



Forum agenda

Meeting to be held on Monday 6 June 2011 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. New cases

- 11-07 Retractions of primary literature papers: what reaction from a reviews journal? (AM)**
- 11-08 Late introduction of an omitted author after online publishing (AMK)**
- 11-09 Mislabelling/duplicate images (SA)**
- 11-10 Duplicate publication and alleged image manipulation (JM)**
- 11-11 Lack of trial registration leads to new concerns about study conduct and ethical review/approval (EV)**
- 11-12 An enquiry about arbitrating reviewers (JA)**
- 11-13 Case of figure duplication and manipulation involving two journals (CH)**
- 11-14 Is this previous publication? (CG)**
- 11-15 Disagreement between authors and sponsor (GW)**
- 11-16 Seeking retrospective ethics approval (NB)**

3. Updates (editors would like further advice)

- 11-05 Data fabrication, lack of ethical approval, withdrawal of paper and publication in another journal (MT)**
- 10-13 Suspect author (SY)**

4. Updates

- 10-15 Inadequate reporting of a trial, despite earlier rejection from a different journal**
- 11-02 Author of rejected letter blames global bias against his message and undisclosed conflicts of interest**
- 11-03 Plagiarism of reviewer's work**

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NEW CASES

11-07 Retractions of primary literature papers: what reaction from a reviews journal? (AM)

In a recent and very prominent case of publication misconduct resulting in the retraction of 12 research papers (to date), many journals have been included in ‘round-mails’ from the whistleblower and other scientists. Our journal (a reviews and features journal) has published a review from the main author associated with the misconduct, which contains reference to six of the retracted papers.

As editor, I have been urged a number of times via email (the true sender of which was not always clear) to retract the said review. At first—before the full extent of the retractions was confirmed—I could do no more than wait, rather than react prematurely, although even at that stage, tracking and trying to verify the claims took some time. Now I have analysed the review—which took a significant chunk of time—and identified the parts that cite the six references. Those constitute around 18% of the body text, mainly the more novel insights.

Qualitatively—and that is clearly more important than quantity—it is exceedingly hard to judge whether the retraction of the six articles nullifies those conclusions and insights. I should furthermore assume that the rest of the review is in order unless (till) proven otherwise. In fairness, I think that if any corrective measure is to be taken, it amounts to a corrigendum noting that the said references have been retracted.

However, I am concerned about the additional workload that investigating the impact of retractions could have if we generally adopt the procedure of publishing corrigenda for every article that they affect. An alternative way of looking at the problem is to acknowledge that retracted references are registered as ‘retracted’ in the scientific indexes (although only if the journal concerned is indexed), and in the venues of publication, and hence on tracing a reference to its source, the reader of the review in which it is cited will see that the particular section of text is no longer supported by a published article.

I am in a quandary between providing the most up to date information in immediate connection with an article and getting into something that could consume significant amounts of already very stretched editorial resources, and then, more importantly, require further corrigenda on the same article if more references are found to be faulty at some point in the future: a seemingly never-ending story...

What is COPE’s advice?

11-08 Late introduction of an omitted author after online publishing (AMK)

The corresponding author as well as the co-authors admit that they have omitted one author (author X) who was responsible for a major part of the work in their study.

Author X insists on having full acknowledgment in the authorship line and would not be satisfied with an erratum. What should I do? Please advise also about whether author X will appear in different citation trackers as one of the co-authors.

11-09 Mislabelling/duplicate images (SA)

We were contacted by a reader who told us that he had spotted a number of cases of image duplication and mislabelling of fluorescent tags that had occurred over the past 4 years. These involved two papers published in our journal, and two other papers published in two different journals. The two papers in our journal were both reviews, and the one that had the most occurrences involved a poster (associated with the review) that we had recently published. Although each paper had different authors, there was one author, author X, common to all the papers. The reader provided extensive evidence.

I checked all the evidence myself, looking up the original sources and concurred with the reader that there was at least image duplication (I could not verify myself whether mislabelling had occurred). I also discussed the case with our publisher. I then contacted the head of author X's institute, copying in author X and his co-authors on the poster article. I told the institute head that I was making no assumptions about wrongdoing but presented the evidence and asked for an explanation.

The matter was referred by the institute head to Dr Y, the Associate Vice President for Research of the university, who appointed a Committee of Inquiry. This committee found reason to launch a full investigation, and so an Investigatory Committee was appointed. I was told that I would receive their report.

While I was waiting for their decision, the editors of the two other journals in question approached me to ask that I keep them informed. I told them about the Investigatory Committee and recommended that they contact Dr Y if they wanted to be kept abreast of the outcome.

Almost 2 months later, I received an email from author X, copied to Dr Y, with an explanation of what had happened. He could not provide an explanation for the error in the first paper. There was one image in that paper that claimed to show something labelled with a particular fluorescent tag, but a different, although similar, tag was in fact used. He said he spoke with his co-authors, who agreed that the error did not affect the scientific conclusions of the paper and that he could either correct the tag name or provide a new correct image.

