



Forum agenda

**Meeting to be held on Tuesday 2 June 2009 at 3pm
Boardroom, Woburn House, 20 Tavistock Square, London WC1H 9HQ**

Agenda

1. Update on COPE activities by the Chair

2. New cases

09-07 Duplicate publication or salami publication? (AM)

08-03 Randomisation and ethics of pilot trials (DMN)

**09-08 Has formal ethical approval been granted that satisfies publication criteria?
(ZH)**

09-09 Authors bearing gifts ... (SB)

09-10 Concern about reporting of a trial and also its DSMB (VB)

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4. Updates

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08-19 Simultaneous publication (LKS)

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08-28 Advice regarding a weird type of content and its authorship (PST)

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09-04 Possible serial misconduct in relation to coauthors and other activities (GT)

09-06 Duplicate publication? (ECP)

NEW CASES

09-07 Duplicate publication or salami publication? (AM)

An author submitted an article at my journal. The editorial board discovered that the author had already published his article in another journal. The editorial board communicated with the author and he defended himself stating that they were two different articles with different titles. However, the editorial board could find no significant difference between the two papers.

There are two issue related to this article.

(1) The author did not notify us, prior to his submission to another journal.

(2) Almost all of the text is the same, indicating duplicate publication. Can this be taken as salami publication? How should we handle this issue?

08-03 Randomisation and ethics of pilot trials (DMN)

This case was previously discussed as **08-03**. After that discussion, our worries about the paper grew. We could not get direct answers from the author. However, we did find that the data in one of their tables which showed subgroups could not be transformed back to match their table of baseline data, and that the randomisation ratio in the subgroups in that table was highly statistically unlikely to have been obtained by proper randomisation. The authors admit that their computer program had small errors, but the ratios in the table were far too high or far too low in some subgroups. Even though we had accepted the paper, we decided to reverse our decision and we rejected it.

After rejection, we asked different groups in the authors' country to start an investigation; the author is at a private institution. One person did agree but all they did was look at the paper sent here. The short report sent back did not add anything to what we already knew.

The paper, as published in another journal, does not contain the subgroup table we saw. What we do know is the mismatch between the table we saw and the baseline table, and the errors in the randomisation ratio in that table. We also have many questions that remain about this study.

Do we have a duty to take this further? Should we contact the other journal's editor?

ORIGINAL CASE

Background

We received a paper with potentially important results. After review and revision, we accepted the paper. On further reflection, and asking more of the authors, we became concerned. It is an RCT and the only protocol available was slim but appeared authentic. There were two protocols: one for a pilot trial and, if that was positive, a second protocol aimed to randomise more people. One residual concern was that there was an imbalance in the two randomised arms. The authors' statistical advisor has explained that such an imbalance, although large, is not necessarily unexpected when using older versions of random number allocation programs.

We present this case to the COPE Forum for discussion as we had not identified a numerical imbalance that might be unacceptable. We also ask for advice: since the pilot trial showed a significant difference ($p < 0.001$), might it be considered unethical to recruit many more participants before publishing? Furthermore, should the unpublished data from the pilot trial be included in the final analysis?

Discussion and advice

The Forum argued that this was probably more of a methodological problem than an ethical issue. All agreed that the authors have a responsibility to publish the data from the pilot study or, at the very least, the editor should request that the methods and results of the pilot study are included in the final report. The Forum suggested that perhaps the editor should question the value of publishing the study. If he believes it has value then he should publish it. Other suggestions were to consider writing a commentary on the paper raising these issues.

Update (June 2008)

We presented an accepted (but not published) paper where we had concerns about

randomisation imbalance and about a pilot trial that had not been presented. COPE reassured us about imbalance and suggested we ask that the pilot trial data be included. We sent a list of our concerns to the authors. The authors' responses were far from reassuring and they refused to include any information from the pilot trial. We have now rejected the paper and have instituted an investigation of our concerns about the conduct of the trial.

Update (August 2008)

We rejected this paper after the authors refused to include the pilot data in the main paper and refused to give us more information on their mode of randomisation or the way they collected side effects. We then received a letter from a libel lawyer. However, our lawyers rebutted the case. We also contacted a government body overseeing drug licensing and trial conduct in this country as the study was done at a private institute where the corresponding author is the clinical director and his wife is the administrative director. Initially someone from that institute agreed to investigate but then the head of the institute and several others were charged with corruption. We have now contacted a further different overseeing institution but have not yet had any reply.

09-08 Has formal ethics approval been granted that satisfies publication criteria? (ZH)

The issue here is whether formal ethics approval has been granted in order to satisfy publication criteria. By way of some background information, a lot of screening data are collected on many athletes in many sports, both nationally and internationally. Historically, clubs and associations have disclaimers whereby athletes sign consent for their data to be used for audit purposes on the proviso they will not be identified individually. This study appears to do exactly that, except that this is not a retrospective audit, this is an interventional study whereby these players have been subjected to a specific regimen.

In my opinion, there are several issues here.

(A) A blanket proforma that these players were asked to sign does not constitute formal ethics approval for this interventional study.

(B) These players are under age and therefore warrant additional protection.

(C) These players are vying for selection and there is no obvious protection from them being coerced into participation

Whilst new training regimens are being introduced into clubs all the time, if the findings are intended to be disseminated through formal publication, then ethics approval should be sought beforehand. I would be very grateful for the Forum's opinion on this matter.

09-09 Authors bearing gifts ... (SB)

The editor of an international journal is bothered: he has received a gift that looks expensive, though it might not be.

