



## **Forum agenda**

**Meeting to be held on Monday 5 March 2012 at 3pm  
The Council Chamber, The Royal College of Paediatrics and Child Health (RCPCH),  
5-11 Theobalds Road, London WC1X 8SH**

### **1. Update on COPE activities by the Chair**

#### **2. Update: Editor wants more advice**

**11-24 Inappropriate authorship (FJ)**

#### **3. New cases**

**12-01 Habitual plagiarist (SS)**

**12-02 How to correct an incorrect decision to publish a flawed paper (TO)**

**12-03 Submissions from institutions where misconduct has previously been suspected (RR)**

**12-04 Accusation of non-attribution of authorship (RJ)**

**12-05 Correcting errors versus privileged information (DF)**

**12-06 Review of a book written by an editor of a journal (JM)**

**12-07 Was this study unethical? (RM)**

#### **3. Updates**

**11-26 Publication of private data**

**11-28 Request to withdraw as an author of an accepted but unpublished paper**

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## **UPDATE: EDITOR WANTS MORE ADVICE**

### **11-24 Inappropriate authorship on students paper (FJ)**

A cross-sectional, questionnaire-based study which was a final year student's project was submitted as an original article to our journal on 30 April 2011. On initial review it was obvious that it was conducted by students and written by them, but the list of authors had the supervisor as the first author, followed by 13 students.

The supervisor, who was also the corresponding author, was questioned on authorship criteria. If it was the supervisor's project and the students had helped, then why were there so many students listed (13 in all)? If it was the students' project, which is a requirement of their curriculum, then why was the supervisor the first author? He/she should be acknowledged only.

The authors decided to withdraw the article on the grounds that they wanted to send it to a foreign journal. We obtained the signatures of all of the authors and closed the file.

The same article was resubmitted as a new article on 29 August 2011. The declaration that it had not been previously published was sent to our journal on 5 September 2011. No change had been made in the names of the authors. The signatures of the students were a photocopy of the original ones submitted previously. Apparently the supervisor thought that the journal office would not be able to associate this article with the previous submission.

The journal believes it would be useless to explain the authorship criteria to the supervisor as apparently he/she is eager to have another article on their CV. The students are the ones who suffer.

What steps should the journal take, particularly with regard to the false authorship?

#### **Advice:**

The Forum agreed that the initial response from the editor was very good, and s/he handled the case well, and it was disappointing that the message did not get through to the authors. The Forum questioned whether the editor would be able to contact all of the students individually. The Forum emphasised once again that journals should consider having the email addresses of all authors when a paper is submitted, not just the corresponding author.

Even if the editor feels it is pointless contacting the corresponding author following this second submission, the Forum argued that the editor should follow due process. The editor should contact the corresponding author and ask for an explanation. If the explanation is unsatisfactory, the editor should contact the author's superior at the institution informing them that this person is not following the accepted guidelines on authorship. The editor could also contact someone at the institution (e.g. the Dean) and ask them if they have an institutional policy on authorship.

For the future, the editor should consider publishing a list of the contributions of all authors in every paper.

**Update:**

The consensus from the Forum was that I should ask for the email addresses and cell phone numbers of all of the student authors and ask the university for their authorship policy. I did both of these. The university has not replied, despite a reminder. Unofficially, I was told that they have no such policy.

I spoke to one of the students who “off the record” told me that the supervisor had asked for these contact details, which the students had provided. These were never sent to the journal. I asked the student to send them to the journal office, which he did. The following statement, signed individually by all of the students and the supervisor, was mailed to the journal.

“Contribution:

I as the undersigned author of the article titled xxxxxxxxxxxxxxxxxxxx, has contributed significantly in the various steps of the research starting from topic selection, literature review, introduction, methodology, questionnaire, analysis, discussion and final review. I am further submitting that there is no conflict among us on any matter regarding the order of authorship.”

The student also told me unofficially that they were coerced into signing this statement. The students who have done all of the work are the sufferers. They cannot raise any objections against the supervisor for fear of an impact on their career.

Can COPE suggest what I should do now?