For the poster, he said that there were a number of images mislabelled, and that this occurred, in essence, because he used 'placeholder' images while creating the poster but forgot to replace them with the correct images. He offered to redo the poster with the correct images and also wanted to replace some other images that were correctly labelled because he had 'better versions' of them. He assured me that the text did not need to be changed and again said that his co-authors agreed that the scientific conclusions of the paper were unaffected.

After again conferring with our publisher, we have come up with a plan:

- I have contacted Dr Y and asked him to verify that the Investigatory Committee has accepted author X's explanation and found no evidence of fraudulent intention.
- I have asked author X to have his co-authors on both papers contact me directly to confirm that the scientific conclusions of the papers were not compromised and that they are satisfied with the replacement images author X is proposing.
- I will ask author X to provide a replacement image for the first article.

- I have told author X that it is not acceptable to replace correctly labelled images with 'better versions' and that we will only deal with those that are incorrectly labelled.
- I plan to issue corrections for both papers. On the poster article, because it involved 10 images, I also plan to include, with the correction, a statement to the effect that a committee was appointed by author X's institute to investigate the mislabelling and that they found no evidence of malicious intent (I'm wary of the wording I use here).
- I also want to reprint the poster and send a copy of it to each of our print subscribers as they will have received a copy of the incorrect version. I am talking to our online hosts about how we can provide a link to the corrected version of the poster because, although I feel strongly that the original should remain online as it is, a correct version should be available.
- I will ask author X to cover all costs associated with the redesign, printing and mailing of the poster, in addition to the costs of the corrections themselves.

I am not convinced by author X's explanation and did look to see whether there were any other published corrections associated with author X's previous publications but did not find any. If the Investigatory Committee confirms that they did not find evidence of fraudulent intent, however, then I feel I have to accept that decision and will proceed according to our plan outlined above. I would very much appreciate COPE's advice on how we have handled the situation so far, and whether our plan of action could be/have been improved.

11-10 Duplicate publication and alleged image manipulation (JM)

The editorial office of journal A was contacted anonymously by an individual who made allegations against two papers, both published by the same author. Paper 1 was alleged to be a duplicate publication, with the paper previously having been published in journal B. The editorial office of journal A, in accordance with the COPE flowcharts, contacted the author informing them of the allegations and asking for a response/explanation. The author freely admitted to the duplicate publication and paper 1 is in the process of being retracted.

The allegation against paper 2 was image manipulation consisting of false bands being included in an assay figure. The editorial office reviewed the images and believes that they have been manipulated. As such they would like to retract the paper from journal A. When the author was contacted he admitted that some of the figures had been made by copy/paste but he maintained that the conclusions of the article are correct.

Numerous other journals have been contacted about similar alleged misconduct by this author, who believes that the accusations must be politically motivated.

The editorial office carried out further investigation of other papers published by the same author in journal A and found another instance of apparent image manipulation (paper 3). When contacted, the author made the same statement as above. When a co-author of this paper at another institution was contacted for his position, he defended the corresponding author and stated that it was inconceivable to him that serious claims of scientific misconduct made by an unidentified accuser could be applied here. Moreover, he claimed that none of the experiments were performed in his laboratory.

As the author denied any real wrongdoing, but to the editorial team it seems quite obvious that the figures have been manipulated, the editors of journal A contacted the author's institution and asked for a response within 3 months. They have not received a reply from the institution.

The editorial team is certain that the figures have been manipulated to provide false data and would therefore like to retract the relevant articles (papers 2 and 3) despite the author's denial of any wrongdoing and no contact from the author's institution. In consultation with the publisher for journal A, it is felt that the best way to proceed would be to issue a non-specific notice of retraction for the relevant papers, thus avoiding potential charges of defamation.

Would the Forum agree with this course of action or does it have any other views?

11-11 Lack of trial registration leads to new concerns about study conduct and ethical review/approval (EV)

Following publication of an article, the editors noticed that the paper reported results of a clinical trial, but no details of trial registration were included in the article. (The journal does have careful checks on trial registration by staff at submission but this paper was not well written and it took careful reading to work out that it did in fact report on a clinical trial).

We contacted the authors to request details on trial registration and the study protocol, which are journal requirements for clinical trials. The senior author (co-chair A of his institution) eventually acknowledged that the trial had not been registered and they provided a document for the study protocol which we considered unsatisfactory. The authors at that stage requested 'withdrawal' of the article.

We replied to the authors to indicate we had the intention of issuing a notice of concern based on lack of trial registration. Given the new issue about the lack of a satisfactory protocol for the study reported, we then requested a copy of the letter of approval issued by the ethics committee/institutional review board cited in the article. Around this time, we were spontaneously contacted by co-chair B of the authors' institution (who is not an author on the paper) expressing serious concerns over the work reported given that:

- (1) they considered the description of the work as potentially misleading given it fulfilled the criteria for a clinical trial; and
- (2) the ethics committee/institutional review board cited in the article does not exist at the authors' institution.