The sender is an author of a paper submitted to the journal; he has just received a “major revisions necessary” decision. In previous emails, the author has suggested hosting the editor in “his native beautiful city”, an invitation the editor has acknowledged, saying he had already visited the city, and it was indeed beautiful! The author identified himself as a student in further emails, thanking the editor in flattering tones for the reviews.

The editor needs of course to acknowledge the gift (no letter was included), or perhaps to return it, but feels very uncomfortable with the situation. The possibilities for this gift range from an innocent gesture of a proud citizen, to one expecting something in return.

The publisher’s code of ethics and business conduct does cover this subject, but its provisions apply to the publisher’s employees, officers and directors, and not to its editors.

The editor therefore asks his publisher, and through him, COPE, for comments, advice and guidance.

09-10 Concern about reporting of a trial and also its DSMB (VB)

We received a paper reporting a trial. There has only been one previous trial of this intervention in this condition that we know of (which was also done by these investigators). There were substantial issues with the reporting of that trial but the end result, as reported by them, favoured the intervention.

The trial we received, presumably approved after that result had come out, had the complication that most patients also received another treatment, and on an intention to treat (ITT) analysis of all patients, those given the intervention did no better and there was increased mortality in the intervention arm. The only positive outcome was from a per-protocol subgroup analysis of patients who did not have the other treatment (which they say is the only group comparable to the previous trial, and hence shows that the first trial was correct).

Although the trial was investigator led, it seemed to us that the authors were trying very hard to make something positive out of this actually rather worrying result. We sent the paper for review, including to a statistician; the reviewers raised a number of issues about the interpretation (eg, the overemphasis on subgroup analysis) and the analysis and reporting.

We felt that this was an important trial that needed to be reported, mainly because of the excess mortality in the intervention arm, but we had the rather odd situation that the authors wanted to emphasise the positive, and the need for further trials of this intervention, whereas the reviewers and editors saw the paper as delivering a negative message and feel actually that the paper will be the death knell for this treatment.

We therefore rejected the paper but offered to see a revised version if it was written more in line with our concerns.

The authors revised and the paper was re-reviewed. The paper was felt to be more balanced, but not yet completely satisfactory (ie, there was still too much emphasis on the positive result in one subgroup and not enough on the mortality).

A further issue then arose in that a reviewer spotted (on re-review) that three of the authors were noted as being on the DSMB for this trial. In their author contributions all are listed as having been involved in “analysing the data” and one, X, as “supervising the statistical analysis”

We asked the authors about these points and they replied:

"(1) We are fully aware that it is unusual for members of the DSMB to be listed as authors, as independence is obviously important for such boards. In our case, the DSMB's independence was not affected for the following reasons:

- (a) Members of the DSMB worked for the entire study period (ie, between 2003 and 2008) completely independent and without any promises or expectation that they would be credited later by a coauthorship.
- (b) My personal decision to include three of the four DSMB members in the list of authors was made a significant time after the final database lock. This decision was long after

completion of the clinical study and its analysis. It credited three members of the board who made some significant advisory contributions to the present manuscript. Only for this reason they were included as coauthors, and it was quite unexpected for them. This decision from December 2008 has in no way influenced their independence and objectivity at the time when the study was running.

(2) As to the contribution of X (one of the members of the DSMB), we have to admit a simple language problem. In our use of the word “supervision”, the word meant that he took a final comprehensive look at our data analysis before the paper was submitted for publication. Importantly, he never supervised (like an academic supervisor) data analysis at any time point before database lock and processing of the data by the clinical research organization. We will change the terminology accordingly.”

We subsequently found that X was also an author on the previous trial.

Finally, the authors did not declare initially any competing interest but after we enquired specifically, they declared that the corresponding author “holds a patent on the use of t[he intervention] for treatment of [the condition]”.

Our concerns overall therefore were that this paper not only reports the outcomes in a way that is not appropriate, but also the composition of the DSMB and the presence of some DSMB members as authors means that the trial may not have had adequate independent oversight.

We felt we had two possible options with regard to publication:

- (1) We reject the paper because it was inappropriately conducted and not appropriately reported.
- (2) We publish the trial after further revision to ensure it is reported appropriately and publish alongside it an editorial that lays out our concerns with the conduct of the trial, but notwithstanding those, our reasons why we think it should be published.

We also discussed whether we needed to raise the issue of the DSMB with the authors' institution.

We discussed the paper with our internal ethics board and they unanimously agreed we should reject the paper (mainly because of the concerns over the DSMB) and inform the authors' institution. We have as yet heard nothing from the institution.

We are bringing this to COPE as this paper raised a number of serious issues we had not come across before. We would appreciate the Forum's opinion on whether we handled this correctly.

09-11 HIV homeopathy (JP)

The authors carried out a study. A homeopathic treatment was given to people with HIV/AIDS. The outcome was quality of life, as measured by a questionnaire after 1 month and 18 months of treatment. Participants were selected for inclusion if they had a HIV seropositive status at the time of study and were not taking any other kind of HIV/AIDS treatment.

The participants were stratified into severely ill (including those who could not walk and were cachectic) and not severely ill. The authors said that they had obtained ethics approval for this study from their own institution in their country of origin and that the Ministry of Health in the country where the trial was conducted had “authorised” the study.

We asked the authors whether the participants had been offered antiretroviral treatment at any time and if not why not? The authors confirmed that the study protocol did not include antiretroviral therapy because “There is no obligation of the ARVs use and conventional medicine is expensive. Many patients...were afraid of white people and of the side effects”.

