with a link to the corresponding assessment(s) of misconduct. We propose to classify publications into three categories: fraudulent data – fabrication, falsification, and plagiarism; non-complying publication – ignoring editorial policies and standards (non-disclosure of conflict of interest, undeserved authorship, for example); and unethical practices – ignoring ethical rules (lack of informed consent of patients in clinical trials, for example).

*Assessment of misconduct by validated authorities* – Assessments should be made by committees dedicated to scientific integrity, who are able to make investigations, and who are located in research institutions and universities. The list of sources of assessments is present on the website. Ideally, institutions would get involved in this project and themselves guarantee the correctness of the assessment available.

*A collaborative tool* – This database is designed as a collaborative tool: any member of the scientific community can submit publication data for anything questionable, together with a link to the corresponding assessment of misconduct. This reference is then submitted for moderation. If it fits the requirements (as explained above) it is included in the database.

*A place for interaction* – The website hosts a blog, which offers the possibility for users to leave comments on each blog post, and users can comment on each publication and the related assessments in the database.

#### A dynamic project, bound to be improved

*Who carries it out?* – So far, we have developed this project as PhD students, and we have decided to make it collaborative. However, in the longer term we believe that this project needs to be carried out by authorities with higher legitimacy, such as institutions where research is conducted or managed, or that deal directly with scientific misconduct (for example, research integrity offices, editors). This would facilitate

updating of the database and help to involve all actors of the scientific community in a joint effort. Identifying who could become involved in such a project is still an open question, but could include research institutions, editors, and publishing associations.

**What about the author responsible for misconduct and the co-authors?** – Misconduct associated with a paper is expected to cast a shadow over the authors, not only the person responsible for misconduct, but also the co-authors. This is a difficult point. The names of the authors could be hidden in the database, but this would seem hypocritical, as this information is easy to obtain from the internet. This suggests that it would be more relevant to leave the authors' names available, but to specify the person responsible for the misconduct. However, this is not always easy when it comes to undisclosed conflict of interest or undeserved authorship. Finally, in order to avoid users focusing on the names of the authors, one possibility is to make items available only through a search tool by topic, and to hide the full list of publications.

**What code of conduct should be adhered to?** – Codes of conduct differ among countries and between institutions within countries. However, for serious cases of misconduct, such as fabrication, falsification, and plagiarism, they don't differ much, except perhaps in the way that incidences of misconduct are handled and in the corresponding penalties. There are now committees working on harmonizing these codes of conduct,<sup>10</sup> for instance in Europe and in the US (with the Office of Research Integrity). This process takes time, and one can even doubt whether a common code can be formulated, as legislation is often cultural. However, as research is international, we need to be practical, and therefore we propose to refer to the codes of conduct of the institution for each case.

### To what extent is this project viable?

From a legal point of view, the first question that emerges from our proposed project is that of defamation. It is important to note that this database focuses on the content of the publication, and not on the quality of the authors. In addition, as explained above, any publication refers to a judgement already made by an authority.

More generally though, how does this project fit into a legal framework? So far, the website is hosted in France, and therefore falls under French legislation. The main point raised is that of the legal status of a scientific publication as personal information. Indeed, handling personal information, and making such a database available, is not straightforward. However, a scientific publication is an item of personal information that impacts the scientific community and beyond. Based on this, how can it be considered in the same way as personal information?

**Outreach** – From a practical point of view, this database cannot be exhaustive. In addition, this service is not meant to address the question of errors in scientific research although it can have a strong impact on public health policy

(see Greenberg<sup>9</sup> for an example of the unfounded authority of a theory due to citation bias). These points question the relevance of this database. However, the existence of such a database, enriched by the platform we propose to promote exchange, has a great educational value, and a high potential for raising awareness of the issue of scientific integrity within the community. Moreover, our first steps stand as a *proof of principle* that it is technically and legally feasible to collect such information and provide it on an integrated platform, and this should be regarded as a milestone for institutions willing to engage in better management of research integrity.

Despite these questions, we are convinced that this project is both feasible and valuable. Information on scientific misconduct must be available, both for the scientific community and society. In addition, it remains very important that researchers, editors, and research institutions work together to address the issue of scientific misconduct. A dialogue, via collaborative online tools, meetings, and written communication, is essential for any rule or guideline to be considered and accepted by the community.

All authors made equal contributions to the work presented here.

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## News Notes

News Notes are taken from the *EASE Journal Blog* (<http://ese-bookshelf.blogspot.com>). Please email items for inclusion to Margaret Cooter ([mcooter3@gmail.com](mailto:mcooter3@gmail.com)), with "News Notes" as the subject.

TinyURLs are given to save space and aid reading; full URLs (clickable links) can be found on the *EASE Journal Blog*.