## NEW CASES

### **12-01 Habitual plagiarist (SS)**

Author F published a single case report (CR1) in my journal. A few months later, I received a letter from author G who claimed that the case published by author F was a verbatim copy of his case report published in another journal H. On comparison of the documents it was obvious that CR1 was an exact reproduction of the article of author G. More than 90% of the sentences overlapped in both articles and even the clinical photographs were identical. The case report by author G was published 6 years before the publication of CR1. Both authors work in different institutions that are more than 500 km apart. Neither journal is indexed in Pubmed and journal H is not a member of COPE.

I contacted author F but received no reply to multiple emails. I also contacted the editor of journal H and informed my editorial board members about the case. One of the associate editors recognised author F's name from another case report which had been accepted for publication at the journal. This second case report (CR2) had been reviewed and recommended by reviewers and no one suspected plagiarism. Following some research into this matter, the associate editor found that CR2 was an exact copy of another article by author P published in journal H. Publication of the article by author P predated the submission of CR2 by 7 years. In CR2, the text and clinical photographs were identical to the article of author P. As the journal is not indexed in any major database, no one had detected the plagiarism. We were able to stop publication of the second case (fortunately the manuscript was in the queue for the printer but had not yet been published).

It seems author F is habituated to plagiarism. It is not known how many such plagiarised papers he has published in various journals. Apparently, author F has never published any papers in journals indexed in Pubmed, suggesting he is aware of what he is doing.

What advice would COPE offer regarding this case?

## **12-02 How to correct an incorrect decision to publish a flawed paper (TO)**

Some years ago our journal published a paper reporting concentrations of a substance in an organ in a small number of people of a particular occupational group who had died of a rare disease. The results have been reanalysed in two subsequent papers and discussed in five pieces of correspondence in two journals. The original paper contributes to a body of evidence used by the defence in some compensation claims in the USA. One of the authors of the original paper is prominent as an expert witness in such cases.

In the course of one of these compensation cases, some original laboratory results behind the original paper were disclosed to a court and were published in another journal. A lawyer complained to us that they undermined the original paper, which the complainant said should be retracted.

We have examined the original paper and the newly published data, and have concluded that the paper is consistent with the new data and the complaint cannot be upheld. However, in the course of this we noticed a completely independent problem: important statements made in the discussion in the paper do not agree with the results presented in the tables in the paper. Although this seems clear once it is pointed out, it has apparently not been noticed by authors of the seven subsequent publications on the data. Our conclusion is that we should not have accepted the original paper in its present form.

The heart of the inconsistency is that the text makes statements which it says are true of all the cases observed, but inspection of the results makes it clear that there is at least one exception, which weakens the impact when there are only a few cases anyway.

Close examination of the original paper also discloses that many of the measurements must have been at low levels, close to the limit of detection, and subject to large uncertainties which make the conclusions insecure statistically. The low level of the results is confirmed by the newly disclosed laboratory data. The paper does not discuss these uncertainties, and they have been ignored in references to the findings in later papers. Although this reinforces our view that we were wrong to accept the original paper as published, there is always room for argument about statistical analysis, so we regard this as a less serious problem than the inconsistency between the discussion and the tables.

The paper was processed before we started using online submission, and the reviewers' and editor's reports no longer exist.

We believe that this inconsistency would justify a notice of correction to the original paper, by the criteria in the COPE guidelines. However, the case does not fit the usual pattern because we are not responding to new information but to a realisation that we made a mistake and that we published a paper which was seriously flawed in parts—we would like to correct the paper because we have changed our mind about it.

Has the COPE forum any comments please?

### **12-03 Submissions from institutions where misconduct has previously been suspected (RR)**

A scientific paper was submitted in January 2011. After initial assessment by the journal's editor-in chief, it was allocated to one of the co-editors. By chance, the co-editor had reviewed the manuscript for another journal only a few weeks before. The manuscript had been rejected by the previous journal for a number of methodological flaws.

The resubmitted manuscript contained significant changes to both methodology and results apparently correcting the flaws noted in the previous reviewers' comments. Realistically, these changes could have only been possible if the study had been repeated but as only a few weeks had elapsed since the previous rejection, the editor suspected fabrication of results.

The editor contacted the editor-in-chief to highlight a possible case of misconduct. The manuscript was rejected primarily because it was not of a sufficient standard to merit publication and also because of concerns regarding the possible falsification of results.

In his letter to the author, the editor-in-chief asked for an explanation of the differences between the two manuscripts. The reply claimed that additional patients had been recruited. Of note, the demographic details (including age, height and weight) in the revised paper were identical to the previous submission, making this explanation unlikely.

