



Forum agenda

**Meeting to be held on Monday 18 June 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. New cases

12-08 Alleged misuse of confidential information (CP)

12-09 Authorship dispute (PL)

12-10 More than a breach of confidentiality? (DP)

12-11 Meta-analysis: submission of unreliable findings (AT)

12-12 Compromised peer review system in published papers (JR)

3. Update: Editor wants more advice

11-24 Inappropriate authorship (FJ)

4. Updates

11-23 Possible overlapping publications/data

11-25 Lack of ethical approval and not reporting experimental evidence

11-29 Reviewer asks trainee to review manuscript

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

12-03 Submissions from institutions where misconduct has previously been suspected

12-06 Review of a book written by an editor of a journal

12-07 Was this study unethical?

CONTENTS

NEW CASES	3
12-08 Alleged misuse of confidential information (CP)	3
12-09 Authorship dispute (PL)	5
12-10 More than a breach of confidentiality? (DP)	6
12-11 Meta-analysis: submission of unreliable findings (AT)	7
12-12 Compromised peer review system in published papers (JR).....	9
UPDATE: EDITOR WANTS MORE ADVICE	10
11-24 Inappropriate authorship on students paper (FJ).....	10
UPDATES	12
11-23 Possible overlapping publications/data.....	12
11-25 Lack of ethical approval and not reporting experimental evidence.....	13
11-29 Reviewer asks trainee to review manuscript.....	15
12-02 How to correct an incorrect decision to publish a flawed paper	16
12-03 Submissions from institutions where misconduct has previously been suspected	18
12-06 Review of a book written by an editor of a journal.....	19
12-07 Was this study unethical?	20

NEW CASES

12-08 Alleged misuse of confidential information (CP)

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.

12-09 Authorship dispute (PL)

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.

12-10 More than a breach of confidentiality? (DP)

A journal received two manuscripts on the same topic in short succession.

Manuscript A was rejected after peer review; manuscript B, submitted a few months later, was accepted after peer review. When manuscript B was published, author X contacted the journal to express concern about similarities between both papers and the fact that the first had been rejected and the second accepted. The journal investigated and found that one of the reviewers of manuscript A worked in the same laboratory with one of the authors of manuscript B.

The journal was further notified that manuscript A, while under peer review, had been discussed in a journal club at reviewer P's and author Y's laboratory. Both author Y and reviewer P, when confronted with these allegations, admitted the incident and apologised but said it had been a lapse with no ill intent.

The journal's editor-in-chief informed author Y and reviewer P that their behaviour was considered a serious breach of confidence and that they would be removed from the journal's list of peer reviewers. The editor-in-chief also considered informing the institution but decided against it at this point in time.

The journal also decided to review its process for monitoring peer reviewers' competing interests and to educate their editorial board about the need for confidentiality.

Author X has recently contacted the journal again to complain that author Y was promoting the paper at public meetings and to ask whether a full investigation of plagiarism had been undertaken.

Should the journal have informed the institution and asked for further investigation?

12-11 Meta-analysis: submission of unreliable findings (AT)

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.

12-12 Compromised peer review system in published papers (JR)

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.

UPDATE: EDITOR WANTS MORE ADVICE

11-24 Inappropriate authorship on students paper (FJ)

Anonymised text of the case:

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

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

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

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

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

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

Advice:

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

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

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

Update:

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

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

“Contribution:

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

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

Can COPE suggest what I should do now?

Advice on follow up:

The editor informed the Forum that the paper has now been withdrawn. The Forum advised that although the editor has to accept that the paper has been withdrawn, she should still pursue the institution for a response. The supervisor has acted inappropriately and the institution should be made aware of this. One suggestion was to send a registered letter (rather than an email) so that the editor is sure the letter has been received. Another suggestion was to contact any professional bodies that the author might be a member of, informing them of the inappropriate behaviour.

The editor could also write an editorial on authorship policies, referring to the International Committee of Medical Journal Editors (ICJME) guidelines. For the future, the editor might consider altering the journal’s instructions to authors stating that the names and contact details of all authors (not just the corresponding author) should be provided to the journal on submission of a paper. The editor could also consider publishing the list of contributors, detailing what contribution each author made to the paper.

Update (June 2012):

I first wrote to the corresponding author asking for the policy of the institution regarding authorship criteria. Simultaneously I wrote to the head of department of the research section and the Dean asking for the same. I received no reply. Recently the corresponding author, who is the supervisor and the first author, sent a letter stating that they wanted to withdraw both articles as they have decided to have the article published in a European journal. I have not replied to that request as I feel cornered.

Two more articles from another medical college of the same university are of a similar nature. I have followed the same procedure but again with no response.