### Elsevier guides sponsored content

The journal publisher Elsevier has publicly released a code to guide the production of sponsored content. The code follows criticisms of Elsevier's presentation of the *Australasian Journal of Bone and Joint Medicine* in 2003 as an unbiased journal, when in fact it was sponsored by a single drug company (see [tinyurl.com/yelnxd7](http://tinyurl.com/yelnxd7)). The guidelines cover the need for full disclosure of funding and the origin of content, and indicate best practice from the International Committee of Medical Journal Editors, the Committee on Publication Ethics, and the Institute of Medicine. See <http://tinyurl.com/y3uwp3h>

### Chinese publishing needs cleanup

Pressure to publish has spawned questionable practices in China, including charging extortionate publication fees, establishment of illegitimate journals, ghostwriting of papers, and authors paying agencies to get their papers published in particular journals, writes Ding Jie on [scidev.net](http://scidev.net). Researchers desperate to succeed in China have driven a fivefold increase in the value of the country's trade in scientific papers. The business was worth one billion yuan (£100m) in 2009, five times that in 2007. Shen Yang, a management studies researcher at Wuhan University, describes China's publishing process as "a massive and integrated production chain". ([www.scidev.net/en/science-communication/news/science-paper-trade-booms-in-china-1.html](http://www.scidev.net/en/science-communication/news/science-paper-trade-booms-in-china-1.html))

### Scientists sacked after papers retracted

Two Chinese university lecturers have been dismissed after 70 papers they published in the international journal *Acta Crystallographica Section E* were retracted, Wu Ni reports on [scidev.net](http://scidev.net). Hua Zhong and Tao Liu published the papers in 2007. Fang Zhouzi, a critic who fights academic fraud in China, said, "A researcher is rewarded and promoted largely based on the number of published papers, which poses dangerous incentives for researchers to commit fraud." Researchers who publish in journals such as *Nature* or *Science* can be awarded as much as 100,000 yuan (£9500). See [www.scidev.net/en/news/china-sets-up-rules-to-combat-scientific-misconduct.html](http://www.scidev.net/en/news/china-sets-up-rules-to-combat-scientific-misconduct.html) and [www.scidev.net/en/news/china-issues-another-crackdown-on-scientific-misco.html](http://www.scidev.net/en/news/china-issues-another-crackdown-on-scientific-misco.html).

### Open access can't work exclusively

The journal *Nature* says that open access, subscription, and hybrid journals will coexist in science publishing, rather than there being a "monoculture," in a recent editorial (2010;464:813, doi:10.1038/464813a). It notes that, for example, the author fees at the Public Library of Science have almost doubled from the 2003 goal of \$1500 per paper. The costs at high impact journals are several times this, and open access depends on whether funding bodies are prepared to pay. Hybrid journals allow authors to choose whether to pay to make their work open access, and the publishing company will soon launch one—*Nature Communications*.

### Emphasis on journals corrupts science

Grant agencies must stop counting citations and actually read research proposals and judge their quality, says Professor Peter Lawrence, emeritus professor at Cambridge University and former editor of the *Journal of Embryology and Experimental Morphology*. "It's a bit like judging a hospital by how quickly the telephone is answered," he says, talking about the

awarding of research grants based on which journals researchers have published in. Funding bodies now award grants almost exclusively to researchers who have published in a handful of top scientific journals, he says. The actual research can be of secondary concern to career scientists. (<http://news.bbc.co.uk/1/hi/sci/tech/8490481.stm>)

### Stem cell researchers complain

Fourteen leading stem cell scientists have written an open letter to highlight their dissatisfaction with the peer review system for publishing their work (<http://eurostemcell.org/commentanalysis/peer-review>). Some stem cell scientists claim that reviewers may be deliberately vetoing high quality science from publication to stifle competition. The open letter claims, "Papers that are scientifically flawed or comprise only modest technical increments often attract undue profile. At the same time publication of truly original findings may be delayed or rejected." Huge amounts of money are spent on stem cell research, and the decision of who to fund depends on who has published in top journals. (<http://news.bbc.co.uk/1/hi/sci/tech/8490481.stm>)

### Access to archaeological information

An explosion in commercial archaeology since 1990—excavation is required before building begins—has created an information access problem for academics. The results of excavations get written up for clients and are held in private offices or local government. They are "not publicly available" say the academics, but contract archaeologists say that they are no less available than many obscure journals and masters theses. "Archaeological information is being treated as a commodity to which developers control access," said one critic. It's a gap that needs to be bridged. (*Nature* 2010 Apr 7, [www.nature.com/news/2010/100407/full/464826a.html](http://www.nature.com/news/2010/100407/full/464826a.html))

### Publication bias distorts evidence

Non-publication of negative results is distorting the scientific record, say the authors of a study in *PLoS Biology* (2010;8(3):e1000344, doi:10.1371/journal.pbio.1000344). Of 16 systematic reviews of interventions tested in animal studies of acute ischaemic stroke, involving 525 unique publications, only 10 publications (2%) reported no significant effects on infarct volume and only six (1.2%) did not report at least one significant finding. The researchers estimate that a further 214 experiments (in addition to the 1359 identified through rigorous systematic review; non-publication rate 14%) have been conducted but not reported. Where data are collected but remain unpublished, they cannot contribute to knowledge, the authors say.

