



Forum agenda

**Meeting to be held on Tuesday 11 September 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. Forum discussion topic: Publishing offensive material

<http://publicationethics.org/survey/cope-survey-publishing-offensive-material>

3. New cases

12-13 Unethical private practice (EC)

12-14 Duplication of data (JH)

**12-15 Possible violation of the Helsinki Declaration on
Scientific Research with Humans (JH)**

12-16 Compromised peer review (unpublished) (TS)

12-17 Anonymity versus author transparency (AW)

12-18 Extensive publication errors. Should we 're-publish'? (EG)

12-19 Complaint regarding letters to the editor (FM)

3. Updates

11-27 Author creates bogus email accounts for proposed reviewers

12-02 How to correct an incorrect decision to publish a flawed paper

12-05 Correcting errors versus privileged information

12-08 Alleged misuse of confidential information

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NEW CASES

12-13 Unethical private practice (EC)

This single author manuscript describes the treatment of 300 women with psychological problems. The women were randomised to either therapy or pharmacological intervention, and this study reports the relative effectiveness of these strategies.

At submission, the manuscript did not contain any mention of ethics approval, consent or trial registration. When the author was queried on these issues, he claimed that the study was performed ethically, but that he did not have ethical approval because he did the study privately, and is not associated with any organization or institution. Since he had listed his affiliation with university X, we questioned how he had access to these patients. The author responded that he runs a private practice, and that he wanted to mention university X out of “personal interest”, and to please remove the name of the institution from his submission.

This author does not have a Scopus or PubMed record, or an institutional email address. Upon searching for his email address in Google, we identified two papers that appear to have been published by this author in journal A and journal B, neither of which are indexed in Scopus or PubMed.

We have contacted university X through their general email address to alert them of this individual, since he appears to be using their name in an unauthorized context, but have not received a response. We contacted the author to express our concerns and inform him that we would keep his file open until the issue has been resolved, but received no response. We attempted to contact the author’s medical council to report this individual, but were unable to find contact information as their website is not in English. We also contacted journal A and journal B to notify them of our concerns, in the event that they also wished to investigate this author; neither journal has responded.

Two months later, we re-sent the notification to university X general email address and received an out of office response.

We would greatly appreciate any suggestions on whether there is anything further we can do other than to reject the manuscript.

12-14 Duplication of data (JH)

It was brought to our attention that there is considerable overlap and duplication of data in two papers that a group of authors submitted and that were subsequently published in two different journals.

The control groups are identical in the two papers although it is claimed that they were matched controls.

The data in several columns in the tables are identical; one figure has been reproduced.

The response from the authors was as follows.

Regarding the considerable overlap and duplication of data, I'd like to explain this:

- In both papers the control group is the same.
- One figure has been reproduced in both papers. The aim of including this figure in each paper is different, being in one case to show the difference against one option and in the other case to compare against a different technique, with their respective different discussion, implications and discussion.
- Having this in mind, it is our opinion that this cannot be considered redundant publication as the rationale behind each of the papers aims to address a different hypothesis and the discussion dealing with the explanation of the results is largely different. Moreover, in the results section, representing the bulk of the achievements of our research, only one figure out of six is present in both papers.
- Both papers are clearly different in their scope, bulk of results, discussion and clinical implications.

The Editor of the journal is not happy with this response, feeling that the author has explained why they did it but does not seem to be concerned that they did it. The editor and publisher of the other journal involved have not responded to our emails.

We would like some advice from COPE on where to take this case and whether retraction is fair.

12-15 Possible violation of the Helsinki Declaration on Scientific Research with Humans (JH)

A manuscript underwent peer review and the resulting reviewer comments raised grave concerns about the ethical legitimacy of the study.

The reviewer:

- questioned the authors' impartiality, suggesting that there was an undeclared conflict of interest;
- raised serious concerns about the extent to which participants gave informed consent;
- strongly doubted that the study would have gained ethics committee approval; and
- suggested that the study violates the World Medical Association's 1964 Helsinki Declaration on Scientific Research with Humans, which states that the welfare of the research subject takes precedence over the interests of science and society.