We asked what the participants were told when they gave consent. The participants were given details of the homeopathic treatment but no mention was made of the existence of antiretroviral treatment. We asked to see copies of the protocol submitted to their ethics committee and the actual approval. The document, which was translated by a member of our staff, did not mention antiretroviral treatment. We asked to see documentation of “authorisation” from the Ministry of Health of the country in which the trial was conducted. There was no written documentation. It was verbal.

We asked why they sought approval from their native country and not the country where the study was carried out. The authors told us that once they had approval from their country, the authorities in the country where the trial was conducted did not require local approval.

We asked how the study was funded and what the authors’ relationship was with the funding body. They provided the names of two funding bodies but no documentation (funding came from funding for a Master’s degree of one of the authors). They did not clarify whether they had any relationship with the funding bodies.

We felt that all participants should have received standard care with antiretroviral therapy, that ethics approval should have been sought from a local ethics committee and that the participants should have been informed about antiretroviral therapy when consent was sought. We were also concerned that this protocol apparently was approved by an ethics committee. We rejected the manuscript and wrote to the authors’ institution expressing our concerns and requesting they investigate further and keep us updated on their progress.

Is there further action we should take?

09-12 The ethics of using privileged information (JP)

A paper published in one of our journals (paper A) provoked the submission of a correspondence article claiming that a minor conclusion of the paper was a misinterpretation and erroneous. The point in contention was a question of zoomorphology and our paper's conclusions were based on analysis using a non-invasive technique while the rebuttal relied on more traditional techniques. We are bringing this case to COPE because although it appears to be in the process of being amicably resolved, with a clear resolution of the scientific issues, it has highlighted an area of confusion about the use of privileged information.

The authors' of the correspondence article (rebutting authors) originally expressed anger and surprise that the paper A contained this error, because they thought they had clearly laid the issue to rest in an earlier rebuttal of a previously published paper making similar errors (paper B). Although this first rebuttal had not yet appeared in print, it had been considered and accepted for publication by the one of the authors of paper A, in his/her capacity as the editor of another journal. Furthermore, this first rebuttal not only challenged the findings of paper B, it also specifically called into question the interpretation of some website data which was included (unmodified) in paper A.

We sent the correspondence article for peer review, and the reviewers supported the soundness of the rebuttal data presented and the alternative morphological interpretation. The reviewers appeared inclined towards the view that the perpetuation of the "wrong" interpretation in paper A was surprising and did not reflect well on the authors of paper A. However, they also indicated that given the close chronology of the various publications, this was a grey area, and not germane to the scientific case for publishing the second rebuttal. We therefore asked the correspondence authors to revise their text to keep the focus on resolving the scientific questions.

Having decided we should, in principle, accept and publish the correspondence article, the authors of paper A were invited to submit a signed response. In this they have clearly acknowledged that the data presented by the authors in both their rebuttals fully support the conclusions reached in these rebuttals and that some of their own data had been misinterpreted in paper A. They also explained that they were already convinced by the first rebuttal which one author had seen in his/her capacity as an editor, and the other had reviewed. However, they had felt it would not be ethical to make use of this privileged information to modify their own paper (paper A) shortly before final acceptance.

We are inclined to accept this as the personal view of the authors of paper A but question whether they adopted the best ethical course.

Questions for COPE

- What is COPE's view?
- How should editors and reviewers proceed when they have access to privileged information which suggests that their own work should be modified or corrected?
- Is there an ethical responsibility to avoid letting known errors into the scientific literature which was transgressed in this case?

09-13 Reviewer misconduct? (MN)

We have received threats of legal action from the authors of a manuscript rejected by our journal, henceforth referred to as journal A. These “aggrieved” authors claim that their manuscript was unfairly reviewed by a close competitor, who then used some of their findings in a paper subsequently published in journal B, without either attribution or citation.

The “accused” scientist had indeed reviewed the paper for journal A, and the date on which he/she had first been sent the paper preceded that of his/her own submission to journal B. The steps of our investigation were as follows:

The aggrieved author was asked to provide additional details on which aspect of his work he/she suspected of being unethically used, and he/she identified a particular paragraph in journal B’s paper, which named two genes which our authors claimed to have identified for the first time in the particular bacterial genus studied in both papers.

Meanwhile, the “accused” scientist was asked to respond to the accusation. His response identified the paragraph in question as being a small area of overlap between the two papers, however, he categorically denied that the content of this paragraph drew in any way on the information presented in the manuscript which he had reviewed for journal A. The accused backed up this denial by sending us a copy of an earlier version of the paper, which had been submitted to and rejected by a previous journal (journal C) months before he had first reviewed the complainant’s paper in journal A.

We confirmed this by contacting the editors of journal C who, after obtaining permission, provided us with an independent copy of the manuscript that had been submitted to his journal. On examination we found that the paragraph in question had remained unchanged, and that the description of the two genes was indeed present before any submission to journal A took place.

We agreed with the accused that this data analysis was a very minor part of the paper published in journal B.