In view of the unsatisfactory response from the lead author, a letter was sent to the dean of the faculty of medicine at the author's institution but no reply has so far been received. Information regarding the academic department in this university has been difficult to obtain as their website is unhelpful so it is not clear if this letter was received by those in authority. Consequently, after several months and following discussion with the journal's publisher, it was decided not to pursue this enquiry further.

Recently, another manuscript from the same department has been submitted. The co-author of the previous paper is listed among the five authors of this new submission. It is not clear, however, if this author was complicit in the previous case of alleged misconduct. The lead author of the previous paper is not included.

This paper has been reviewed and is again of a poor standard that does not warrant publication. There is, however, no reason to suspect misconduct with this current study.

The editor-in-chief has so far not responded to the authors regarding publication but has asked for the contact details of the head of the academic department from the lead author. An email reply included a contact name. A letter to the academic lead asking for clarification of the previous submission has been sent. A reply is awaited.

At present it is impossible to establish whether the previous misconduct is the result of a single rogue researcher or an institutional problem with research governance. The failure to receive an adequate explanation may simply be a problem with contacting somebody in authority who can investigate the conduct of research within this department. It is however, difficult to believe that a co-author of a paper where misconduct almost certainly occurred was not aware of such behaviour.

### **12-04 Accusation of non-attribution of authorship (RJ)**

In 2008, our journal published a specific series, and an author offered to write short introductions to a series of summaries of the management of various medical problems. One of the articles used a summary written by the complainant, who was fully acknowledged in the table accompanying the article written by his colleagues, but not included as an author. Two years after publication, he complained that he had not been acknowledged as a full author and so could not use the publication in his CV. In response, the first author of the introductory article wrote an explanatory and apologetic email explaining how she had gone about writing these articles:

"This process involved amending the article to make it suitable for national publication and writing a brief overview to accompany the article. Modifications to the article to nationalise it were made by me but fully approved by the original author and submission was not done unless the author agreed. This process involved approaching the author for permission, an email exchange to confirm changes and a final sign off by the original author to indicate their satisfaction with the changes. No submissions were done without the approval of the author of the article and the author of the overview article. Various people agreed to write the accompanying overviews."

In April 2010, following the complaint, the editor wrote to apologise if there had been a misunderstanding, but also pointed out that the journal had acted in good faith, given the explanation above.

We heard nothing further until 2 years later when the complainant made contact again saying that he still wanted to be an author so that he could use the reference in his CV. The editor responded sympathetically but concluded that "we published it in good faith on the reasonable assumption that the authorship was agreed. It really isn't appropriate to ask us to re-attribute authorship in this way".

We told him that we had now decided to refer the case to COPE.

## **12-05 Correcting errors versus privileged information (DF)**

The editor-in-chief received an email from author A regarding a recently published corrigendum by authors BCD, one of whom (author C) is a member of the journal's editorial board. In this email, author A claimed that the corrigendum, which corrected some errors in an earlier article by BCD, was based on illicit use of privileged information, obtained by two of the authors (B and D) who were reviewers on two different versions of a critique submitted by author A, which pointed out these errors. In the email, author A requested that the corrigendum be retracted and that the journal "publish an editorial note that sets the record straight".

The sequence of events was as follows. Author A's critique was rejected by the handling editor, based in part on negative reviews and in part on the grounds that it was far too long and contained redundant and unnecessary material. One reviewer (B) invited a revised version. A second round of review led to the suggestion by reviewer D, that BCD publish an erratum acknowledging the errors uncovered by the submission, together with a further comment from author A. The handling editor in turn proposed that author A and authors BCD submit a joint manuscript acknowledging the errors and other points. Author A refused, on the basis that the information that authors BCD obtained was privileged, and therefore could not be appropriated. Author A informed the editor that the critique had now been submitted to another journal.

Authors BCD then proposed submitting a corrigendum, but the handling editor informed authors BCD of author A's objection along with the admonition that authors BCD's "only option is to wait for the critique to appear in print and publish a response to that".

About 1 year later, authors BCD submitted the corrigendum, suggesting that sufficient time had elapsed to allow author A to publish the critique, and that "This seemed to us to balance the interests of author A against the ethical requirement to promptly correct errors that we are aware of in our published paper". The original handling editor had since left the journal but recommended publication, and the recommendation was accepted by the editor-in-chief.