### How much does a search cost?

We think web searches are free, but there's an energy cost. Google's data centres are estimated to contain nearly a million servers, each drawing about one kilowatt of electricity. So every hour Google's engine burns through one million kilowatt hours. Google serves about 10 million search results an hour, so one search has the same energy cost as turning on a 100 watt light bulb for an hour. In the United States, data centres account for 1.5% of energy use. ([www.newscientist.com/article/mg20627546.700-search-engines-dirty-secret.html](http://www.newscientist.com/article/mg20627546.700-search-engines-dirty-secret.html))

### Problems with statistical significance

Modern science is wedded to the use of statistical methods but "the standard methods mix mutually inconsistent philosophies and offer no meaningful basis for making such decisions. Even when performed correctly, statistical tests are widely misunderstood and frequently misinterpreted," says Tom Siegfried in *ScienceNews* (27 March 2010, [www.sciencenews.org/view/feature/id/57091/title/Odds\\_are%2C\\_its\\_wrong](http://www.sciencenews.org/view/feature/id/57091/title/Odds_are%2C_its_wrong)). He goes on to criticise the way separate studies are combined and then explains how the P value is

so often misinterpreted because it is "neither necessary nor sufficient for proving a scientific result," according to one interviewee. Statistical significance is also often confused with importance.

### London sees further

"See Further: The Festival of Science + Arts" is a festival at the Southbank Centre in London from 25 June to 4 July to mark the 350th anniversary of the Royal Society. The festival explores links between the sciences and arts and features many cross disciplinary collaborations, scientific, and artistic events. Robin Ince's *Nine Lessons and Carols for Godless People* mixes comedians, scientists, and musicians; "Expect quantum physics, evolutionary biology, accordions and contemporary dance to meld with excitement, laughter and mind boggling ideas. Carl Sagan and Richard Feynman impressions also guaranteed," says the press release. (<http://seefurtherfestival.org>)

### Sexist thinking continues

That men often come before women in English writing, such as "he or she," bears witness to cultural sexism, says John von Radowitz, writing in the *Independent* ([www.independent.co.uk/news/science/sexist-thinking-still-present-in-writing-1921617.html](http://www.independent.co.uk/news/science/sexist-thinking-still-present-in-writing-1921617.html)). In a study, researchers searched for pairs of names that placed either the male or female name first, for instance, "David and Sarah" or "Sarah and David" (*British Journal of Social Psychology* 2010, doi:10.1348/014466610X486347). Pairings with the man first accounted for significantly more mentions. In another study 86 people wrote down the names of an imagined gay male or lesbian couple and assigned attributes such as earnings to each person. The volunteers assigned significantly more masculine attributes to the person named first.

### Lancet retracts MMR paper

The *Lancet* has fully retracted Andrew Wakefield's 1998 case series that started the scare surrounding the measles, mumps, and rubella (MMR) vaccine. The editors said, "Following

the judgment of the UK General Medical Council's Fitness to Practise Panel on Jan 28, 2010, it has become clear that several elements of the 1998 paper by Wakefield et al are incorrect, contrary to the findings of an earlier investigation . . . Therefore we fully retract this paper from the published record." Ten of the paper's 13 authors had already retracted the "interpretation" of the data. (*BMJ* 2010;340:c696)

### Climate change emails show flaws in peer review

Emails hacked from the University of East Anglia in November suggested that mainstream climate scientists might have been trying to censor their critics, and the correspondence raises questions about the effectiveness of peer review. The scientists involved say they were keeping bad science out of journals, but because it was the reviewers' work being criticized they had obvious conflicts of interest. Phil Jones, head of the University of East Anglia's climatic research unit, as an expert in his discipline regularly reviewed papers and sometimes wrote critical reviews that may have had the effect of blackballing papers criticizing his work. ([www.guardian.co.uk/environment/2010/feb/02/hacked-climate-emails-flaws-peer-review](http://www.guardian.co.uk/environment/2010/feb/02/hacked-climate-emails-flaws-peer-review))

### German funding applications limit papers

Germany's main research funding agency is the first in Europe to drastically restrict the number of papers that researchers can list in their grant applications. From July a total of only seven papers may be listed. The agency hopes this will counter the pressure on scientists to publish as many papers as possible in order to win funding or academic appointments. Also, it will not consider supporting papers that have been submitted to academic journals but not yet accepted for publication. (*Nature* 2010 Feb 24, [www.nature.com/news/2010/100224/full/4631009a.html](http://www.nature.com/news/2010/100224/full/4631009a.html))

Thanks to Margaret Cooter and Emma Campbell

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## The Editor's Bookshelf

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Please write to [paola.decastro@iss.it](mailto:paola.decastro@iss.it) or [pennyhubbard@gmail.com](mailto:pennyhubbard@gmail.com) if you wish to send new items or become a member of the EASE journal blog (<http://ese-bookshelf.blogspot.com/>) and see your postings published in the journal.

### ECONOMICS AND FUNDING

Editorial. **The health illiteracy problem in the USA.** *Lancet* 2009;9707(374):2028.