The manuscript described a randomised controlled trial conducted on patients undergoing brain surgery. Participants were recruited between January 2010 and February 2012 in a single hospital.

The stated aim of the study was to establish the effect of varying the time for which blood flow to the brain is substantially reduced during surgery on postoperative function and disability. It is already well established that reducing the blood flow in this manner for a prolonged period of time is likely to lead to poorer postoperative outcomes due to reduced oxygen supply to the brain.

In order to avoid negative outcomes and improve patient safety, there already exists a warning criterion used in surgery of this type, which alerts the surgeon to the extent of oxygen starvation and indicates when blood supply should be restored.

In the manuscript, the authors outline their knowledge of the risks involved with the procedure itself and report that many surgeons feel that this procedure should only be used as a last resort due to the increased risks outlined above. However, if the procedure is deemed necessary, the risks can be reduced by employing the pre-established warning criterion.

The authors outline perceived flaws of the pre-existing criterion and go on to describe a novel warning mechanism—conceived by them—that they intend to test during the study. Testing this new criterion entailed increasing the time that blood supply in the brain is reduced—to the extent that, in some cases, the current recommended threshold was significantly exceeded. Additionally, while the procedure was underway, the authors ensured that the surgeon was blinded to the patient's status with respect to the established risk threshold, making it impossible for them to gauge the level of danger the patient was being exposed to. This, as is noted within the manuscript itself, poses serious risk to the patients.

Participants were divided into three groups. In the control group risk was assessed using the normal criterion and blood supply was reinstated once this threshold was crossed. In the second group, blood supply was reduced for a longer period of time. In the third group, blood supply was reduced or completely cut-off for significantly longer.

In the results section, the authors state that in the control group one patient became severely disabled, and that all patients in the third group experienced negative outcomes. Of the patients in the third group, three were severely disabled as a result of the procedure and all of the others suffered some neurological deficit. As the reviewer points out, the pattern of outcomes raises serious ethical concerns about the study. The reviewer questions whether this was (and even should have been) tested on animals first, as this would have provided an upper limit and 'red flag' for testing.

The reviewer believes that this study violates the Helsinki Declaration on Scientific Research with Humans in that it appears that subjects randomly included in the third experimental group were subjected to an experimental condition that caused three of them to be severely disabled, and nine of them to be moderately disabled by the experimental manipulation. Because patients were assigned to intervention and control groups randomly and the outcomes between the groups differed significantly, it is highly unlikely that the negative outcomes among group 3 patients are attributable to technical surgical problems in one group only.

It appears that the negative outcomes found in group 3 patients were due to the blood supply being reduced or occluded for long periods of time. The reviewer also suggests that for the control group, there was an omission of a standard of care practiced by most modern medical centres that perform this procedure. The reviewer suggests these findings cannot be generalised to other procedures of this type and he wonders why the original warning criterion was not utilised in this study to further protect these patients.

Furthermore, the study raises serious concerns about the extent to which patient consent (for which consent forms were obtained from the patients) was properly informed. There is no indication that the risks to the participants inherent in an experiment such as this were properly conveyed to the individuals before participation was agreed.

Finally, the Declaration of Helsinki also states that "Physicians must immediately stop a study when the risks are found to outweigh the potential benefits..."; the authors did not stop this study for over a year. And, an unproven intervention should only ever be carried out if "it offers hope of saving life, re-establishing health or alleviating suffering" and that in the nature of research it is "designed to evaluate its safety and efficacy" not to establish a benchmark.

12-16 Compromised peer review (unpublished) (TS)

A manuscript was flagged to editor X as having received reviewers' reports indicating very high interest. At that point the manuscript had been through one round of review, revision and re-review, and all three reviewers were advising that the manuscript be accepted without further revision.