Does the Forum think that I should write to the higher authorities, such as the Higher Education Commission and Medical Council, about the largest medical university in the country having no specific policy on authorship and the credit of the research being usurped by the faculty members, thus depriving the young students?

I am collecting all of these cases and will make a presentation at the next conference on publication ethics.

UPDATES

11-23 Possible overlapping publications/data

Anonymised text of the case:

As editor-in-chief of a journal (journal A), I was contacted by an individual (N) who indicated the following: authors of an article published in journal A were questioned as to the similarity of a figure and a table appearing in both journal A and in another journal (journal B). N noted that reanalysis of the data of the published work by the authors suggested errors and inconsistencies of the similar data across journal A and journal B.

Subsequently, N provided additional details, including notice of a third journal (journal C) that appeared to have published a table similar to those in journals A and B. The editor of journal B responded to this saying that although the authors had been contacted by the editor of journal B and a response from the authors was pending, they agreed that retraction from journals A, B and C was required. Of note, journal B had previously resolved a challenge from N with respect to the study in question as a letter to the editor regarding data interpretation with a response from the authors. The editor of journal B shared both of these publications with the other journals involved at the request of journal A.

Although the authors referenced journal B in the article they published in journal A and stated that it was an extension of the study published in journal B, they only indirectly referenced the figure and table. The figure and table did not include a reference or acknowledgment to indicate where they were initially accepted/published or submitted elsewhere. Hence it appears that a very similar figure and table appeared in three publications and a figure in two publications without appropriate assigning credit.

This appears to journal A to be a possible case of overlapping publications by the authors. Taking the first publication dates (including Epub dates) on PubMed, it appears all three articles were published at around the same time, with the article in journal C publishing first as an Epub article.

It should be noted that the authors retained the copyright to their article published in journal B. I am not sure about journal C.

Most recently, N sent another email to all three journals questioning the housing conditions of the animals used in the study and whether the statement indicating that the authors had received approval from their ethics committee of experimentation on animals is actually true.

As editor of journal A, I forwarded all the information to my publisher. I plan to contact the authors on review and after discussion with COPE. My publisher has also informed journals B and C of our plans to contact COPE before taking any initial action.

As well as asking for guidance on how best to handle this case, we would like COPE's opinion on which journal should be taking the lead to resolve these concerns, as it involves multiple journals.

Advice:

The advice from the Forum was to follow the COPE flowchart on redundant publication. Initially the editor should contact the authors and ask for a full explanation. If the editor feels that the explanation from the author is not satisfactory, he can then retract the paper, if his is the second journal which published the paper. If a paper is published online it should be considered as being published, so the epublication date is the date to go by. The first version of the paper should remain and the others should be retracted.

However, if the editor feels that there is only partial overlap and readers would benefit from the availability of new data, then the editor could issue a notice of redundant publication.

Another scenario would be if the editor accepts the explanation of the authors that they made a genuine mistake. In this instance, the editor could issue a correction (for example, figure 7 has appeared in a previous publication).

The Forum stressed it is important to contact all of the authors, not just the corresponding author. The Forum also agreed that regardless of whether or not N is acting in good faith, the editor should investigate the accusations by contacting the authors and asking for an explanation.

The Forum advised liaising with the other editors if possible, and jointly contacting the institution.

The Forum also suggested looking at the copyright issues (e.g. the date on which copyright was transferred to a journal, if applicable).

Update:

The editor retracted the paper. To the best of his knowledge the other papers in question have also been retracted.

11-25 Lack of ethical approval and not reporting experimental evidence

Anonymised text of the case:

In May 2011 a letter from the Vice-Rector for Personnel of a reputable university was sent to the editor mentioning that two articles published in the journal contained two statements not supported by documented evidence. The two statements related to: (1) approval of the local ethics committee and (2) representation of the experimental evidence.

With regard to point (1), the authors stated in the article that they had approval in 1995 for their research protocols but the authorities state that there is no written documentation of this agreement and that this cannot substitute for formal approval of the research. The journal and the university rules indicate that formal approval of an ethics committee is required.

With regard to point (2), a statement in both articles cannot be sustained for one of three patients in one of the articles and for one case in the second article. Laboratory analysis revealed contradictory evidence from the authors' statements in the articles. The authors gave three reasons why they ignored this information. Unfortunately, the samples kept in the authors' laboratory were destroyed in a fire.

According to the letter from the academic authorities, "the authors have been kept informed of these facts, which are in breach of the rules of good scientific conduct".

One of the articles is co-authored by three colleagues from another university. They have asked the journal that their names be removed.

The author of the articles, who received a copy of the letter from the Vice-Rector, asked to have some time to send in his rebuttal of the accusations. For both issues the answers provided by the authors were submitted to the university and were judged as unsatisfactory.