Co-chair B indicated they would like to remain anonymous but would be willing to publicly outline their concerns as part of an internal investigation if a formal request for an enquiry was sent to the institution. We did not hear back from the authors for some time after our request for a copy of the ethics committee approval, so we posted a comment on the article to make the readers aware about the fact that the article did not adhere to the journal's requirements for clinical trials and replied to the authors to explain the implications of a retraction—versus a 'withdrawal'—and also reiterate the request for the letter of approval issued by the ethics committee.

In a further development, the authors finally then contacted us to say that the study was not in fact done at their institution but in a separate part of the country, that they did have approval from an ethics committee other than the one stated in the paper and provided a copy of that ethical approval in a non-English language. They provided no explanation for the discrepancies in study setting and ethical approval. It is not clear to us who recruited the participants as no authors seem to be based at the institution now claimed as providing ethical approval.

We have been unable to independently verify the authenticity of the claimed ethical approval and have concerns about the discrepancies in the various accounts of what took place. We think it is important to retract the paper and propose to do that, highlighting the lack of trial registration, lack of an appropriate protocol and that the ethics committee named in the article did not seem to give approval.

Is it adequate to proceed to retract the article on the basis of the concerns raised and the lack of an adequate response from the authors, given that we have been unable to establish

whether the trial did or did not receive ethical approval? We also propose to submit a formal request for investigation to co-chair B of the institution (who currently wishes to be an anonymous complainant) and update the retraction later should we receive further clarification from them.

11-12 An enquiry about arbitrating reviewers (JA)

Under certain circumstances, the editors of journal A use ‘arbitrating’ reviewers. These reviewers advise an editor where, for example, an editor has split reviewer reports or a rebuttal to a decision that was based on split reviewer reports. This reviewer has sight of the other reviewers’ reports as he/she both evaluates the manuscript and assists the editor, through their advice, to arrive at an informed editorial decision. Historically, the use of arbitrating reviewers has arisen out of a discussion between an author and the handling editor, with their use being at the discretion of the editor. More recently, the use of arbitrating reviewers has become more routine and has not always involved a discussion with the authors concerned.

The journal’s editors are currently reviewing this peer review option, partly in response to an author’s complaint that arbitrating reviewers bias the peer review process. We would welcome the committee’s feedback on the use of arbitrating reviewers and to have their advice regarding any additional safeguards the journal should put in place when using these reviewers to maintain unbiased peer review. We will use this advice to formulate a new set of editorial guidelines on arbitrating reviewers.

11-13 Case of figure duplication and manipulation involving two journals (CH)

The editors in chief of journal A and journal B, both owned by society C, received a letter from the last 'senior' author, also the corresponding author on one of the papers (author D), concerning separate papers published in both journals (paper E published in journal A and paper F published in journal B), informing them that one of the co-authors on both papers is under investigation for scientific fraud. The results of the investigation are expected to take a further 6 months to complete. The case has, however, attracted some press attention and there is a danger that the journals will be brought into disrepute by association. Author D listed the figures in both papers E and F which the other co-authors had determined, on subsequent examination, to be fraudulently manipulated and/or duplicated and asked for advice on further action. Figures had been duplicated from a third publication, journal G. Author D is a member of the editorial board of journal A. The letter sent by author D was co-signed by all authors except the author accused of fraud. The accused author was the corresponding author on one of the papers.

The editors in chief of both journals also received a letter to the editor from an independent observer of the case, criticising the editorial process that led to the papers being accepted and published.

The society and editors in chief naturally wished to respond rapidly to author D and to avoid any damage to the reputations of the journals through association. Following standard society policy and procedure, and consulting the COPE guidelines, the editors in chief referred the case to the society's independent publication ethics committee.

The editors in chief also instigated a review of the editorial processes for the two papers to be conducted by (1) a former editor of both journals who was familiar with the subject area covered by the papers and (2) the chair of the society's publications committee.

The publications ethics committee investigated and confirmed the figure duplication and manipulation described by author D and recommended:

- (1) that author D should be asked to retract the papers
- (2) that all authors should be banned from submitting to either journal for 1 year, as is standard practice, on the grounds that all authors take joint responsibility for the content of submitted papers
- (3) the third journal, journal G, should be informed of the actions being taken
- (4) that author D be asked to resign as an editor of journal A to avoid bringing the journal into disrepute by association.

The outcome of the internal review of the editorial process leading to the publication of the papers is incomplete at the time of submission. Findings will be fed back to the editorial boards at meetings in July for discussion and action.

The society and editors in chief seek a second opinion on the recommendations of the publications ethics committee and advice on improving the editorial process to avoid similar cases in future. We would also like advice on:

- (a) what opportunity to reply must be afforded to the author, who is effectively accused prior to a public notice by the journals;
- (b) in light of the COPE guidelines and the COPE definition of 'author', should the nature of response depend on whether the corresponding or another author notifies the publisher/journal of a potential problem.