On checking the credentials of the three reviewers, editor X was unable to find the publication record of any of them. All three reviewers were found to have been suggested by the authors. Institutions were given for the suggested authors but the supplied email addresses were all with webmail services. The reviewers were found not to exist.

Associate editor Y had invited the author suggested reviewers and two of their own choosing, neither of whom had replied to the invitation.

After it was determined that the reviewer suggestions were faked, a previous publication by the same authors with the same 'fake' reviewers was identified.

Following the recommendations of COPE regarding a recent similar case discussed at the COPE Forum (case number 12-12), all of the authors were contacted to ask if they could supply more details of the suggested reviewers, but they have not responded. We have attempted to find a contact at the authors' institution. It has proved difficult to identify a research ethics committee, any individual senior member of the university or contacts for the university administration. During other searches, a vice principal of the university was identified but was found to be the senior author on both manuscripts.

We are now seeking guidance on the best course of action with regard to both the unpublished and published manuscripts, in the absence of any response from the authors and no reliable contacts at the authors' institution. Our current intention is to reject the manuscript under review and issue a retraction of the published article.

12-17 Anonymity versus author transparency (AW)

An editor invited an author to submit a paper to his journal. Colleagues of the author suggested “unsubmission” because it could be damaging to the author’s career. The editor contacted the publisher and requested that the paper be withdrawn. The editor then contacted the author asking if he would consider publishing the paper anonymously (ie, with no identifying names). The editor did not consult with the publisher on this matter.

The publisher has placed the paper on hold, seeking guidance on author transparency. Publishing anonymously is typically not permitted by the publisher because of concerns about author transparency and because the publisher believes that they should publish in the highest ethical regard. There are no patient confidentiality or privacy issues associated with this case.

The publisher would like the Forum’s view on the balance between anonymity and author transparency. Should there be a set policy in place?

12-18 Extensive publication errors. Should we 're-publish'? (EG)

In March 2012, our journal published a posthumous excerpt of a book by a prestigious scholar, who had died before completing the book. We chose to publish because the unfinished book represented the scholar's life work, and would not find another publication venue. The excerpt included a number of large figures, which we also published.

At our publisher, we had a new production team, and they had a very difficult time getting the issue out, but there were an exceptional number of errors introduced to the posthumous excerpt during the layout process. (Our upload to the publisher was free of errors, and we have documentation to establish that this was the case. It appears that a considerable amount of information was stripped from the file during our file export process. We had used the publisher's software to do the upload, and followed their procedures throughout the production process. We forwarded documentation establishing the fact that our upload was clean to our production editor.)

We received the first set of proofs and made all corrections. At that time, because of the extensive number of changes required, we requested that we have the opportunity to review a second and third proof. When we received the second proof of the excerpt, some corrections had been made, but other, new errors appeared. We made all of the required corrections and forwarded them to our publisher's production editor. The production editor acknowledged receipt of the corrections to the second proof. When we repeated our request to see a third proof, the publication editor assured us that all corrections would be made before publication.

When the excerpt was published, some corrections appeared, but new errors were added to the earlier problems. Figures were out of order. Some captions were incomplete, others were incorrect. Paragraphs of text were scrambled and appeared as gibberish. We contacted our publication editor immediately, and requested that we be allowed to correct the article, and have the corrected version appear in the electronic version. Our publisher said that it was impossible to change the electronic version (?!).

Since that time, we have found a new publisher, in part because of the quality control problems we experienced with our original publisher. Now we need to publish an erratum for the excerpt, which will go on for a number of pages. Since our intention was to honour a deceased colleague, would it be appropriate to 'republish' the entire excerpt free of error, so it can appear in the form the author originally intended? My board feels that the extent version of the excerpt is an insult to the author and her family (who kindly edited and prepared the excerpt for publication). The excerpt was originally presented as part of the editorial (non-peer reviewed) content of our publication.