Questions for COPE

- At this point, we feel that our investigation has exonerated the accused reviewer of one allegation (unethically using information obtained during the peer review process in his/her own publication). Does COPE agree?
- If the manuscript submitted to journal C (providing independent confirmation of the accused’s defence) had not been available, how would such a case be investigated?
- The other allegation (of the reviewer causing the authors’ manuscript to be unfairly rejected) remains unresolved. The reviewer denies misconduct, but there is at least the appearance of misconduct on the basis of conflict of interest. However, we do not think that any further investigation can resolve this issue. Does COPE agree?
- The aggrieved authors have asked for a correction to acknowledge their work (which was published in yet another journal one month before journal B published its article). While the reviewer did not “steal” any data or ideas, he may have unfairly “squashed” the authors’

publication. However, the data analysis in question is a very minor point in the article published in journal B, and the authors' work may simply be independent corroboration. At this time we do not feel a correction is warranted because we have no evidence of wrongdoing. Does COPE agree?

- Are there other options that might be used in place of a correction?

09-14 Possible plagiarism (MM)

A review paper (paper 1) was published in journal A. A review paper on the same subject (paper 2) by a different author was published in my journal (journal B) later in the same year. The authors of paper 1 and the editor of journal A informed me that paper 2 had in part been plagiarised from paper 1.

I as editor of journal B looked to the COPE flowchart for guidance and I wrote to the author of paper 2 for an explanation. Although I did not consider the author's explanation satisfactory, I felt uncertain whether this was a case of "minor" or "major" plagiarism based on the fact that the paragraphs copied verbatim from paper 1 constituted only a small fraction of paper 2 and because this was a review paper and not original data.

I then contacted COPE and the chair of COPE gave me some personal advice. I was advised that plagiarism is not a matter of percentages but of principle and to take another look at the flowchart and decide if the author had given a satisfactory explanation for the "overlap". If there was no satisfactory explanation, I should consider a retraction and informing the author's institution.

In considering what to do, I was advised to take account of COPE's guidelines and code of conduct under the headings of encouraging integrity of the academic record and pursuing misconduct..

Based on the advice communicated to me, the journal decided to retract the paper and to inform the dean of the author's institution about this incident. The retraction note has been published online (and will also be printed in the next possible print issue).

Update

The authors' institution has established a high-ranking committee to look into this matter, consisting of the dean, the rector and representatives from the Academy of Sciences.

This case is for information only and not for discussion.

UPDATES

08-15 How many “mistakes” are too many? (ZM)

Background

We published a randomised trial by six authors. Some years later, we received a letter from a researcher who had been looking into the trial in the context of a meta-analysis. She noted “implausibilities of serious concern”, including “a highly unusual balance in the distribution of baseline characteristics”, 95% CIs that were non-symmetrical about the effect estimate, and use of a stratification variable the value of which could not have been known in all patients if the trial was conducted in the way reported.

We asked the corresponding author to write a letter of response, which he eventually supplied a few months later. Owing to the author’s poor English and the level of statistical knowledge needed to assess the response, we sent the exchange of letters to a statistical reviewer. The reviewer said that the letter of concern was “completely correct” in everything it said and that the author’s explanation for the unusual degree of balance in the covariates was “rubbish”, and that the 95% CIs had either been “doctored” or “incompetently estimated”.

In the meantime, an exchange of to-and-fro letters between a different researcher and the same author was published in another journal, relating to a paper reporting on a subset of the same trial data. We were alerted to this by the editor of that journal.

We sent the reviewer’s remarks to the author of our trial, who then consulted two independent statisticians of his own. He soon contacted us to say that, “surprisingly and regretfully”, these statisticians agreed that there were implausibilities and inconsistencies in the data, and asked for more time to investigate more fully. During this time, the author of the letter expressed concern that we had not made the possibility of these problems known to our readership, so we published her letter.

The author has now sent us a more comprehensive response, admitting that the randomisation process was not as described, the 95% CIs were all wrong (he supplied a recalculation), and the trial report had omitted some details of the protocol necessary for understanding it properly (now supplied). Our reviewer suspects that, given his free admission of all this, the author is probably incompetent rather than fraudulent, but that the extent of the incompetence could not give us confidence in any of the data. What now?

Advice

The Forum agreed with the editor’s opinion that the author is probably incompetent rather than fraudulent and should be given the opportunity to redeem himself. It was suggested that perhaps the paper should be submitted for review again. The Forum noted that this was probably a good internal learning exercise in that the statistical errors should have been picked up when the statistical review of the data was performed by the journal. A suggestion was made for the journal to set up a “sin bin”. Some journals operate a “sin bin” or “publication review committee” where once a year papers which readers or others have expressed serious doubts about post-publication are reviewed to determine whether or not it was “a mistake” to publish the paper.

However, some members of the Forum argued for stronger action and suggested contacting the author's institution. But most agreed that the paper should be retracted as the research may be unethical.

Follow-up

August 2008

We have managed to find details of whom to contact regarding informing the author's institution, and the deputy editor has written to him. We await a reply with anticipation.

May 2009

The institute has responded to say that an investigation is under way and will take another couple of months to conclude.

08-19 Simultaneous publication (LKS)

About a month after our journal (Journal A) published a paper (Paper X), the journal received emails from readers that Paper X was very similar to a paper (Paper Y) that had just been published by another journal (Journal B). Some of these emails were sent to both journal offices. Paper X was submitted to Journal A a few days before Paper Y was submitted to Journal B and Paper X was published in Journal A about 3 weeks before Paper Y. The two papers superficially do not look similar and Paper X contains results that are not in Paper Y. However, the two papers also contain highly similar results and make similar key conclusions. Truth be told, all of the additional results presented in Paper X were added during revisions. The authors of Paper X knew that Paper Y had been accepted by Journal B by the time they submitted the first revision of Paper X to Journal A

The authors of these two papers overlap significantly and the two co-corresponding authors are the same. We contacted the authors. The authors pointed out the obvious differences between the two papers and a few minor differences that most readers would not detect, and insisted that the results reported in these two papers were obtained separately rather than the same results reported in two papers. In addition to those readers who sent emails to us, the reviewers of Paper X and a few editorial board members of Journal A who read these two papers all agreed that the two papers were highly similar.