11-14 Is this previous publication? (CG)

A manuscript was submitted to our journal and, on running a routine CrossCheck report, we found that it contained a 68% match (over 5000 words) to a report from a funded project by the authors that had recently been published online. Because the similarity match was so high, we rejected the manuscript.

The author is now contesting the rejection, arguing that the funded project had demanded the full copy of the submitted manuscript as part of a mandatory annual report to maintain the funding, and published the full report on their website without his approval.

Are we wrong to consider this previous publication?

11-15 Disagreement between authors and sponsor (GW)

Our journal was contacted by a representative of a company following acceptance of a manuscript that was based on a clinical study sponsored by that company. Upon acceptance, the senior author had forwarded a copy of the manuscript to the company, who had identified some discrepancies between the data presented in the article and an initial report that had been presented to them while the study was still underway. They stated that these discrepancies had been brought to the authors' attention but that a satisfactory resolution had not yet been reached and that they had requested a full independent audit.

The authors, who were copied on this correspondence, were asked by the journal to clarify the discrepancies. In a letter to the company and the journal, the authors responded to each of the concerns that had been raised. They stated that the discrepancies resulted to some degree from the use of a different analytic technique in the submitted article, citing the CONSORT statement as evidence that this technique has become the favoured form of analysis since the initial report, which was presented almost 15 years ago. They also acknowledged that one of the numbers presented in the initial report was inaccurate and that they had redone their analysis to ensure accuracy in their results. A revised manuscript was included with this letter.

The company responded that they were not satisfied with the authors' responses and repeated their request for an independent audit. The authors responded that they did not have the resources for an independent audit but would conduct an extensive internal one. The associate editor who had accepted the manuscript and a representative of the editorial board were asked to assess the company's claims and the authors' responses and they felt that the authors had satisfactorily answered the questions that had been raised.

The company contacted the ethics committee of the authors' institution and reported their concerns. The institution's CEO reported that the ethics committee, as well as the institution's board, had investigated the situation and found that while errors had occurred, they did not constitute research misconduct and that they did not see this as an ethics issue. They said that the decision regarding publication should be left to the discretion of the journal's editors.

Our publisher's legal team was consulted, and they felt that if the journal editors were satisfied with the authors' responses and the relevant changes to the manuscript, then they did not see any reason not to publish the paper. They also advised that our society's board of directors should review the situation and recommended that the case be submitted to COPE, a recommendation endorsed by the society's board.

The company continues to maintain that an independent audit is needed. The authors contend that the company is seeking to delay or stop publication of the article, perhaps because of its implications for a line of products produced by the company.

11-16 Seeking retrospective ethics approval (NB)

I received a submission that had asked a series of questions of visitors to a website about a mental health issue. It was reviewed by a senior colleague and myself. While the science was fine we were both concerned that no mention had been made of any ethics approval. I raised this issue with the authors, especially given that deception was involved.

The authors then appear to have sought ethics approval from their institutional committee. The committee appears to have granted approval with the proviso that a notice be placed on the website indicating that the material from the questionnaires was to be employed in research, not in supporting the activities promoted by the website, as first indicated. The notice was to remain up for 3 weeks so that any of the participants could ask for their responses to be deleted from the study. No one asked that this be done.

We have discussed this issue 'in-house'. It has been suggested that publication is not necessarily precluded given that the institutional review board has approved the study and required the authors to notify participants of their intentions. Of course, the institutional review board and the authors would need to be able to produce full documentation relating to the submission and approval of the project.

We would be grateful for COPE's advice on this issue.

UPDATES (editors would like further advice)

11-05 Data fabrication, lack of ethical approval, withdrawal of paper and publication in another journal (MT)

Anonymised text of the case:

This query refers to a clinical trial comparing two forms of treatment which has since been published in another journal.

I originally received this manuscript in 2009. One of the referees alerted me to the fact that the data looked strange. Furthermore, the test and controls groups were perfectly distributed, which is almost impossible. Along with the usual requests for modifications, I asked for the ethics approval letter to establish if the trial had been registered or approved somewhere and also expressed the concerns to the authors.

In the revised submission, I received a letter signed by one of the authors stating that the study was discussed at a departmental meeting and it was established that there was no need for ethical approval. According to my understanding of the matter, running a clinical trial in my country without ethics approval is a criminal offense. The authors also acknowledged that severe mistakes had been made to some of the measurements but did not admit to any wrongdoing.

I asked the authors for a file with some of the images from their study and the clinical data for each individual case. Authors who submit to our journal agree to make the original data available upon request during either peer review or later. The paper was withdrawn by the authors.

In the paper recently published in the other journal, the senior author as well as an assistant professor from the same institution listed as authors in the original submission that I handled no longer appear on the list of authors. Two additional authors are now listed. I originally did not pursue the matter when the paper was withdrawn from consideration in my journal as I understood that the authors had realised that there was something wrong. On publication in a different journal, however, I was alerted by the original referee that the paper that was withdrawn was now published.