12-19 Complaint regarding letters to the editor (FM)

Our journal routinely sends letters commenting on published articles to the authors of those articles. This gives the authors an opportunity to respond to any criticisms. The letters and the responses are then considered together and we make a decision on which ones to publish.

If a letter is not selected for publication, our usual practice is to send the author's response to the person who wrote the letter to the editor. Most people are pleased that their comments are considered, even if they are not published. It was therefore a surprise when an author complained that his response had been provided to the person who wrote a letter about the article.

The author's complaint was that he had prepared the response for possible publication, rather than as a personal reply. The complaint was not that the letter and response were not selected for publication, but that it was a violation of confidentiality to send the response to the writer of the letter.

Our editorial executive committee thought it strange that the author had privacy concerns about one person seeing the response, but no concerns about the tens of thousands of people who would have seen it if it had been published. Does COPE have a different view?

UPDATES

11-27 Author creates bogus email accounts for proposed reviewers

Anonymised text of the case:

Recently, as co-editor of my journal, I received a manuscript submitted for publication. The author had recommended two reviewers along with their Gmail accounts and affiliations. I was curious about the affiliation of one of the reviewers. I looked this person up and discovered they had a different email address than that provided by the author. So I used the email address that I found to contact the reviewer (reviewer 1). For the second recommended reviewer (reviewer 2), I also looked up a current email address and used the one I found instead of the Gmail address that was provided by the author.

Reviewer 1 responded with the comment that the title of the manuscript looked similar to a manuscript review that he/she had been asked to confirm for another journal (journal 1). Reviewer 1 asked me to contact the editor of journal 1. After contacting the editor of journal 1, I discovered that the author had provided bogus email accounts for the recommended reviewers.

The editor of journal 1 became suspicious of the reviews when he received a review within hours of the request to review the manuscript. It was at this point that the editor of journal 1 discovered that the email addresses provided were bogus.

Reviewer 2 declined my request to review the manuscript. To test that the Gmail provided by the author was bogus, I sent a request to review the manuscript to reviewer 2's bogus Gmail account. Within hours I received a review. I then called reviewer 2 to confirm that he/she had not provided a review. He/she had not, and was very concerned that a Gmail account was created using their name.

In summary, this author has been creating false email accounts for suggested reviewers which are going to unknown individuals who are providing reviews under false pretences and giving inaccurate information as to their identity and affiliation. The outcome of this is that my fellow co-editor and I have banned this individual from publishing in our journal. The author has also been banned from publishing in journal 1. Does COPE have any additional advice on this case?

Advice:

The Forum agreed that this was a serious form of misconduct and may even be criminal, as the author was impersonating the reviewers. The advice was to contact the author's institution and inform them of the situation, explaining the author's inappropriate behaviour. Other advice was to look at the peer review of previous submissions/publications from this author in case they also involved fraudulent reviews. The Forum suggested that good practice is always to check the names, addresses and email contacts of reviewers, and especially those that are recommended by authors. Another suggestion was for the editor to write an editorial on this issue.

Update:

The editor reported that the author had his papers published in another journal. The editor plans to write to the other editor asking him to check the reviewers on the manuscripts.

12-02 How to correct an incorrect decision to publish a flawed paper

Anonymised text of the case:

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?

Advice:

The Forum suggested issuing a notice of correction but the editor should perhaps consult with the publisher's legal department before publication. As the problem occurred nearly 10 years ago, another suggestion was to write an accompanying editorial explaining the whole case. The journal can issue a notice of correction without the approval or consent of the authors, but the advice was to contact the authors in the first instance and try to agree on the wording of a correction that is acceptable to all. The journal could draft the notice and send it to the authors for their comments. If agreement on the wording cannot be reached, the editor could suggest an arbitrator or panel of arbitrators. The editor could also allow the authors a chance to reply or comment further in the journal.