Health illiteracy is the inability to comprehend and use medical information, affecting access to and use of healthcare systems. Most individuals with low health literacy are elderly, poor, and from minorities. They are more prone to visit emergency rooms, stay longer in hospitals, and use fewer preventive services, with consequent high costs for healthcare systems. A reliable US national health literacy measurement tool is not available at the moment but it should be developed and assessed.

### EDITORIAL PROCESS

Audisio RA, Stahel RA, Aapro MS, Costa A, Pandev M, Pavlidis N. **Successful publishing: how to get your paper accepted.** *Surgical Oncology* 2009;18(4):350–356.

Starting from an historical perspective, ethical issues in publishing are discussed and technical suggestions on how to get the final draft accepted for publication are outlined.

The rejection of a manuscript is a frustrating experience and is mostly due to poor experimental design, failure to conform to the target journal, methods not being described in detail, confusing presentation of tables and figures, conclusions not supported by data. Reasons for rejection are listed and described in detail.

Editorial. **Peering into review.** *Nature Medicine* 2010;16:239.

The peer review process is a cornerstone of the scientific publication process. Fourteen editors of stem cell journals recently signed an open letter expressing concerns over the confidential peer review process and suggesting the publication of reviewers' comments. A common fear among authors is that rival scientists could make unreasonable demands to intentionally delay or reject the publication of truly original findings. The peer review process might be improved and properly managed, but it is not clear whether publishing reviewers' comments would be the solution. In confidential peer review, conflicts of interest should be recognized and additional expertise could be asked to evaluate disputed aspects of a manuscript.

Whitlock MC, McPeck MA, Rausher MD, Rieseberg L, Moore AJ. **Data archiving.** *American Naturalist* 2010;175:145–146.

Most data in ecology and evolution are lost to science very quickly after they are collected or summarized. Once a study has been published, the data are often stored unreliably. Yet these data are invaluable to science, for meta-analysis, new uses, and quality control. The example of GenBank shows the value of the availability of data. To promote the preservation and fuller use of data, the *American Naturalist*, *Evolution*, *Journal of Evolutionary Biology*, *Molecular Ecology*, *Heredity*, and other key journals in evolution and ecology will soon introduce a new data-archiving policy.

### ETHICAL ISSUES

Editorial. **Scientific fraud action needed in China.** *Lancet* 2010;9709(375):94.

Editors at *Acta Crystallographica Section E* alerted the scientific community to a fraud involving papers they had published in 2007. It is surprising that wrongdoing

evaded detection during the peer-review process and that the truth was discovered slowly. China's government controls almost all funding for research, and authors need to publish as many papers in high impact journals as possible. China must assume stronger leadership in scientific integrity, developing standards for teaching research ethics and for the conduct of research itself.

Editorial. **Are you an honest scientist? Truthfulness in science should be an iron law, not a vague aspiration.** *Medical Hypotheses* 2009;73:633–635.

In the past 20 years there has been a progressive decline in the honesty of scientific communications. In science, truth should be the primary value, and truthfulness the core evaluation. Everyone should be honest at all times and about everything – especially scientists, otherwise the activity stops being science and becomes something else: Zombie science, a science that is dead but it is artificially kept moving by a continuous infusion of funding. Many are the causes of dishonesty in science; for example, scientists may be subjected to such pressure that they are forced to be dishonest. The corruption of science has been amplified by the replacement of “peer usage”, with “peer review” as the major mechanism of scientific evaluation, thus creating space into which dishonesty has expanded. The hope is in an ethical revolution capable of re-establishing the primary purpose of science: the pursuit of truth.

Graf C, Battisti WP, Bridges D, Bruce-Winkler V, Conaty JM, Ellison JM, Field EA, Gurr JA, Marx ME, Patel M, Sanes-Miller C, Yarker YE. **Good publication practice for communicating company sponsored medical research: the GPP2 guidelines.** *BMJ* 2009;339:b4330. Good Publication Practice 2 (GPP2) represents a useful document for the

publication of company-sponsored medical research. Commissioned by the International Society for Medical Publication Professionals (ISMPP) and developed by a recruited steering committee, the guidelines update those published in 2003. The document is mostly based on the recommendations of codes of practice developed by international associations (COPE, WAME, CSE), but it takes into account the publisher's perspective (Elsevier and Wiley-Blackwell) and the perspective of pharmaceutical companies. From authorship to conflicts of interest, the document provides recommendations for articles and presentations to ensure that results of clinical trials are communicated to the medical and scientific community in an effective and timely manner.

Lacasse JR, Leo J. **Ghostwriting at elite academic medical centers in the United States.** *PLoS Medicine* 2010;7(2):e1000230.

Ghostwriting can no longer be defined as the "dirty little secret" of the medical literature. Over the past several years, medical writers, journals, and editors' associations (such as ICMJE and WAME) have highlighted the problem, developing a policy on ghostwriting and requiring contributorship statements for authors. But what has been done by medical centres and associations? Among the top 50 academic medical centers in the United States, only 20% explicitly prohibit ghostwriting. Administrators of academic medical centres should insist on this point, and should define medical ghostwriting as dishonest, unacceptable, and comparable to misconduct, say the authors. By prohibiting ghostwriting, academic medical centres can cooperate with editors and publishers in improving research integrity.