I contacted the editor of the other journal who did not seem as concerned as we are with such possible misconduct. Essentially the complexity lies in the triangle between two journals and two institutions as well as in the possibility that two aspects may be wrong: the ethics and the data. The change in authorship for the same manuscript in the two submissions is also significant. Even though the paper was withdrawn from my journal, should I pursue the matter with the relevant officers of the two institutions involved?

Advice:

The Forum agreed that the editor does have a duty to follow-up the case, even though the paper was withdrawn from his journal.

The editor should contact the ethics committee and/or the institution, and copy in the authors so that they are aware of what the editor is doing. The editor should send the ethics

committee a copy of the paper and ask them if they approved the study. If they say they did not approve the study, then the editor should request that they investigate the matter. The editor should also raise the question of the change in authorship with the institution.

The editor should try and involve the other editor if possible.

Another suggestion was to write a letter to the other journal, as an author, raising the issues of the unusually smooth data and also comment on the apparent lack of ethics committee approval.

Follow-up:

Acting on the advice from COPE, I contacted the editor of the journal that published the article. The answer was as follows: “I welcome any opportunity to collaborate to reduce scientific misconduct. With that said, the practical actions become more challenging. Perhaps if we can discuss this in person, we can define some practical steps to get started. We investigated the human subjects ethical review for the paper you identified, and the responses were acceptable. Although I have worked closely with ethical review boards in my country, you are certainly more knowledgeable than I about the details in your country. The practical details always make me nervous.”

Apparently, the journal found the ethics review acceptable and in the response it is apparent that no further follow-up is planned.

At this stage, contacting the ethics committees of the two institutions involved will be my next step. This is very delicate as this has already been done by the other journal (I am not sure if the ethics committee or the authors were contacted).

How should I proceed? Should the editor of the other journal be copied in on the correspondence? To whom should my letter be addressed—all authors from both versions of the papers or only those of the version that was originally submitted to my journal? And in terms of the ethics committee for the two institutions, should I cite in my letter that since the case falls outside the normal best practice guidelines, I have sought input from COPE? Should I say that I contacted the other editor but failed to obtain a satisfactory response?

10-13 Suspect author (SY)

Anonymised text of the case:

Author A has published approximately 150 original articles since ~1994, with ~100 on one particular topic. Since some of these events were up to 16 years ago, and there are no formal records from then relating to these studies, the only information we have is the memory of the editors of the affected journals in post at the time. According to their accounts, suspicions were aroused over the validity of the data, in particular the similarity between baseline data of some of the different studies. When author A was pressed to provide raw data, he stopped responding and stopped submitting papers to the specialty journals, switching to general journals where he continues to publish. The editor of one specialty journal raised concerns with the author’s institution (in another country) approximately 12 years ago; it responded saying it saw no reason to investigate further. A letter, published in one of the specialty journals in 2000 by an independent researcher, asked the question “Why are author A et al’s data so nice?”, pointing out that the probability of such results occurring by chance were

infinitesimally small, but as far as we know there have been no formal investigations of author A's work

Following an April 2010 editorial in one of the specialty journals about research fraud in general, that mentioned this particular author by name, a correspondent raised the lack of investigation into author A, stating that his update of a systematic review was being hampered by the (suspect) influence of author A's work in this area. The current editor-in-chief of that journal contacted the current editors of seven other affected specialty journals, who until this point were largely unaware of the problem, or its extent, having not been in post at the time the papers were submitted to their journals. We have since been discussing the problem and possible courses of action. The points raised are:

(1) Regarding the older papers:

(i) the journals themselves do not have the ability to mount an investigation;

(ii) it is unlikely that an investigator, bona fide or not, will still have original data from the older studies;

(iii) it is unlikely that author A's institution will be interested in investigating studies so old, and we think he might have moved universities since then;

(iv) currently we do not have any firm data of wrongdoing, just suspicions. Options for gathering more data include asking the original correspondent and the systematic reviewer to provide a more formal commentary, although we have not done that yet. Meanwhile, one of the editors has gathered data on all author A's studies: there are 135 in which author A is the first author, reporting almost 12,000 randomised patients in 17 years. Most are with one of the same three co-authors. The largest group of papers (by topic) are all very similar in design, with very little variability in baseline placebo event rates, and generally similar results although the outcome measures differ and there are one or two 'surprising' (at best) findings. One particular drug features in 71 studies. Dropouts are hardly ever reported.

(2) Regarding the newer papers:

(i) these may be easier to investigate since the data should still exist;

(ii) we could contact the editors of the non-specialty journals (there are many, publishing just 1-2 of A's articles each) to alert them but the problem of having only suspicions remains (compounded by the relatively large number of journals, each with a small number of papers);

(iii) we could ask a respected academic in author A's country to make discreet enquiries of author A's co-investigators, some of whom may not realise what is going on, or they may have concerns themselves. However, this could be a delicate situation for such a person. We would welcome COPE's advice on how best to proceed.