Long discussions within the publications committee of the journal with representatives of the publisher and the scientific society led to the decision that an "expression of concern" should be published. Prior to publication, the expression of concern was sent to the authors and the university for their comments. Just before the deadline, a letter arrived from the university (signed by the Vice-Rector and the Rector). The conclusion of the letter was that the university believed an expression of concern was not needed. The university believed that the authors recognized that they made mistakes in relation to both issues but since they acted "in good faith" the university had closed the case and did not consider an expression of concern appropriate.

So the journal was faced with an author admitting two serious "mistakes" in two articles. The institution that originally raised the concerns backed off in the end. After consulting with the editorial team I wrote to the author asking him to send a letter to the editor signed by all authors correcting the serious mistakes in the literature. A confidential draft letter was received from the author, and edited and completed by the editor so that both issues were mentioned. This letter was signed by all authors at this author's institution. Three authors at a different institution refused to sign the letter as they believed that the letter to the editor did not clarify the situation. These three authors confirm their initial position and encourage the other authors to retract both articles. A copy of the email correspondence between the author and a spokesman for the other institution indicates that the author does not want to do this.

A possible conclusion would be to publish the letter to the editor signed by the authors from the institution of the first author as well as a letter to the editor from the three authors from the other institution. This would be accompanied by an expression of concern or an editorial by the editor, highlighting the necessity of proper ethics approval and reporting all experimental data.

An additional question to COPE: should other editors be informed of this? In a sister journal, an article was submitted mentioning the same very outdated ethics approval.

Advice:

The Forum agreed that this was a very interesting but complicated case. There are two issues here: (1) approval of the local ethics committee and (2) representation of the experimental evidence.

Regarding the first point, the Forum suggested that if the validity of the ethics approval is not in question, then this may not be an issue. The editor does have a right to expect a higher standard from the authors, but they do not seem to have broken the rules as at the time of submission (in 1995), formal approval was not considered mandatory.

In terms of the data, the Forum agreed that the editor needs to decide whether the basic findings of the study are sound. By leaving out some of the data, were the readers misled? If the data that have been omitted are incidental and do not change the findings of the study, then the advice was for the editor to issue a correction. If however, the editor feels that the study is flawed and the findings were presented in a misleading way, then the article should be retracted.

Some felt that as the authors at the second institute want the article retracted, then the editor should consider retracting it (since these authors no longer stand by the findings). As these three authors believe there are grounds for removing their names because they were unaware of the lack of ethics approval and the omitted data, then the editor should consider this option too.

In the end, it is up to the editor to decide. If s/he decides to issue a correction, s/he could detail in the correction notice which authors were aware of (or responsible for) the errors that occurred. But if the editor has doubts about the underlying science, then s/he should retract the paper.

Update:

After the COPE Forum discussion, a decision was taken to correct the literature by publishing two letters to the editor. The first letter from all of the authors recognises the errors made and explains the reason why the omission of the experimental evidence did not put into question the validity of the work. The letter was signed by all of the authors except the three authors from the other institution; they had asked that their names be removed as authors and they explain in a letter to the editor why they requested this. An editorial was written in relation to these two letters, reiterating the facts and insisting that proper ethical approval is required and that all experimental evidence needs to be reported. The conclusion of the editorial is that the journal decided not to retract the paper but to publish a correction. Both letters to the editor and the editorial will be linked to the two articles in the literature.

11-29 Reviewer asks trainee to review manuscript

Anonymised text of the case:

A known expert in a certain content area was asked to review a manuscript. He asked if one of his trainees (not a content expert) could review the manuscript instead, with some oversight and as a training exercise. He stated that he would provide the trainee with a full

explanation of confidentiality. The section editor replied that it was the particular expertise of the invited reviewer that was being sought. The invited reviewer agreed to review the manuscript. Subsequently, the reviewer contacted the section editor, stating that his trainee had reviewed the manuscript and felt the manuscript should be rejected; the reviewer also read it and concurred, suggesting that the editor reject the manuscript as poor science (my words), but did not include a detailed review. The section editor pushed him to return a detailed review, which he has now done.

My question is: is giving this manuscript (not blinded, ie, author names and affiliations are evident) to the trainee a breach of confidentiality on the part of the reviewer? If so, what steps do you recommend taking?

Advice:

The Forum suggested that this type of behaviour probably occurs quite frequently. As the editor did not specifically state that the reviewer should not share the manuscript with the trainee, the Forum believed it was not strictly a breach of confidentiality. If the manuscript is unblinded and the editor is happy for the reviewer to see the author names and affiliations, then it is probably acceptable for the trainee reviewer to see this information, but the journal should be informed. The journal may need to ensure that the trainee does not have a conflict of interest and has the expertise to review the manuscript.