Larivière V, Gingras Y. **On the prevalence and scientific impact of duplicate publications in different scientific fields (1980-2007).** *Journal of Documentation* 2010;66:179-190. Duplicate publications have received little attention in the information

science community. A bibliometric technique was developed to detect, between 1980 and 2007, duplicate papers across all fields of research, considering common metadata: exact same title, same first author, same number of cited references. The prevalence of duplicates is low (one in 2000) and it is higher in the natural and medical sciences than in the social sciences and humanities. The scientific impact of duplicate papers is below average as they are generally published in journals with impact factors below the average of their field.

Osborne NJ, Payne D, Newman ML. **Journal editorial policies, animal welfare, and the 3Rs.** *American Journal of Bioethics* 2009;9(12):55-59.

A randomized sample of editorial policies of English language peer-reviewed journals that publish original research involving the use of animals. Do policies promote animal welfare and dissemination of information on the 3Rs (reduction, refinement, replacement) within the scientific community? Many journals do not have a policy on the use of animals, and those that do are often limited to requiring that standard regulatory requirements are adhered to. Information is provided for editors and publishers to help them review their editorial policies.

Price RE. **Ethical issues in publishing.** *Radiography* 2009;15(2):95-96.

Research involving humans and including identifiable material and data (images, photographs, names, initials, hospital numbers) should comply with the ethical standards of the Declaration of Helsinki. Editors have the responsibility to ensure that research articles submitted for publication meet these standards. No information should be included in written descriptions unless it is essential for scientific purposes and written informed consent has been obtained. Research misconduct is another serious problem: it includes fabrication, falsification, unethical experimentation, and plagiarism, which are not always easy to detect despite the peer-review process.

## INFORMATION RETRIEVAL

Hartley J. **On the need to distinguish between author and journal self-citations.**

*Scientometrics* 2009;81(3):787-788.

Letter to the Editor clarifying a term used to indicate the elements that together determine the impact factor of a journal. By reading a recent article – by Loet Leydesdorff – on the subject, Hartley found out that what he had meant by "self-citation" was interpreted not as the "authors (who) cite their own works" but as "journal self-citation". Different usages of the term "self citation", shown on a table taken from Google Scholar, lead to a problem of interpretation.

Shulenburg D. **University public-access mandates are good for science.** *PLoS Biology* 2009;7(11):e1000237.

The ArXiv online physics preprints and postprints aimed, in 1991, to simplify scientists' access to and sharing of scientific papers. On the DOAR home page the site advertises over 1500 listings. When researchers face such a wealth of information, accessibility has some disadvantages. What's more, open access and public access pose the question of immediate access to a work, or in the case of public access, of a limitation period in order to weight the cogency of a paper. ArXiv has no peer review but since 2004 has introduced endorsement for publication of a paper from another ArXiv author. Some authors are not interested in having their work peer reviewed. The article considers the policy of Harvard, MIT, and the University of Kansas and other universities regarding displaying their scholarly literature over the web, the economic benefits and potentials of such an initiative, the request to "expand the NIH mandate to all federal funding agencies", etc.

## LANGUAGE AND WRITING

Cantillon P, McLeod P, Razack S, Snell L, Steinert Y. **Lost in translation: the challenges of global communication in medical education publishing.**

*Medical Education* 2009;43:615–620. Communicating meaning between different cultures and contexts is an important issue for international medical education journals, and responsibility should be shared between authors, peer reviewers, and editors. A group of education researchers from Europe and North America examined the comprehensibility of terminology of all articles recently published in four major international journals on medical education. Many of the articles included terminology with a shared understanding of setting between authors and readers.

Verspoor K, Cohen KB, Hunter L. **The textual characteristics of traditional and Open Access scientific journals are similar.** *BMC Bioinformatics* 2009;10:183.

The authors collected two sets of documents, one consisting only of Open Access publications and the other consisting only of traditional journal publications. They examined surface linguistic structures (incidence of conjunctions, negation, passives, pronominal anaphora) and found that the two collections did not differ. The distribution of sentence lengths in both collections were characterized by the same mode. Small differences did exist, but the conclusion is that no structural or semantic differences appeared between the Open Access and traditional journal collections.

## PUBLISHING

Abbasi K. **Keep libel laws out of science.** *Journal of the Royal Society of Medicine* 2010;103:39.

A good editor will publish articles that will upset some readers or institutions. But if the level of upset prompts legal action, the journal is exposed to a substantial financial risk. This knowledge inevitably complicates editorial decisions. The only way a journal can fight legal action is by relying on its insurance policy, but insurers require that potential risk identified by the libel lawyers is eliminated. In the UK, it is easy to launch a libel action; the burden of proof is on the defendant.

The resulting fear of libel is killing scientific debate in medical journals.

Alvarez DE, Helm K, DeGregori J, Roederer M, Majka S. **Publishing flow cytometry data.** *AJP – Lung Cellular and Molecular Physiology* 2010;298:L127–L130.