Follow up:

As advised by the Forum, the journal discussed the issue with their publisher's legal advisers and wrote to the authors proposing a notice of correction. They have just received a reply. The editor is trying to avoid involving arbitrators. Meanwhile, the editor has had a new submission from a third party reanalysing the original data.

Follow up (September 2012):

The author has proposed simply updating the table, and has given an explanation of all the inconsistencies except one. The faults in the paper have moved into the area of what the editors consider to be poor scientific judgement rather than deceit or factual error. The editors regret that these questions of judgement were not dealt with before publication, but in view of the age of the paper they have decided to accept the author's proposal just to correct the table, and to leave discussion of the paper's conclusions to other authors.

12-05 Correcting errors versus privileged information

Anonymised text of the case:

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 authors 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?

Advice:

The Forum suggested that an addition could be made to the correction, citing author A's published work. The journal should take some of the blame for not finding an acceptable solution for the publication of author A's original critique and perhaps issue an apology to that effect

Update:

The Editors are grateful for COPE's advice, which they followed. An addendum was published which (1) cited author A's published work and (2) expressed regret that the editors

were unable to agree with author A on an acceptable solution for the publication of A's original critique in the journal.

12-08 Alleged misuse of confidential information

Anonymised text of the case:

In early 2012, author A submitted a paper reporting on the gene mutated in a rare syndrome seen in a specific population. The paper was citing an earlier (2006) report by author B that had mapped the disease locus to a narrow chromosomal location but had stopped short of actually identifying the gene (which would have been laborious by the technology available at the time).

Author A's submission independently replicated the mapping data of the earlier paper and proceeded to identify the gene by exome sequencing, a technology that had become widely available since the publication of the mapping paper in 2006. Since the mapping was independently replicated, the methodology used would have been sufficient to identify the gene whether any prior knowledge was available or not.

Author A requested that the editor exclude author B as well as author C (another researcher from a different institution) as reviewers. Because the earlier paper made it clear that author B was a competitor, and external referee expertise was readily available elsewhere, the request was granted. After some minor revisions, the paper was accepted for publication in the journal. On the day of acceptance (the timing being pure coincidence), two papers by authors B and C were published online in a different journal, reporting the same gene discovery, plus some functional data about the gene.

Author A's paper appeared a month later and, shortly thereafter, the editor received an email from author B, requesting that author A's paper be retracted. It was alleged that the work reported by author A had inappropriately used information, given to him confidentially by author B, in 2009. The information consisted of disclosing the identity of the mutated gene that author B had already discovered at the time but was not publishing, waiting for the functional studies to be completed. Author B alleges that the information had been confidentially given to author A at a closed meeting, to help in the clinical management and genetic counselling of the patients. No non-disclosure agreement or similar document was signed. It was also alleged that author A's group included in their study three of the patients that they knew were already studied by author B.

The confidential nature of the meeting, attended by several physicians and researchers, is in dispute. Author A, in response to a request by the editor, said that the meeting was open to all but could not provide any copy of a public announcement. Author A learned about the meeting from a private email (from one of the attendees, not directly from author B). The email, a copy of which was provided to the editor, does not mention confidentiality but may be interpreted as an invitation to collaborate. Author B and his collaborators say that the meeting was by invitation only and the confidential nature of its content was made obvious. The clinician collaborators of author B did not respond to repeated requests by the editor to identify the three overlapping patients by their pedigree IDs in each of the two papers.

In his response to a query by the editor, author A stated that in 2006 (and, therefore, prior to the 2009 meeting), he had obtained funding to identify the gene. One fact is clear: in 2011, knowledge of the mutated gene was not necessary to perform the work reported by author A.

The work reported by author A addresses the question “from scratch” using hypothesis-free methodologies and requires no prior knowledge.

The editor believes that, although the behaviour of author A may not have been the most collaborative and collegial, no misconduct justifying retraction of the paper has been committed.

The opinion of COPE would be highly valued in resolving this matter.