We contacted the office of Journal B and the Editor, and received a message from the managing editor of Journal B that the Editor of Journal B had asked for and received an explanation from the authors and was satisfied with the explanation.

I would like the Forum's advice on how to handle this case, in particular as it seems that Journal B will not take any further action.

(1) Given that the paper was submitted to Journal A earlier and published in Journal A earlier than in Journal B, albeit only a few days, would retraction by Journal A be appropriate?

(2) If the authors indeed did something wrong, would a simple Concerns on Duplicate Publication be sufficient? Would banning the authors from publishing in Journal A for a few years be appropriate?

(3) Are there other options?

Advice

The Forum agreed, regardless of what the other journal does, the editor should publish a notice of concern. If it becomes clear that the data are the same, then the editor should consider publishing a notice of duplication. It is COPE's policy not to encourage banning authors or to apply any other sanctions, partly because of the risk of litigation.

The Forum was interested in whether or not journal B was a member of COPE, as if this were the case, the chair of COPE would be prepared to write to the editor.

Follow-up

August 2008

The case is being investigated by the author's institution.

May 2009

As mentioned in the last update to this case, this case was being investigated by the authors' institution. A response was received from a Pro-Vice-Chancellor and Vice-President of the university. According to this letter, the investigation group consisted of one external expert (who coincidentally is a member of Journal A's editorial board) and two university professors. The Group's report was discussed among the Vice-Chancellor and some senior leaders of the university, including the Pro-Vice-Chancellor and Vice-President who sent a response to the editor, and has been accepted by the university. Because this response was sent as a hardcopy instead of an electronic file, a summary of the key findings and conclusions of this investigation are shown below by directly copying appropriate statements from the response.

"The Group was informed by (the authors) that the two studies—were conducted in two consecutive time periods with completely different patients, and the number of cell lines used were not the same." "The Group saw some raw data supplied by (the authors)". "Based on the findings reported by the Group, the Vice-Chancellor and I are satisfied with the explanation that the data reported in the two papers came from two sets of experiments, and there is no evidence that the data were altered for the purpose of publication in two different papers". "(The) Group pointed out that there appeared to have been significant double publication of a large portion of the (Journal B) paper's data in the (Journal A) paper". "Yet, the Investigation Group noted the defence of the co-corresponding authors—that the two studies had been conducted with two different patient groups in two different time periods, with the second study having some modifications of protocol and methodology". "The Group's report stopped short of saying that duplicate publication of the same data had indeed happened, although the Group made it clear that reporting similar data in different publications without proper referencing was not good practice, which the university entirely agrees." "Our Vice-Chancellor intends to issue a letter of reprimand to the lead/co-corresponding author" "Another letter will be sent by the Vice-Chancellor to the other co-corresponding author reminding him of his proper responsibilities as a senior author and a department head in ensuring that commonly accepted codes of practice in the academic and research community, not to mention the university's relevant policy, should be followed by researchers under his guidance." "The university has accepted the Group's report, and has advised the authors of the (Journal A) paper of the University's decision accordingly."

Journal A is satisfied with this investigation conducted by the authors' institution, and the communication regarding proper author/researcher conduct in publications from the authors' institution to these authors.

08-23 A(uthor) vs C(omplainant) authorship dispute (WR/RH)

Background

A was a researcher in C's lab for 1 year, during which time they published a joint research paper in a third party journal (journal S). After leaving C's institute (henceforth called institute X), A published in the journal (journal T as a sole author). The affiliation provided by A on the paper was institute X. All of the data reported in this paper were obtained while A was still employed at institute X, of which C is a senior faculty member and in whose laboratory A was a research associate.

C contacted journal T with several allegations regarding A's paper in journal T. These were:

- (1) A submitted the paper after having left institute X
- (2) A never discussed with C his intention to publish these data from C's laboratory
- (3) Material published in journal T is "fully based" on data they had previously published together
- (4) A did not have approval to use material published in journal T
- (5) Questions two of A's methods/techniques and the data that resulted
- (6) A never acknowledged a funding body

An editor on journal T reviewed these allegations and believes that A's paper was a follow-up paper, and that it appears to take further the research A and C had previously published in journal S. The paper had undergone peer review on journal T by one specialist referee, who provided a full and penetrating report.

A admits to claims:

- (1) The work was conducted at institute X
- (6) A admits to a mistake here

A disputes points:

- (2) A claims to have raised publication multiple times with C, C's role in generating the "results were almost non-existent" but indicates that C claimed the data were C's for C to decide
- (3) The paper is a follow-up to their previously published work
- (4) A personally analysed the material with the material produced by a technician
- (5) A claims that the materials are different images of different samples

Journal T submitted relevant correspondence to the dean at institute X and asked them to investigate. Institute X's dean responded 4 working days later endorsing C's position on the basis that A had duplicated material, had misrepresented its novelty and did so both without permission or agreement from C.

The investigation by institute X failed to communicate with A or ask for A's response to the matter. When the findings were presented to A by journal T, A strongly refuted its key points.