Flow cytometric analysis is an important method for better understanding cellular activity. The complexity of the data arising from this technique demands a standard way of publishing. A way of consistently summarising flow cytometric experimental information as supplemental data is proposed, emphasising experimental and sample information, data acquisition, analysis, and presentation.

Heimans L, van Hylckama Vlieg A, Dekker FW. **Are claims of advertisements in medical journals supported by RCTs?** *Netherlands Journal of Medicine* 2010;68(1):46–49.

To provide adequate information for an appropriate prescription, drugs advertising in medical journals should include a minimum set of information – that is, an evidence-based description of both benefits and risks. Heimans et al studied to what extent randomized trials support the claims of drugs advertisements in leading general medical journals. Balanced information may be missing, claims made in advertisements may not be evidence-based, and RCTs used to support claims may have been sponsored by the pharmaceutical industry. A regulatory code for advertising in medical journals is needed.

Hrynaszkiwicz I, Norton ML, Vickers AJ, Altman DG. **Preparing raw clinical data for publication: guidance for journal editors, authors, and peer reviewers.** *BMJ* 2010;340:c181.

Many journals and funding bodies require researchers to make their raw data available, but there is a need for guidance on data sharing in clinical medicine. For any data relating to individuals, risks to patient confidentiality and anonymity should be minimized, and, where possible,

consent should be sought from participants. Direct identifiers should not be included in the data, and if three or more indirect identifiers (age, sex, etc) are included, data should be reviewed by an independent researcher or ethics committee before submission.

Marcus E. **A publishing odyssey.** *Cell* 2010;140(1):9.

Cell Press has been working on a project they call “the Article of the Future” to rejig the format of research articles to better use the capabilities of the internet. Articles in this issue sport the new format.

Moizer P. **Publishing in accounting journals: a fair game?** *Accounting, Organizations and Society* 2009;34(2):285–304.

Considers the purpose of publishing in academic journals and the motivations of authors, reviewers, and editors. The article includes a survey of the suggestions to improve the publishing process, considering that the purpose of publishing is above all that of communicating important results to inform public debate on major issues.

Young SN. **Bias in the research literature and conflict of interest: an issue for publishers, editors, reviewers and authors, and it is not just about the money.** *Journal of Psychiatry and Neuroscience* 2009;34(6):412–417.

Conflicts of interest are not limited to financial aspects, but cover other aspects of human behaviour, and therefore pervade every aspect of publishing. There is no entirely satisfactory way of dealing with conflicts of interest, but researchers should be aware of the issues.

## RESEARCH EVALUATION

Aksnes DW, Rip A. **Researchers’ perception of citations.** *Research Policy* 2009;38(6):895–905.

While the use of publication and citation indicators increases, their application is controversial. Researchers perceive citations as part of the reward system of science, but on the other hand they criticize them

for not reflecting actual scientific contribution. Viewpoints present in the Norwegian scientific community were investigated by means of a questionnaire survey. Respondents' answers and comments related to their perceptions of citations and role of citations offered an informal sociology of citations. Even scientists who have produced highly cited papers doubt the fairness of citations as performance measures. More information on the so-called "citation myths" should be given to the scientific community.

Crookes PA, Reis SL, Jones SC. **The development of a ranking tool for refereed journals in which nursing and midwifery researchers publish their work.** *Nurse Education Today* 2009;1532.

The Journal Evaluation Tool, tested on 52 responding journals, may overcome some problems associated with the sole use of journal impact factors and may be utilized as an alternative measure of journal quality.

Editorial. **Conclusion by exclusion.** *Nature Genetics* 2010;42:95.

Studies of systems models and biomarkers present large amounts of information to publishers and readers. To aid review and readability, such studies should propose explicit hypotheses and demonstrate exclusion of competing explanations. To distinguish the direct influence of the research independently of the publications that describe it, hypotheses and experiments would be coded and separately cited. A study providing strong inferences will be both well used and highly cited.

Smith DR, Rivett DA. **Bibliometrics, impact factors and manual therapy: balancing the science and the art.** *Manual Therapy* 2009;14(4):456–459. Presents a history of bibliometrics and describes citation analysis and the use of impact factors. Alternative measures to impact factors, such as article download counts and internet-based journal sessions, should be developed.

Van Nierop E. **The introduction of the 5-year impact factor: does it benefit statistics journals?** *Statistica Neerlandica* 2010;64(1):71–76.

Impact factors for statistics journals are lower than those in other disciplines. In 2008, the Thomson Institute for Scientific Information introduced the 5-year impact factor (IF5), which uses the 5-year window instead of the traditional 2-year window (IF2). Statistics journals show a substantial increase in impact factor when moving from IF2 to IF5. The new impact factor calculation appears to benefit social sciences more than it benefits science.

## SCIENCE

Editorial. **Do scientists really need a PhD?** *Nature* 2010;464:1785.