Advice:

The Forum agreed that as the paper is scientifically sound, there are no grounds for retraction. It is important not to retract a paper where the data are not in question. Although author A may not have behaved well, there has been no misconduct. If the paper could have been written without the information gained from attending the meeting, then the complainant has no grounds for complaint.

The Forum suggested that the editor should invite author B to write a letter to the editor for publication in the journal. The readers will then be alerted to the issue and author A will have the opportunity to respond. The editor may wish to tell the authors that he has sought the advice of COPE.

Update:

The editor communicated the advice from the Forum to both authors. Author B and his co-authors decided not to write a letter on the matter.

12-09 Authorship dispute

Anonymised text of the case:

A manuscript was published by journal X and submitted by author A (last author). Author B claims that fraud occurred in relation to authorship for the following reasons.

- (1) Author A did not take part in producing the data for the paper and has never been a co-author on any version of the manuscript.
- (2) A paper with very similar content, which was part of the PhD thesis of author C (first author), was accepted for publication in journal Y.
- (3) The figures in the paper published in journal X were identical to the figures in author C’s PhD thesis.
- (4) The name of author B was misspelt in the paper published in journal X to avoid identification of the article search in PubMed.

The editor of journal X contacted all of the authors by email and they responded as follows: author C (first author), author D and author E agreed with author B (claiming author). Author F did not respond, despite receiving five emails.

In addition, author B sent us a letter signed by the Vice-Rector at his University, agreeing and supporting the point raised by author B.

Author A (last author) disagreed with all of the allegations and pointed out the following.

(1) Author A declared that he was the principle investigator of the project in country Z during 2004–2009, and the role of author B was to help in the analysis of the samples in his laboratory, located in country W.

(2) Author A submitted an official complaint to author B's university, alleging that they (authors B, C and D) had no right to use data without notifying or asking his permission. In addition, they did not have any patient consent.

(3) The paper published in journal X was the original manuscript and it was circulated to all of the authors.

(4) The name of author B was misspelled in journal X by mistake.

Author B requests that the paper must be retracted from journal X, and he also demands that the editors ensure that the paper will disappear from PubMed.

In summary, all of the authors confirm that the data are correct but they disagree regarding the issue of authorship?

Advice:

Because of the quality of phone line, it was not possible to discuss the cases at the forum. Council instead gave the following advice on this case.

All agreed it was a complicated case and it would be useful to know which paper was submitted and published first, X or Y, although it seems likely it was X. Also what is the role of author F, is he/she affiliated with the institution of author A or authors B, C and D? Before dealing with the authorship dispute, it is essential to confirm whether patient consent was required and obtained. There are grounds for retraction of the paper if the study was not conducted ethically. Is journal Y aware of this dispute? As both parties disagree on the fundamental points, such as whether author A was involved at all in the study, official documentation and, if available, email conversations need to be produced to consider how to proceed. Hence more information gathering and a request for a formal investigation by the institution should be undertaken to find out exactly what the real issues are first. It might be useful for the journal to check or ask for information from both parties on national regulations and institutional policies for transfer of biological material and data sharing in this case of collaborative research.

The paper cannot “disappear” from PubMed, but at this point, most agreed that an “Expression of Concern” should be issued immediately and the editors need to investigate further the issues of (1) patient consent, (2) overlapping content and (3) the roles of each author on both manuscripts. Institutional input is also needed and the editor should clarify if the statements from the university vice rectors resulted from a formal inquiry.

The ethical issue can be compounded by the policy by some universities that if they conduct research outside of their own country, they require dual ethical approval by (1) that university and (2) whatever ethical system is in place in the countries where the research is undertaken.

Update:

The editor published an expression of concern in his journal, stating that one of the authors had questioned the authorship of the corresponding author. The submission is on hold and the authors have been all informed of the claim. The investigation by the journal has not yet

reached a conclusion. Pending the results of the investigations, the journal decided to publish an expression of concern to alert readers to the fact that serious questions have been raised about the authorship of the paper.