Advice:

The Forum was unanimous in their opinion that this should really be resolved by the researcher's institution. Although there is no hard evidence, it was suggested that all of the journals, as a team, approach the institution and ask the institution to conduct an investigation. It was felt that this would provide a more powerful case than a single editor on his own. Meanwhile, the editor should try and gather more evidence, perhaps by contacting the ethics committees who supposedly approved these studies. The editor may then be able to

determine whether in fact the studies took place as reported. The Forum advised against informing the non-specialty journals at this point as there is no real evidence at this point, so it would be difficult for them to know what to do. The Forum also suggested that the editor should advise anybody doing a meta-analysis on this subject to include a sensitivity analysis to test the effects of including the data from these studies.

Follow up:

The group of editors-in-chief are planning on sending a letter to the author and the institutions. The delay has been in obtaining an independent analysis of the suspect works, which so far indicates that the likelihood of fabrication is very high.

Follow up (May 2011):

The editors are still planning on sending a letter to the author and the institutions but this has been delayed pending the analysis, which has now been submitted to journal A for publication! (It concludes that fabrication is almost certain.) Meanwhile, a separate publication scandal has distracted the editors' attention recently.

We did not investigate ethics approval since we felt that should be for the institution to investigate. My main question is what to do with the analysis that has been submitted:

- (i) process as usual and publish if accepted (after discussion with/approval by the publishers); that is, let the author/institution respond if they wish
- (ii) present it to the suspect author (with the analyst's permission) and remove from the publication process for the time being
- (iii) present it to the suspect author's institution(s) as per (ii)
- (iv) present it to the suspect author and institution(s) as per (ii)

I have sought an independent statistical review of the analysis from a respected statistician but have not had a response.

UPDATES

10-15 Inadequate reporting of a trial, despite earlier rejection from a different journal

Anonymised text of the case:

We have been contacted by a reviewer after he spotted a paper he had reviewed for us (journal 1) now published in a second journal (journal 2). Both journals are members of COPE. The reviewer had advised we reject the paper when it was sent to him to review in September 2008. This was based on his assessment of the paper and also the supplementary material he was sent by us: protocol, CONSORT statement and trial registration details. Seeing the paper now published in journal 2 (April 2010), he still has the same concerns about the conduct of the trial and validity of the data presented—for example, the study was not really randomised (subjects were allocated alternately to treatment or control and participants' trial numbers were known to the outcome assessors)—and reporting of the outcomes was inconsistent with scores given in the raw data.

Following the original rejection from journal 1, the authors appealed against our rejection and we declined their appeal. The letters from us made it clear that we had concerns about the methods and reporting, and both external reviewers' reports were available for the authors to see.

So in spite of making it clear that there were problems with their paper, the authors still went ahead and submitted to journal 2.

When contacting us, the reviewer wanted to know if he could contact the editor of journal 2, make it known that he had seen supplementary material when he had reviewed the paper and explain his concerns to the editor. We advised him to write to the editor of journal 2 in confidence, explain what had happened and say that we had reservations about breaching confidentiality in this way but thought that the benefits of doing so outweigh the risks.

The editor of journal 2 has now contacted us asking to see the supplementary material. Unfortunately, we no longer have the files available (we do not archive them that long) but we do have the reviews.

Questions for COPE

- Should we share with journal 2 the reviews we still have?
- In principle, could we have shared the supplementary files and original submission (although they are no longer available)?

Advice:

The Forum's advice was that the journal should not share reviewer comments with another journal without first asking the reviewer's permission. However, it is quite acceptable for the reviewer him/herself to decide to send their comments to another journal if they think this is

the right thing to do (since the review belongs to the reviewer). Most people felt that the material submitted by the author to the journal (ie, the protocol) should not be sent to another journal, as this is privileged/confidential information. It is up to the second journal to contact the author and ask for this material. If the author does not respond, then the journal that had actually published his work (journal 2) should contact the author's institution.

Follow up:

The editor of journal 2 contacted the author of the paper who provided some, but not all, of the answers which he sought. So, after discussions with the reviewer and a member of the editorial board, they decided not to dig deeper with the author but instead publish the letters that had been sent to the journal about the paper.

The editor of journal 2 published several letters, including a letter from the editorialist who had provided a commentary about the paper.

11-02 Author of rejected letter blames global bias against his message and undisclosed conflicts of interest

Anonymised text of the case:

The editor in chief received a letter to the editor criticising a paper published earlier in the journal. The editor first told the author of the letter that he would publish the commentary after he had given the authors of the criticised paper a chance to respond. When asked by the author of the letter, he later added that he would also publish the letter if the authors failed to respond.