Journal T's position has been to ask institute X to investigate fully the above allegations (including communicating with A) and to make an official statement linked to the case.

However, we seek COPE's recommendation whether this should be our position or whether they would advise a different approach.

Advice

The Forum acknowledged that the institute failed to handle this case correctly and that their investigation is of little value. However, there is little that the editor can do about the institutional failing other than reporting it to someone more senior than the Dean of the institute. The Forum believed that there was a case for asking institute X to carry out an unbiased independent investigation. In the absence of an independent review, the editor could publish a "notice of dispute", informing readers of the situation and explaining that it is not for the editor to decide the rights and wrongs of the case. All agreed that the editor had handled the case very well and agreed with his course of action.

Follow-up

The editor requested an independent investigation from the institute. It is the editor's belief that once he asked for an independent investigation, one that is truly independent and would have taken some effort on their part, that the institute decided it was not worth the trouble. The editor suspects that he will not hear from them again.

08-28 Advice regarding a weird type of content and its authorship (PST)

Background

Our company publishes clinical pathways.

They were initially authored by local experts, but have since been retrofitted with evidence, if possible. This was done by expert "evidologists", not clinical experts; they were acknowledged solely by their company name (it was "out-sourced"). If the evidence did not fit, the pathway was discarded.

We undertake to review all of the pathways annually. However, the annual review process is often the first time a clinical expert is reviewing the pathway. As a result, their "suggested revisions" are extensive. We would like to make such people authors (if they agree), partly so we can collect and display their competing interests and partly so that they can get "credit" for their work. This means that each year there could be new authors (reviewers with useful comments).

My questions are:

- Should we make all reviewers authors, even if their report is pants?
- I suspect not, but can you think of a useful threshold as to when a reviewer has done enough to be an author?
- Year on year, we would have to check the "original" reviewers (ie, the first years') are still happy with the revised pathway, right?

- But what if there are authorship disputes? We would have to mediate them, right?

Does this approach to authorship make any sense at all?

Advice

The Forum advised against getting too bogged down with the terms “author” and “authorship”. These terms can be redundant in some forms of publication. Terms such as reviewer or contributor can be used and are just as valid. The Forum emphasised the importance of transparency about who writes what and when. Hence the Forum advised identifying an updated version of the pathways, with a list of those who contributed to it. This could be linked to previous versions, with a similar list of contributors, whether or not they are the same. Another suggestion was to ask the reviewers and the people who use the pathways what they want – do they want to be acknowledged? Do they want to see who has contributed what to each pathway?

Follow-up

We are grateful to the participants of the recent COPE Forum for helping us to understand how “authorship” can be dealt with in a non-traditional editorial model. Our thoughts continue to evolve on how we will handle this, but we are likely to list “authors” as a group, assuming that they have all participated under the aegis of one body or leader. Others who participate will be listed, and we will operate no minimum threshold for their involvement, although we will probably reserve the right to reject the offerings of those who have been pants. All “authors” will be called “contributors”, if we need a name for them at all, so as to avoid any conflict with the definition of authorship provided by the International Committee of Medical Journal Editors (ICMJE). Taking such an approach will ensure that we are not only collecting the names of those who provide valuable contributions, but also ensure that we can make public any and all potential conflicts of interest so that the reader is fully aware of potential biases.

08-32 Unethical withdrawal of a paper (JL)

Background

The terms author A and author B will be used to refer to the corresponding and non-corresponding authors, respectively, of the paper in question. The term Editor will be used for Editor A of our journal and Editor B of the other journal involved. The term Editorial Assistant will be used to refer to the person who is in charge of correspondence for our journal.

Author A presented a paper to a conference and submitted the paper to the conference proceedings. After the conference and before the papers were selected for consideration for the conference special issue, Author A submitted a paper to Editor A for review. The only comment Author A made regarding the status of the paper is ‘Finally, this paper is our original work and has not been submitted to any other journal for reviews.’ No mention was made that the paper was in a conference proceedings or is under consideration for possible inclusion in other journals.

Editor A decided to handle the review process himself and assigned reviewers to review the submission. Later, Author A sent a letter to the Editorial Assistant indicating that he/she was requesting that the submission be withdrawn from review. Author A’s note is shown below:

“I have a problem with this paper. I presented an earlier version of it at the xxxx conference in xxxx this xxxx. Each year the Editor B’s Journal xxxx runs a special edition based on xxx and our paper has been short listed from the papers presented at this year’s conference. So unfortunately I am going to have to withdraw it from your journal. However, this is part of an ongoing research project and hopefully we will have some more interesting results to submit to your journal within the next few months. Thank you for the time spent on this paper and apologies for any inconvenience caused.”

Editor A called Editor B and inquired about Author A’s paper. Editor B indicated that he was unaware that the paper was under review when he solicited it. Editor B stated that it was not his intent to solicit papers under review. Editor B indicated that he believed that Author A accepted his offer for special issue consideration after Author A sent a withdrawal letter to Editor A. Editor B agreed that Author A’s paper was under review at the time that the letter from Editor B was sent.

Editor A delayed taking action as he was to meet with the publisher in the near future and wanted to verify with them that his intended action—a ban for double submission—was suitable. On returning from meeting with the publishers, Editor A discovered that the paper had never been withdrawn from consideration and the review process was now complete (entire review process 28 days). The reviewer comments warranted rejection. Editor A sent a rejection letter to Authors A and B advising them that the paper was rejected, it was double submitted and comments were being forwarded as the paper was never actually withdrawn and that a submission ban was being placed on them for the journal for 5 years.