Authors BCD's corrigendum acknowledged that the errors were "called to our attention" by author A. When notified of author A's email complaint, authors BCD proposed to submit a second corrigendum to supersede the first one, and would cite author A's critique, which had now appeared in another journal. This proposal was rejected by author A on the grounds that authors BCD had appropriated author A's ideas.

There are several complications.

1. Author A's manuscript was a direct critique of authors BCD's earlier article, but author A did not consult with authors BCD before submitting it. However, author A nominated author B as a potential referee.
2. Authors BCD did not inform author A that, despite author A's objection, they later submitted the corrigendum, nor did the handling editor at the time inform author A that the corrigendum had been submitted and later that it had been accepted.

3. Although author A informed the handling editor that the critique had been submitted elsewhere, author A did not apprise the editor of its progress.
4. Authors BCD did consult with the editors of the journal at each step.

*Questions:*

- What is the appropriate balance between two conflicting ethical principles: the need for authors BCD to correct an error in their work versus the need to maintain the confidentiality of privileged information?
- Is retraction of the corrigendum warranted?
- Is authors BCD's proposal of a second corrigendum a reasonable solution?
- Are there other actions that should be taken?

**12-06 Review of a book written by an editor of a journal (JM)**

Two scholars and professional colleagues, A and B, serve as co-editors of a peer-reviewed international journal. Editor A, who recently had a book published, has requested that editor B solicit a review of the book from a scholar in the field. Editor A would like this review to be published in the journal that they edit together. Editor B is concerned that this situation would put him in a situation of conflict of interest, compromising his ability to serve impartially as editor and, moreover, that the reputation of the journal would be compromised. Editor B has a strong interest in maintaining cordial and collegial relations with editor A.

### **12-07 Was this study unethical? (RM)**

We reviewed and published a randomised controlled trial in which children's exposure to parental secondhand smoke (SHS) was either sustained (usual practice control) or parents were asked to avoid smoking around their children (intervention group). The study included more than 400 children averaging 9 years old. Parents provided written informed consent. The study was approved by the ethics committee of the researchers' institution. Data were collected in a school setting by self-reported questionnaire and interviews. Children were classified according to amount of SHS exposure and baseline data about several types of symptoms were collected. The children with the symptom of interest and exposed to SHS were then randomly divided into two groups. Smoking members of families in group 1 were asked not to smoke in the child's presence for a period of 6 months while those in group 2 were asked not to change their smoking habits. Parents were told prior to study enrolment that they would not be required to quit smoking as part of participation, rather they would be asked to reduce children's SHS exposure. After 6 months, data were collected again in order to assess the proportion of children in the two groups who were still displaying the symptom of interest. Following this trial, all parents and children were asked to participate in a 5 month school project related to the risks of SHS exposure. The project included weekly lessons about the risk of SHS, attended by children with their parents to educate children about SHS risks and help parents to either reduce or cease smoking in their children's presence.

Neither the handling editor nor reviewers raised questions about the study's ethics during the review process. However, following publication, we received a letter questioning the study's ethics on the basis that the known risks of SHS exposure outweighed any benefit to be gained by learning about whether SHS was a risk factor for the studied symptom and that the study procedures violated the Helsinki Declaration. The letter's authors argued that the study's advising of smoking parents in the control group to not change smoking habits could have unnecessarily exposed their children to additional SHS if parents who would have otherwise quit or reduced their children's exposure did not do so because of the study. The authors of the letter claimed that they could not think of a single research question that would justify exposing children to SHS.

In discussion, one of the editors argued that the letter missed the important point that after the randomised controlled trial, all participants received the 5 month intervention to promote smoking cessation among parents and/or to reduce SHS exposure, and questioned the letter's implication that the children would be better off if the study had not been conducted. S/he wrote that the letter seemed to argue against any controlled trial to reduce children's exposure to SHS, but that there were several dozen of these in the published literature.

The questions are twofold: was this an unethical study? And, if so, how should the journal proceed?