The corresponding author of the paper explained that the same person had attacked every single article from his group for about 5 years, that they had responded to the critics adequately in the past and declined doing so again in this instance. He portrayed the letter as a disingenuous and fraudulent commentary and asked that we do not publish the letter, which in his view would be a disservice to science.

He attached to his email a correspondence with the editor of another journal, detailing a similar case for one of his previous articles. He also copied in four editors of other journals who received similar commentaries from the same person, as well as one opinion leader in the field. The four editors had rejected the letters and two had banned the author of the letters from publishing in their journal. One of these editors replied to confirm the story, stating that in his view, the letter he had received was not founded on fact, and backed the request not to publish this new commentary.

At this stage, the editor of our journal contacted me as managing editor to ask if he could reject the letter outright. I advised that he could, but only based on the scientific merit of the letter, and not on the history of its author. He sent the letter for review to the associate editor who handled the criticised paper, and who briefly concluded that the commentary was probably not worth publishing. The editor then rejected the letter, initially mentioning various peer reviewers, when in fact there was only one short set of comments.

The author of the letter was quite irate as, based on the editor's initial replies, he expected his letter to be published in any case (with or without a response from the authors) and found the peer review process and decision letter that he received unsatisfactory. A first clarification of our position resulted in a long threatening point-by-point response, to which the editor

responded by reasserting his position and clarifying again why the letter had been reviewed by a referee and himself and subsequently rejected.

The author of the letter then changed tactics and sent the publisher a rather libellous letter in which he argued that the editor is incompetent, was not impartial and was influenced by the author of the article; that the author of the paper and the peer reviewer (whose identity he does not know) have undisclosed conflicts of interest; and that opposite interests and prevalent opinions, relayed by a mainstream advocacy group and the WHO, colluded to silence him, a whistleblower. One of the assumed conflicts of interest mentioned involve the head of an institution from which the authors received a grant and is therefore very indirect. Another, however, relates to a global advocacy group, endorsed by the WHO and which seems to reflect the predominant opinion in the field, and which the authors presumably belong to. The corresponding author of the paper calls this group a forum for exchange on the topic, but it has a clear health policy agenda. The authors did not mention this in their declaration of interest.

Our publisher informed him that she was taking his concerns seriously and would ask me to investigate in accordance with COPE's guidelines, particularly in relation to our disclosure policy. Although the author of the letter accused us of being in breach of the COPE policy on fair peer review in an earlier correspondence, he then replied that in this case COPE's 'concepts' are meaningless and, for example, anonymous peer review or impartiality are impossible, since he is the only researcher "who has exposed a long series of frauds" on the topic. He went on to reiterate his request that we (a) do not contact the authors of the paper he criticised (he was initially fine with the authors responding to his critique), (b) ask referees specifically if they belong to the advocacy group discussed above and (c) send, if needed, his blinded manuscript to 'independent' (from the advocacy group mentioned and industries) reviewers, of which he proposes three names.

We have not replied to this last email and decided to seek COPE's advice on how to best close the case and what we should have done better earlier. In particular, I'd be interested in your opinion on:

- how the editor should have communicated with the author of the letter in the first place?
- What are the main weaknesses in our handling of the case?
- if the authors of the criticised paper belong to an advocacy group, should they disclose this in their declaration of interest section?
- how to respond to the author of the letter about allegations of unfair peer review, and his rejection of a blinded peer review system?
- should we, and how could we, check whether the author of the letter is genuinely trying to make valid points against prevalent opinion?
- on the other hand, should he have an undisclosed agenda, should we take any action against him?

His letters and emails are often unconvincing, based on assumptions, defamatory and threatening, and he seems to belong to an opposite advocacy groups without disclosing it. In addition, the bulk of his contributions to the literature is made of commentaries criticising articles but he does not appear to publish any original research himself.

Advice:

The COPE Code of Conduct states that editors must be willing to consider cogent criticisms of work published in their journal. However, the editor needs to decide if this is "cogent

criticism” or not. The Forum suggested there were three issues here: (1) whether the editor should publish the letter; (2) the undisclosed conflict of interest of the authors; and (3) the defamation of the editor on websites.

Regarding whether the editor should publish the letter, the Forum noted that editors have a duty to publish letters unless they are factually incorrect or libellous, and it is the responsibility of the editor to make that decision. Some of the members of the Forum argued that an editor should veer towards publishing everything, while others noted that there are different types of letters in different journals and it is not always appropriate to publish. However, all agreed that the editor should not make false claims (saying that the letter would be published with or without a response from the authors and then going back on that decision) and that the journal needs to tighten its editorial processes. The journal should have a clear process for handling letters. Are they peer-reviewed? The decision to publish a letter should be based solely on its academic merit.

Regarding the undisclosed conflict of interest of the authors, the editor should contact the authors directly and ask them to respond to this accusation and emphasise that non-financial conflicts of interest are also very important. If it transpires that the authors do have a conflict of interest, the editor could publish a correction. The editor should also check the journal’s policy on this issue.