In the USA and Europe, students usually finish a multiyear programme of postgraduate training before they can fully participate in the front rank of research. The Chinese Beijing Genomics Institute has enrolled about 500 university students to join the Institute soon after their graduation, participating in and contributing to hands-on research. This model may be worth serious consideration although it is not sure that the institute can prepare its student-workers to meet the wide range of skills needed by industry and academia. If this approach works, it could be a model not only for creative approaches but also for education and training.

## SCIENCE COMMUNICATION

Editorial. **Onwards and upwards.** *The Economist* 2009;19:35–38.

Examines scientific, economic, and moral progress. "From the perspective of human progress, science needs governing. Scientific progress needs to be hitched to what you might call 'moral progress.' It can yield untold benefits, but only if people use it wisely. They need to understand how to stop science from being abused. And to do that they must look outside science to the way people behave."

Jones N. **Q&A: Carl Zimmer on writing popular-science books.**

*Nature* 2010;463:737.

Second in a series of five interviews with authors who write science books. Carl Zimmer says that a lot of editors do not put much work into the editing process and sometimes authors have to hire freelance editors to get the guidance they need.

Jones N. **Q&A: Peter Atkins on writing textbooks.** *Nature* 2010;463:612.

Authors of good science textbooks should imagine themselves as students and capture a mode of thinking, says Atkins, author of *Physical Chemistry* (1978). The rewards for textbooks can be considerable, and not only economic. The intellectual rewards are also great: there are real pleasures in helping to mould people's attitudes.

Pierre-Leon T. **De Sciences à l'école.** *Accedit* 2009;10.

The teaching method in this article aims to build a scientific mind in 5 to 12 year old students. This procedure is illustrated in 15 modules, each focusing on specific subjects: magical thinking, rational thought, validation of scientific affirmation, daily lives solving techniques, living organisms, earth, moon, the universe, etc. It will require skilled teachers.

Roland MC. **Quality and integrity in scientific writing: prerequisites for quality in science communication.**

*Journal of Science Communication* 2009;8(2):A04.

Most scientific papers and researchers' writing practices provide evidence that they do not conform to quality criteria. A majority of researchers tend to ignore standards and good practice guidelines and to conform to the model that circulates within their communities. The review process obviously does not play its role in maintaining quality and integrity of the sources. Then both researchers and reviewers of scientific papers are responsible for quality in the first place. As a solution, the whole system of science communication must be reconsidered.

Thanks to Anna Maria Rossi, John Glen, Massimo Antonucci, James Hartley, Silvia Maina, John Hilton, Margaret Cooter.

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## Forthcoming Meetings, Courses, and BELS Examinations

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### 30th EMWA conference: "Medical writing in an electronic era"

11–15 May 2010, Lisbon, Portugal  
<http://www.emwa.org/>

### Council of Science Editors (CSE) annual meeting: "The changing climate of scientific publishing—the heat is on"

14–18 May 2010, Atlanta, USA  
<http://www.councilscienceeditors.org/events/annualmeeting10/index.cfm>

### The Future of Our Magazines: Is the medium the message? (University Research Magazine Association)

18–21 May 2010; Orlando, FL, USA  
<http://www.urma.org/conference.php>

### 2nd ESP/EAP (English for specific/academic purposes) conference: "ESP/EAP innovations in tertiary settings: proposals and implementations"

21–23 May 2010, Kavala, Greece  
<http://www.teikav.edu.gr/foapep/espeap/>

### Science Communication Conference: "Audiences for engagement"

24–25 May 2010, London, UK  
<http://www.britishscienceassociation.org/ScienceCommunicationConference>

### Third European Conference on Scientific Publishing in Biomedicine and Medicine

27–29 May 2010; Leiden, Netherlands  
[www.lumc.nl/ecsp2010](http://www.lumc.nl/ecsp2010)

### Reflections: Editing Content and Culture (Editors' Association of Canada)

28–30 May 2010; Montreal, Canada  
<http://www.editors.ca/conference/index.html>

### American Association of University Presses (AAUP) Annual Meeting

17–20 June 2010, Salt Lake City, USA  
<http://aaupnet.org/programs/annualmeeting/index.html>

### 2010 Joint Annual Meeting:

### "Revolutionary ideas in biocommunications"

Association of Biomedical Communications Directors, Biocommunications Association, Health & Science Communications Association  
 2–5 June 2010, Boston, USA  
<http://www.bioconf.org/>

### IEEE Professional Communication Society: International Professional Communication Conference (IPCC 2010)

7–9 July 2010, Twente, Netherlands  
<http://ewh.ieee.org/soc/pcs/>

### ALPSP International Conference 2010

8 September 2010, Wyboston, UK  
[http://www.alpsp.org/ngen\\_public/article.asp?aid=131503](http://www.alpsp.org/ngen_public/article.asp?aid=131503)

### Mediterranean Editors and Translators Meeting 2010 (METM10)

28–30 October 2010, Tarragona, Spain  
[http://www.metmeetings.org/index.php?page=metm10\\_call](http://www.metmeetings.org/index.php?page=metm10_call)