12-11 Meta-analysis: submission of unreliable findings

Anonymised text of the case:

A meta-analysis was conducted of about 1000 patients included in a number of small trials of a drug for emergency management administered by route X compared with route Y. The report concluded that administration by route X improves short term survival.

Chronology

1. The paper was submitted to our journal in September 2011 and after peer review was returned to the authors for revision in November 2011.
2. In the letter sent to the authors, the editor stated: “Before coming to a final decision on your paper we will need to see your responses to our referees' comments. We will also need you to discuss the preliminary results of the large randomised controlled trial (RCT) recently presented at a national meeting which conflict with and may negate the conclusions of your meta-analysis.”
3. The revised version was sent back to us in January 2012. It contained only one mention of the large RCT without quoting any of its findings. The covering correspondence discussed the RCT findings that had been recently presented and speculated as to why they appeared different from the findings of the meta-analysis.
4. We accepted the meta-analysis in January 2012. We considered that the differences described by the authors were irrelevant, because the large RCT had not, at that time, been published in a peer-review journal and the only information available was from data presented at a meeting.
5. We now know that the authors of the meta-analysis were fully aware of the findings of the large RCT at the time they submitted the revision because the RCT paper had already been accepted by a high profile journal and the lead author was co-author on the meta-analysis submitted to our journal. None of this was revealed to the journal prior to accepting the meta-analysis
6. In March 2012, the high profile journal published the large RCT which randomized more than 2000 patients to drug treatment by the two different routes. The main conclusion was of no difference in survival for route X versus route Y. This finding rendered meaningless the finding of the meta-analysis accepted by our journal 6 weeks previously.
7. The authors of the meta-analysis were then emailed asking if they would now update their meta-analysis with inclusion of the RCT data.
8. The response was negative but an email from another co-author (who wrote the editorial accompanying the RCT in the high profile journal) agreed “it makes no sense to report a meta-analysis claiming death reduction considering available data”. He then copied us in an email he had sent to the lead author of the meta-analysis in January 2012, before it was sent back to our journal: “just to let you know that I am finishing an editorial on (the RCT) which will likely come out very soon with the main Ms....I would suggest that you try to include (the data from the RCT) into your meta-analysis ASAP”
9. The authors chose not to include the data from the RCT in the revised version of the meta-analysis they submitted to our journal, even though they had available those

data. Since then the authors of the meta-analysis have steadfastly refused to update their paper. Meanwhile the editorialist for the high profile journal has asked that his name be removed from the meta-analysis in our journal.

The authors of the meta-analysis, one of whom was the lead author of the high profile journal report, had full access to the RCT data at the time they were preparing their revised paper for our journal. They knew that the main finding of the RCT contradicted the conclusion of their meta-analysis and ignored the suggestion of a co-author (the editorialist) to include the RCT data in their revised paper to our journal.

COPE states that journal editors should consider retracting a publication if they have clear evidence that the findings are unreliable. The authors of the meta-analysis knew their findings were unreliable at the time they submitted their revised paper and we now wish to have the paper retracted

Advice:

The Forum agreed there were grounds for retraction of the paper. Clinical decisions are often based on meta-analyses and the editor cannot rely on all readers being aware of the newly published meta-analysis in the other journal. However, the ideal situation would be for the author to correct the published paper. Although the author has refused to do this, the Forum suggested that the editor should contact the author again, asking him to correct the paper. The editor should tell the author that if he refuses to correct the paper, then the editor will be left with no option but to retract the paper.

The Forum suggested that the fact that the editor did not ask the authors to wait until the results of the RCT were available before submitting their final paper has contributed to the confusion surrounding the case. Going forward, the editor should consider revising journal policy to request authors to send any related papers under submission to them when they submit an article.

Update:

Following the Forum's advice, the editor emailed the corresponding author of the paper, copying in the co-authors, stating that he hoped the authors would agree to update the meta-analysis whereupon the matter would be concluded. He told the authors that if they did not agree to provide an update, he would retract the paper. The editor received no reply and therefore retracted the paper.