The Forum questioned how trainees can gain experience in reviewing manuscripts. A suggestion was to circulate good reviews to trainee reviewers, but with the consent of the reviewer and probably also the author.

Update:

The editor shared the Forum's advice with the section editors, that they need to be very clear in the invitation to review if they do not wish a trainee to review the manuscript.

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.

Update:

As advised by the Forum, we discussed with the publisher's legal advisers and wrote to the authors proposing a notice of correction. We have just received a reply. We are trying to avoid involving arbitrators. Meanwhile, we have had a new submission from a third party reanalysing the original data.

12-03 Submissions from institutions where misconduct has previously been suspected

Anonymised text of the case:

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

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

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

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

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

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

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

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

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

Advice:

To avoid a similar situation in the future, the Forum advised the editor to tighten the journal's instructions to authors and request the contact details of all authors, not just the corresponding author, on submission of a paper. The Forum agreed that it is difficult to deal with the second paper until the issues with the first paper have been resolved, particularly as there is no evidence of misconduct in relation to the second paper. The advice was to contact the ethics committee or institutional review board in relation to the first paper, or if a response from the institution is not forthcoming, consider contacting any professional bodies the author might be a member of or the funding body. The editor might consider contacting any collaborating institutions listed on the second paper.

Update:

Despite a further letter to the university in question, the editor has not yet received a reply. The editor was given the email address of the head of the academic department from the author of the second submission. No response has been forthcoming. The editor is not aware of other bodies who could be contacted to shed light on the matter. The research project was funded internally.

The journal's Guide for Authors has been updated and authors are made aware that extra information can be requested by the editors. Should the journal receive further submissions from this institution, they will receive thorough peer review and if there is any doubt as to the validity of the data, the journal will seek clarification from the authors.

12-06 Review of a book written by an editor of a journal

Anonymised text of the case:

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

Advice:

The Forum agreed that this was a dilemma—if the book review is not published in the editor's journal then you may be denying readers knowledge of a potentially useful book but if it is published, is editor A abusing their position as editor? How will the journal be perceived if the book review is published? Would readers question why this book was selected for review? And would the reviewer of the book feel under pressure to give a good review? Most of the Forum agreed that it would be best not to publish a review of the co-editor's book. If there are other journals in the field, it would be best to publish it in one of these. However, if the editor does decide to publish the book review, the process must be transparent. The Forum suggested publishing an explicit statement about who wrote it and explaining the review process. The Forum also suggested talking to editor A and explaining that the journal needs a policy about editors publishing in their own journal and, if allowed, the journal needs to develop stringent guidelines on this process, which should be completely transparent.

Update:

The editor decided not to solicit a review of the book authored by the co-editor because of the conflict of interest for the editor as well as potential book reviewers. Moreover, there are at least two other scholarly journals in the field that could review this book.

On the other hand, an edited anthology in which an article written by the co-editor appears seems to be a likely candidate for review since a prospective book reviewer could choose to mention that particular article or not.

The co-editor and the editor agreed that an important step in considering the matter was to ask COPE for their perspective(s). The editor is grateful to the Forum for their discrete and conscientious handling of the matter.

12-07 Was this study unethical?

Anonymised text of the case:

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

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

In discussion, one of the editors argued that the letter missed the important point that after the randomised controlled trial, all participants received the 5 month intervention to promote

smoking cessation among parents and/or to reduce SHS exposure, and questioned the letter's implication that the children would be better off if the study had not been conducted. S/he wrote that the letter seemed to argue against any controlled trial to reduce children's exposure to SHS, but that there were several dozen of these in the published literature.

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

Advice:

The editor told the Forum that questions raised during the review process were answered by the authors. The authors informed the editor that institutional review board (IRB) approval had been obtained but they did not provide evidence of IRB approval. The Forum advised the editor to publish the letter and give the original authors a chance to reply. The editor might also consider publishing the IRB approval statement. Another suggestion was to commission a commentary or write an editorial on this issue.

Update:

As advised by the Forum, we contacted the study authors to clarify the protocol instructions. In addition, we obtained a confidential consultation from an independent ethics body with four experts who summarised relevant ethical concerns. We also solicited a letter from the study authors to respond to the issues raised in the letter to the editor and solicited expert opinion from the department of bioethics of a major institution.

The letter to the editor, response from the authors and the expert commentary will be published in the upcoming issue along with a brief introductory statement from editorial staff. The introduction invites our readers to review the materials in the interest of furthering reflection on the ethical implications of research on the effects of secondhand smoke and the ethics of the research enterprise more generally.