### National Association of Science Writers: Annual Meeting

4–9 November 2010, New Haven, USA  
<http://www.nasw.org/meeting/>

## COURSES

### ALPSP training courses, briefings and technology updates

Half-day and one-day courses and updates. Contact Amanda Whiting, Training Coordinator, Association of Learned and Professional Society Publishers, Tel: +44 (0)1865 247776; [training@alpsp.org](mailto:training@alpsp.org); [www.alpsp-training.org](http://www.alpsp-training.org)

### Publishing Training Centre at Book House, London

Contact: The Publishing Training Centre at Book House, 45 East Hill, Wandsworth, London SW18 2QZ, UK. Tel: +44 (0)20 8874 2718; fax +44 (0)20 8870 8985, [publishing.training@bookhouse.co.uk](mailto:publishing.training@bookhouse.co.uk) [www.train4publishing.co.uk](http://www.train4publishing.co.uk)

### Society for Editors and Proofreaders

*SfEP runs one-day workshops in London and occasionally elsewhere in the UK on copy-editing, proofreading, grammar, and much else.*

Training enquiries: tel: +44 (0)20 8785 5617; [trainingenquiries@ssep.org.uk](mailto:trainingenquiries@ssep.org.uk)  
 Other enquiries: SfEP, Erico House, 93–99 Upper Richmond Road, Putney, London SW15 2TG, UK. Tel: +44 (0)20 8785 5617; [administration@ssep.org.uk](mailto:administration@ssep.org.uk); [www.ssep.org.uk](http://www.ssep.org.uk)

### Society of Indexers workshops

*The Society of Indexers runs workshops for beginners and more experienced indexers in various cities in the UK.*

Details and booking at [www.indexers.org.uk](http://www.indexers.org.uk); [admin@indexers.org.uk](mailto:admin@indexers.org.uk)

### University of Chicago

*Medical writing, editing, and ethics are among the many courses available.*

Graham School of General Studies, The University of Chicago, 1427 E. 60th Street, Chicago, IL 60637, USA. Fax +1 773 702 6814.

<http://grahamschool.uchicago.edu>

### University of Oxford, Department for Continuing Education

*Courses on effective writing for biomedical professionals and on presenting in biomedicine, science, and technology.*

Contact Leanne Banns, CPD Centre, Department for Continuing Education, University of Oxford, Littlegate House, 16/17 St Ebbses Street, Oxford OX1 1PT, UK.

Tel: +44 (0)1865 286953; fax +44 (0)1865 286934; [leanne.banns@conted.ox.ac.uk](mailto:leanne.banns@conted.ox.ac.uk)

[www.conted.ox.ac.uk/cpd/personaldev](http://www.conted.ox.ac.uk/cpd/personaldev)

### BELS - Board of Editors in the Life Sciences examination schedule

See: [www.bels.org/becomeeditor/exam-schedule.htm](http://www.bels.org/becomeeditor/exam-schedule.htm)

23 May 2010, Tokyo, Japan; register by 2 May

10 November 2010, Milwaukee, WI; register by 20 October

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## EASE Business

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### EASE goes to ESOF

We are organizing a session in the careers section of the EuroScience Open Forum being held in Turin. It will be on Saturday 3rd July, with brief presentations from several EASE members followed by discussion/questions and answers. We encourage any EASE members who are attending ESOF to join us at this event.

We will be holding our AGM in Turin on the same day. We are delighted that Philip Campbell of Nature has agreed to speak after the meeting (see p37). We hope that all members attending ESOF and some of those based in Italy will make an effort to come to hear him.

### Triennial Conference

We have completed our analysis of the Pisa conference. We received some very helpful feedback via the questionnaires and are grateful to all those who submitted one. We are taking all of this into account in planning the 2012 conference.

We have two excellent proposals, one from Eva Baranyiova for a meeting in Brno and one from Mare-Anne Laane for a meeting in Tallinn. We have asked for further details for each and will make our decision in July. Suggestions for the theme of the conference or just for certain sessions are welcome and should be sent to the Secretary.

### Membership update

Almost everyone has now paid their subscription for this year.

Unfortunately, we have lost several members, owing to retirement and/or financial constraints. EASE relies on subscription income to fund its activities: please encourage all

your colleagues and editorial contacts to join EASE to enable us to continue with our activities.

### Can you help?

As you can see, Council, the Publications Committee and the Secretary are keeping busy on your behalf. We would appreciate your help with various things:

- Does your company recruit editors? If so, does it use the EASE website? If not, have you suggested this to your HR department?
- Do you/your company offer Editorial services or Training courses? If so, please list these on the relevant part of the EASE website.
- Is there a department at your local university that might subscribe to ESE? Please encourage relevant institutions to subscribe or provide contact details to the Secretary.
- Are you following EASE? On Twitter? Facebook? LinkedIn? Are your colleagues? Please spread the word.
- Are you going to a meeting where other editors might be present? Could you take EASE fliers or copies of ESE? The Secretary would be pleased to send these to you or directly to the conference organizer.

Do you have any further suggestions for EASE activities or ways in which we can raise our profile? All ideas welcome!

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## New Members

### Individual

#### Dr Robert Allwood

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