The retraction notice stated that the findings of the paper were unreliable because they failed to address data from the large RCT, to which the authors had access prior to submission and which contraindicated the paper's conclusion. The notice said that authors were asked to update the paper to include the RCT findings but, with the exception of one of the authors, they declined. Owing to this difference of opinion, this author asked to be removed from the list of authors, a request to which the journal acceded. The notice stated that under these circumstances, the matter was considered by COPE who recommended retraction and this paper has now been withdrawn.

12-12 Compromised peer review system in published papers

Anonymised text of the case:

On noticing a high volume of submissions from corresponding author A, editor X flagged up concerns with the preferred reviewers being suggested and their comments. Author A had in most cases suggested the same preferred reviewers for each submission, preferred reviewer accounts had non-attributable email addresses, comments were being returned very quickly (within 24 hours) and were often brief and positive, largely restricted to grammatical changes. All preferred reviewers favoured immediate acceptance or acceptance subject to minor revisions.

Author A was asked to provide further information on the preferred reviewers and admitted that these were either dummy accounts or associates of author A. The dummy accounts had email accounts accessible by author A and/or author A's students or collaborators. Author A asked the preferred reviewers (or the people behind the accounts) to submit favourable reviews of the papers and turn them around quickly or author A submitted the reviews via the dummy account. Author A admitted employing this system for a number of papers, but not every paper, although we found similar patterns of peer review activity for these also. Author A states that the papers' co-authors were not aware of this activity.

Author A has agreed to retract published papers for which they admit to influencing the peer review process and we are planning retraction notices for these. We are now seeking advice as to what to do about the remaining published papers to which author A has not admitted influencing the peer review process. We suspect the peer review of these other articles was compromised through use of preferred reviewers suggested by author A, but we have no evidence that these preferred reviewers used dummy accounts or that the article content is flawed. We are therefore considering issuing an expression of concern for these papers.

We have attempted to make contact with all co-authors to explain the problem and seek their approval for the chosen course of action. Only three co-authors have responded, two supporting our decision, and one (whose paper we planned to issue an expression of concern for) replied that it was unfair on the co-authors without any concrete evidence. Author A's institution has been contacted but we have received no response.

We respectfully ask COPE to provide advice on managing those papers author A has not agreed to retract and, in particular, the case where a co-author disagrees with our intention to issue an expression of concern

Advice:

The Forum agreed that there are many issues involved here, not least a serious form of misconduct which may even be criminal, as the author was impersonating the reviewers and committing fraud by using colleagues as false reviewers and, possibly impersonating other reviewers. In addition, as the author has admitted fraud, can the editor trust the validity of any of the papers?

The advice was to contact the author's institution and inform them of the situation, explaining the author's inappropriate and possibly criminal behaviour. The editor should also contact the reviewers who were the associates of the author who provided favourable reviews and contact their institutions.

The Forum advised re-reviewing the remaining published papers to which author A has not admitted influencing the peer review process. If the journal wishes to stand by these papers, then it is essential that all of the papers are re-reviewed. In the meantime, an expression of concern should be issued for all of these papers. One suggestion was to inform the author of the course of action that the journal is going to undertake and see if he wishes to retract all of these papers.

The Forum noted that the journal should take some responsibility for failure of their peer review system. Good practice is always to check the names, addresses and email contacts of reviewers, and especially those that are recommended by authors. Editors should never use only the preferred reviewer.

Update:

The journal has now published (or are in the process of publishing) retraction notices for all of the papers that the corresponding author agreed to retract. The journal has taken the suggestion from the Forum of 're-doing' the peer review process for the other papers seriously and are planning on doing this

The institution contacted us and wants to discuss the details of what we found out as they investigate the author.

There are still two outstanding issues: (1) the other non-retracted papers; and (2) the decision of the author's institution on what action they will take.