On the third point, if an editor is defamed on a blog or website, what can he do? Some suggested a dignified silence as the best option as otherwise it can fuel the problem and encourage more debate. However, all agreed that if the accusations are potentially libellous then the editor should seek legal advice.

Follow-up:

I contacted Dr C, the author of the letter, to let him know that his case had been discussed at the COPE Forum and to reiterate our decision not to publish his letter. Most members present, if not all, supported the view that the editor in chief can reject a letter or commentary at his discretion. I sent a brief overview of the discussion and explained that the COPE Forum was of the opinion that declarations of interest should relate only to direct potential conflicts of interest. That meant that the DOI in the criticised paper was satisfactory and appropriate. The remaining question concerned the author’s role within an ‘advocacy group’.

I said we would contact the author and ask him to clarify the purpose of the group, their involvement in it and whether they think it might be a conflict of interest. We would then assess their response against our current requirements regarding declarations of interest.

Dr C quickly sent a couple of inflammatory emails in his usual style, where he repeated that there is a “universal conflict of interest that must be disclosed” because 90% of authors, editors and reviewers belong to the same bias ‘advocacy group.’ He then went on to contradict himself and, in my view, invalidate his only claim which may have had some merit:

“I would like to clarify that [the group] is absolutely not an ‘advocacy/regulatory group’. To take a modern example, it is something like Facebook where authors [...] advertise and publish the kind of publication that I have criticised in details for their flaws. In that case I don’t really see what the problem is.”

I intended to get the author’s opinion on this but have not pursued the matter.

I've not heard anything more from Dr C since and consider the case closed

11-03 Plagiarism of reviewer's work

Anonymised text of the case:

Several Europe-based authors, including well known, respected and much published ones, submitted an essay for the journal's section on research methodology.

We rejected it without external review as it wasn't making sufficiently new points. We offered to see it again, however, if it was revised and if it added some worked examples using this methodology within published studies. A year later the article was resubmitted after revision, and we sent it for review.

One of the reviewers, who was chosen by the handling editor via a PubMed search, replied quickly, saying that:

- 1) she used to work in the senior authors' department where her PhD was supervised by the most senior author;
- 2) the worked examples in the revised submitted article, and some of the accompanying explanatory text, were copied and pasted (with translation into English but no attribution) from her unpublished PhD thesis, which was on this same topic;
- 3) she had contacted the most senior authors about this, and they had apologised saying they hadn't realised that the first and most junior author, who had revised the submitted article, had lifted the cases from the reviewer's PhD thesis. They said that the first author was contrite, and they advised the reviewer to tell the journal what had happened and seek our advice;
- 4) this reviewer also supplied a helpful, constructive and positive review of the article (as, meanwhile, did another reviewer and subsequently did a third, as we felt unable to use the first reviewer's review).

We replied saying we took plagiarism very seriously and that we appreciated the reviewer's constructive approach to all of this. We suggested that she should be added as an author of the revised article, and she agreed—in a phone call with the editor handling this article—and seemed happy with that solution. We asked all of the authors to write to us to confirm: (a) that plagiarism had occurred and (b) that the reviewer was now a co-author of the article. All did so.

The revised article, with the new co-author, was provisionally accepted after appraisal at the manuscript committee meeting (where all members had read the article's full history) and is now back with all of the authors for final revision. We expect to accept the article and we intend to reiterate in the acceptance letter that plagiarism is a serious form of misconduct, that we appreciate everyone's cooperation here and that we hope the institution now has clear rules on avoiding misconduct.

Did we do the right thing, and is there more we should also do?

Advice:

Most of the Forum agreed that this was a good result; all parties were happy in the end and the plagiarism was detected. The majority view was that it seemed to be an honest mistake which was dealt with well. However, some of the Forum put forward the idea that this was an act of theft and the senior author should have known better. But the majority view was that the editor should not pursue the matter and contacting the institution would be an

overreaction. A constructive suggestion was to ask the authors what steps they are going to take to prevent this from happening in the future.

Follow-up:

We asked the authors how they would prevent this happening in future. They said: “In response to the editors’ comment on the issue of plagiarism, we like to start here by emphasizing that we as well have taken this very seriously. Plagiarism of any kind is not and will not be accepted at our institutions. We have codes of conduct that explicitly stipulate that this is not accepted. In this case, we do not believe that there was an issue of voluntary, intended and therefore wrongful appropriation of close imitation of previously written scientific papers.

The current exchange with the editor and the journal, however, has clearly taught us that we should be even more careful when the work of PhD students builds on that of others, and that we should carefully examine the results and writing of subsequent researchers in our groups before we submit anything to a scientific journal. Previous work should always be acknowledged and when there is a contribution that justifies authorships according to the current standards, we should invite that colleague to become a co-author on new papers. Similar issues have never happened to us before, despite the fact that we have supervised several dozens of theses and hundreds of papers. Nevertheless, we have now taken action to institute this as a new standard policy in the manuscript preparation process.”