Author A and Author B attempted to correspond with Editor A. However, Editor A never received notification of these attempts as the Editorial Assistant did not bother to read Author A’s or B’s or any other emails for a long period of time. (The Editorial Assistant was replaced for performance reasons shortly afterwards.) Editor A received a telephone call from Author A, since Author A did not receive a reply. Editor A responded by email to Author A’s voice message advising that the decision taken was not open to discussion.

Author A contacted the publisher stating that if a retraction of the statement of double submission and journal ban is not made, he may take legal action.

Editor A’s position is that double submission occurred because Authors A and B demonstrated their intent to have their paper considered by both journals simultaneously by withdrawing their paper as soon as Editor B expressed an interest in it. If either Author A or Author B asked Editor A’s opinion on what to do, Editor A would have stated you are banned from the journal for double submission unless you allow the review process to complete. This opinion is not only held by Editor A, but is typical in the field. Editor A has many letters from authors who participated in a recent conference asking if their papers have been selected for a special issue, so they know whether they need to wait for a decision or should go ahead and submit their paper to an alternative outlet.

Editor A is willing to retract the use of the term double submission as he can see that the authors can reasonably argue that if the paper submitted to Editor B was given to Editor B after

the rejection note was sent by Editor A one can make an interpretation that the paper was not physically under consideration at two places at once.

However, Editor A is not willing to retract the 5 year ban because Author A and Author B have abused the review process. They have wasted the time of two reviewers. Editor A typically waits for a year prior to asking reviewers who are not on the editorial board to consider another paper.

If a 5 year ban is retracted, Editor A fears that Authors A and B will continue to abuse the review process at his journal and that other authors will do the same. As the number of submissions has increased by almost 300% over the past 3 years, Editor A believes that anonymous reviewers warrant as much respect and consideration that can be offered.

In summary, Editor A feels that the intent to have a paper considered at two places simultaneously is unethical and is accurately described by the term double submission. Editor A is willing to retract the term double submission. Editor A will retract a 5 year submission ban, under extreme protest, if told that Author A and Author B have acted in an ethical manner.

Advice

The advice from the Forum was that ideally both editors should stand together and reject the paper but the Forum was told that editor B is unwilling to do this. The Forum noted that although the authors are responsible for wasting the time and editorial resources of the journal, authors are entitled to withdraw their papers. The proper course of action would have been for the authors to have rejected the offer from Journal B saying that their paper was under consideration elsewhere. However, the authors did request that their paper be withdrawn from Journal A but there seems to be some confusion regarding the withdrawal process (the editor informed the Forum that an administrator in Journal A's office had been inefficient).

The Forum questioned whether or not the authors then believed the paper had been withdrawn and so they were free to submit it elsewhere. As it is unclear if or when the paper was actually withdrawn, the Forum agreed that a 5 year ban was too harsh. Also, COPE reiterated its views on sanctions. COPE believes that sanctions should be imposed only if misconduct has been proved after a proper formal process has taken place, involving an independent panel where the author is allowed to present his case. Otherwise sanctions can be seen to be unfair and could provoke litigation.

All agreed that the best sanction is to decline publication of the paper. If the editor feels he would like to take it further, then he could contact the author's institution and request an investigation.

Follow-up

I informed the authors involved in the incident that this issue was taken to the COPE Forum and that the suggestion was that the recommended manner for dealing with situations in which there is a question of academic dishonesty was that the paper be rejected and that information regarding the incident be forwarded to the host university(s) so they can conduct an investigation and act as they see fit. I then indicated that we have adopted this recommended procedure to address future cases and as this case entered prior to the onset of this policy, the

original submission ban is lifted and we will not be submitting the information to the universities.

09-03 Multiple publication of research (PP)

Background

We submit to COPE a case regarding the suspected multiple publication of research on four separate occasions in four different journals.

Close inspection of the articles in question revealed that the author had directly copied and reused extensive sections of text, including tables in all four articles. After this matter was brought to the attention of the Editor of Journal A, the chronology detailed below was pieced together. It demonstrates that there is significant overlap in the dates the articles were submitted and copyright forms signed. This, coupled with the extent of the similarities between the articles, lead us to believe that the author would have been aware they were submitting near identical articles to multiple journals over a short space of time.

Upon this realisation the authors were contacted according to the COPE guidelines and the lead author cooperated with us in putting together the information below. Because of the complexity of the case, the number of journals and the time frames involved, we would be grateful if COPE could advise us on the correct course of action.

- In January 2002, three UK authors submitted a paper to Journal A based on the lead author's PhD thesis. This paper was accepted by Journal A in October 2003 and a copyright assignment form was signed in November 2003. This article appeared in Journal A in January 2005.
- In July 2002, the lead author presented a conference paper based on the same PhD research and submitted it to Journal B. This too was accepted and a copyright agreement was signed in June 2003. This article went on to appear in Journal B in January 2004. This paper acknowledges the conference it was given at.
- In June 2003, the lead author gave another conference paper based on their PhD research which was subsequently published in Journal C in December 2003. The copyright assignment form for this article was signed in September 2003.
- In January 2006, another paper was published in Journal D, again based on the same PhD research. We do not know when this was submitted or when the copyright form was signed.