## UPDATES

### **11-26 Publication of private data**

#### **Anonymised text of the case:**

An article was submitted for publication. This was a survey of research activity in a specialist area and included, among other things, research funding amounts from each institution. This led to a sort of 'league table'. The information was provided by the responding director of the specialty area or head of school/research group of each institution. The cover letter stated this is for research purposes. No particular ethics approval was sought for the study as it was based on staff/professionals and most were known to the principal author.

The question is whether these data on amount of funding are private or public. While grant income data can be available in funders' websites/reports or the institutional/departmental websites, certainly it exists in Research Assessment Exercise (RAE) reports that are available to the public for past research, this may not be the case for all research grants (ie, industry grants or private donations).

Is research grant income classified as private data (in which case, consent is needed) or it is public data and the cover letter to the study means that returning completed questionnaires implies consent?

#### **Advice:**

The Forum suggested that although the authors may have a responsibility to the individual researchers at the institutions who provided the data, this case appears to relate to institutional data. As there are no individual data, breaching any privacy laws is not an issue. There was some concern from the Forum that the individuals may face disciplinary action by their superiors for supplying the data, but all agreed that this was not really an ethical issue, and there is no ethical principle to protect the institution. Most believed that it did not appear to be a very well-designed study and perhaps the editor should talk to the authors about anonymising their data and broadening the message of the paper. The editor might also like to draw their attention to the potential obligation the authors have to the individual researchers. However, no one suggested that the paper should not be published.

#### **Update:**

The authors were allowed to continue with the submission and the paper is now undergoing peer review.

### **11-28 Request to withdraw as an author of an accepted but unpublished paper**

#### **Anonymised text of the case:**

Last March we accepted a paper written by a post-doctoral fellow (PD) and an assistant professor (AP). The work was done by PD in AP's laboratory; PD has now moved on (to another country, in fact). Soon after the manuscript was sent to production, AP sent an email asking to delay production of the manuscript because AP was worried that there may be an 'error' in the manuscript that might require 'some adjustments'. Months passed with no further word from AP. A few weeks ago, I wrote to AP regarding an update. Last week, AP

replied, asking to remove his/her name from the manuscript because AP and PD "... have an insurmountable scientific disagreement over how the data for this study should be tabulated and presented".

AP called yesterday to provide some additional background. AP believes that PD made some questionable decisions with respect to the data and AP is unable to replicate the findings in his/her own analyses of the data.

AP's university looked into the matter and found insufficient evidence to pursue a claim of scientific misconduct. According to AP, PD interprets this to mean that PD has done nothing wrong.

I have not yet said anything to (or heard from) PD. I am certainly not comfortable in moving forward with this publication. Yet, without knowing more details and without hearing PD's side of the issue, it would seem unfair to PD to withdraw our decision to publish the paper. And, anticipating what PD might say ("I did everything correctly"), I find it hard to imagine how we can adjudicate the issue.

**Advice:**

The editor updated the Forum by telling them that PD had provided him with a copy of the email from AP's institutional panel saying that after careful review of the evidence, the panel unanimously reached the conclusion that allegations of misconduct against PD were not merited and no further proceedings were warranted. Also, AP emailed the editor and requested that the manuscript be withdrawn. AP continues to insist that s/he cannot back up the data but refuses to say why.

The Forum agreed that for the editor to make a decision on whether or not to publish the paper, he needs more facts. The Forum suggested the editor should contact the institution and ask them for the details of the allegations. The editor is perfectly within his rights to push the institution for this information. It is critical for the editor to know why AP wants the paper withdrawn before he can make a decision. Another suggestion was for the editor to respond to AP asking her for her scientific reasons as to why the paper should be withdrawn.

**Update:**

The editor contacted AP's university for additional information regarding its inquiry but was provided with nothing useful. In essence, the editor was told that the inquiry had found no evidence for scientific misconduct.

At this point, with no firm evidence of misconduct, the journal was inclined to move forward with the paper. The editor told AP that she had one last chance to provide specific details regarding the nature of her disagreement with PD. It turns out that it involved two data points (out of 22) in a supplementary analysis. AP and PD could not agree on the value of those data points.

The editor then suggested to AP and PD that they re-do the supplementary analysis, based on the 20 scores that were not in dispute; if the pattern reported originally was confirmed in the new analysis, then the journal would publish the paper.

The editor has just received the revised manuscript reporting these new analyses; it looks as if the results replicate and that the journal will be able to publish the paper.