Advice

The advice from COPE was to consult the flowchart for "redundant publication in a published paper". The flowchart advises that you check the degree of overlap. If it is substantial, contact the authors. If an unsatisfactory response is received, the editor should consider publishing a notice of redundant publication or retraction. Sometimes the author makes a genuine mistake or the instructions to authors are not clear enough (does your journal say that submitted work should be original and not submitted elsewhere) or the author is very junior. In such cases writing to the author explaining the situation and outlining the expected behaviour is sufficient.

However, if the editor is satisfied that this is not a simple error and an unsatisfactory response has been received, he should consider contacting the author's institution and asking them to investigate. The Forum would also advise contacting the other journals.

Follow-up

We have agreed with the publisher that the paper will be retracted.

09-04 Possible serial misconduct in relation to coauthors and other activities (GT)

Background

I am the editor of an international clinical journal and am facing a very unusual problem that does not fit readily into COPE flowcharts.

Through a reviewer, I was informed that an author had submitted a paper without the approval of at least one of the other authors. This appeared to be confirmed by two other authors. In response to my bringing this possibility to the first author's attention, he asserted that all coauthors had given informed consent to publish the work as it is. I have requested that he provides written corroboration of this. If this is not forthcoming I will send the paper to the other authors and seek permission to identify their views to him.

Another reviewer raised concerns about the ethics of a component of the submitted investigation. The author has responded that the work was investigated by the university to which at that time he was affiliated and received ethics approval. I have requested written confirmation of this. The author is no longer at the institution at which the work is reputed to have been conducted: he quotes as his current affiliation an institution that does not exist and gives only an email contact.

Additional criticisms of the work from a scientific perspective made it clear it was not acceptable for publication and I have informed the author of this. The author has two other manuscripts in submission and I have requested documented confirmation that the listed coauthors of these approve of their content. Furthermore, my attention has been drawn to items in four other journals raising issues about coauthors' approval of other papers from the same first author and additional concerns about misappropriation of material from the publications of others.

In his response to my request for clarification of the issues raised with regard to the paper I rejected, the author stated his intention to "formally let open further legal steps against you". Nevertheless, it seems clear that I have a responsibility to continue investigating the foregoing issues.

The journal has no established regular mechanism for this circumstance. A way of proceeding could be, after as much possible information has been assembled, to draw together a small panel, including appropriate experts and representatives from the sponsoring international society whose task would be to review the information (in anonymised form) and advise on any further action, both from the point of view of my journal and the wider issues. In the absence of an identified current employer, it may be that the institution at which the work was

performed is the most appropriate to charge with responsibility for any further investigation and action.

Is the presumption correct that, in the event of the author translating his statement about legal steps into action, the editorial team and others involved on behalf of the journal would be indemnified by the publisher?

Advice

The Forum noted that this is a case that perhaps can never be satisfactorily resolved. It is very difficult for editors to intervene in authorship disputes. The advice was for the editor to contact the author's institution and ask them to investigate the authorship dispute. As the editor suspects unethical research, that is another reason to approach the institution, report the matter and request an investigation. The Forum noted that this is as much as the editor can do with regard to the submitted papers. On the wider issue, the editor could publish an expression of concern for the papers already published and alert the other journals where the previous papers have been published. In the UK, a physician could be reported to the GMC for his conduct – there may be a similar body in other countries to whom the author's behaviour could be reported.

Follow-up May 2009

In brief, I took advice, based on an anonymised set of information, from the three chairmen of the relevant committees of the European Society of which the journal is the official journal. Their view was clear that the various concerns amounted to a serious departure from appropriate standards.. They concurred with the recommendation that responsibility for action should include the employing institution. This was communicated to the main and co-authors. However, at this stage, communication was received from a lawyer on behalf of the main author and the matter has been taken up by the publisher's lawyers.

09-06 Duplicate publication? (ECP)

Background

We are fortunate to have very knowledgeable reviewers who are immersed in their specialty and in the literature. A reviewer informed us that s(he) was working on a review of a manuscript and thought that there had to be more qualitative studies on this subject. S(he) began to look and found three articles not cited by the author and then a fourth.

The fourth study was published in another journal but was written from the exact angle, reporting the same data and in the same way as the article submitted to us. While some of the wording had been changed and the introductory material moved around a bit, it was essentially the same study. The quotes describing each category had also been changed, but little else. Surprisingly, one of the major differences is that the published article contained a much richer explanation of the methods, a sample description and study limitations than the version submitted to us. We do not understand what the author was thinking, submitting essentially the same paper, albeit one of lesser quality.

I ask that COPE provide me with recommendations as to the follow-up course with the author.

Advice

The Forum was unclear about whether the same authors were involved—was this a case of plagiarism or duplicate publication? The Forum also noted that any action depends on the journal guidelines. Does the journal document in its guidelines how much overlap is allowed? If the authors are the same, the advice was to follow the steps in the COPE flowchart “Suspected redundant publication in a submitted manuscript”. The flowchart advises that you check the degree of overlap. If it is substantial, contact the authors and request an explanation. If an unsatisfactory response is received, reject the paper and contact the authors explaining your position and the expected future behaviour. The editor might also like to consider contacting the author’s institution and informing them of the author’s misconduct.

Sometimes the author makes a genuine mistake or the instructions to authors are not clear enough (does your journal say that submitted work should be original and not submitted elsewhere?) or the author is very junior. In such cases writing to the author explaining the situation and outlining the expected behaviour is sufficient.

Follow-up

May 2009

We rejected the article and the author said he learned an important lesson. The editorial board met and it was unanimous that the situation should be reported to the author’s university academic integrity committee for review.