PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Systematic review on the primary and secondary reporting of the prevalence of ghostwriting in the medical literature
AUTHORS	Stretton, Serina

VERSION 1 - REVIEW

REVIEWER	Fernando Alfonso Cardiac Department. Hospital Universitario de La Princesa. Madrid. Spain
REVIEW RETURNED	23-Feb-2014

GENERAL COMMENTS	This study provides a systematic review on how evidence for ghostwriting is reported. With this aim relevant bibliometric databases were searched electronically to identify publications reporting estimates of ghostwriting prevalence (primary outcome measure). Out of 848 publications screened, 48 reported estimates for possible ghostwriting. Sixteen primary publications reported findings from surveys or descriptive studies whereas 32 were secondary publications. Estimates on the prevalence of possible ghostwriting in primary and secondary publications varied markedly. Results were influenced by the definition selected, population type and whether evidence from primary publications was correctly cited. Ghostwriting remains an important editorial and scientific problem. Evidence on prevalence of this phenomenon is limited. Efforts to systematically analyze the evidence on this phenomenon are of scientific value. In this regard the current study is of interest Some issues, however, should be addressed: 1. Readers may be interested in the definitions used by the international committee of medical journal editors ICMJE and on whether or not they differentiate ghostwriting from ghostauthors. This appears important because according to table 2 "…but most definitions did not differentiate contributions that merited authorship from those that did not merit authorship…" 2. A total of 124 publications were excluded because they did not report a numerical estimate of the prevalence of possible ghostwriting. However, summarizing "qualitative" information from these studies would be of additional value to the readers as this may complement the numerical estimates summarized in the current report. 3. Some of the selected studies had response rates of less than 50%. It would be of interest to see if the main study findings remain

	constant when these publications with a very low response rate are excluded 4. Further data on the methodology used in this systematic review would be of help. Did the study follow the available recommendation on systematic review and metaanalysis? (PRISMA etc). 5. Numerical estimates for primary publications are easy to understand. However, the way numerical estimates were obtained in secondary publications remains obscure especially as they differed from the original source. Please clarify 6. The main novel findings of this study with regard to previous knowledge should be emphasized. Many previous reports and surveys support the idea that this phenomenon is quite prevalent. Please address. Likewise, the prevalence varied according to the type of documents yet an interpretation on the different prevalences according to the type of studies would be illustrative

REVIEWER	Adam Jacobs Dianthus Medical Limited UK My company provides professional medical writing services
REVIEW RETURNED	25-Feb-2014

GENERAL COMMENTS	There are some problems with the results. On page 15, first paragraph, the results of the surveys by Jacobs and Hamilton are presented as showing that the % figures quoted for ghostwriting refer to the % of association members who had ghostwritten at least once. This is incorrect. The figures refer to the mean % of contributions that were ghostwritten. It is not clear how the definitions of ghostwriting as reported in table 2 match the standardised definitions given in the last paragraph of page 7. For example, the Gotzsche 2007 paper (reference 29) used, as is correctly described in the text, a non-standard definition of ghost authorship. It is not clear how this was categorised into a standard definition, and indeed I doubt that it is even possible to do so given the unusual definition used by Gotzsche et al. The case study of the Healy & Cattell paper makes fascinating reading, but in fact the evidence from that paper is even more misleading than described. One problem with the Healy & Cattell paper that has not been addressed is that it did not include any assessment of ghostwriting. Not only did it include 2 papers where a medical writer was acknowledged, and so would not fit a standard definition of ghostwriting, but it may also have included papers which did not receive writing assistance, so were also not ghostwritten. So the 57% figure is too high, as the 55 articles were not all ghostwritten. But it's even worse than that. The other problem is that the numerator and denominator used to calculate the % of ghostwritten articles are really not comparable. The numerator included all the articles written by the medical communications company, as it was based on an internal document, and is therefore a complete list.

	However, the denominator is based on a rather unsophisticated Medline search, and probably missed many articles. Therefore the total number of sertraline articles is almost certainly higher than the 96 reported, which would reduce the % of ghostwritten articles still further (though of course it has to be acknowledged that it is possible that some articles other than the CMD ones may also have been ghostwritten). Finally, the Prisma checklist is not complete: some items are reported as "not applicable". Although this is reasonable for some items, such as details of the meta-analysis (given that there was no meta-analysis), other items should be described. For example, item 5 on "protocol and registration" should at least state that the protocol was not published if it wasn't. This is an important review. Much has been written about the prevalence of ghostwriting, and much of it is very misleading. This paper makes an extremely useful contribution to the literature on this topic, and I have no hesitation in recommending it for publication once the minor problems with the results section are corrected.

REVIEWER	Jonathan Leo Lincoln Memorial University United States No Financial conflicts. I have written several papers on ghostwriting.
REVIEW RETURNED	28-Feb-2014

GENERAL COMMENTS	Readers of medical papers should be able to look at a byline and trust that the names on the byline were the people who wrote the article and that there are no invisible authors – or ghostwriters. The medical community is very concerned about papers whose bylines contain invisible authors. Most people refer to this as ghostwriting. The uproar in the medical community is not about the accuracy of the acknowledgement section. The uproar is about the accuracy of the byline. Traditionally, the acknowledgement section of the paper is for mentioning those contributors who did not rise to the level of authorship. The paper under consideration implies that the medical community is very concerned with accuracy in the acknowledgment section. In the process they develop a unique definition of ghostwriting that applies to the acknowledgement section and not the byline. Based on the definition of ghostwriting that these authors use I reject this paper. The authors state, “Ghostwriting occurs when writing contributions to a manuscript that do not meet authorship criteria are not disclosed in the acknowledgements. “ If someone rises to the level of authorship then they need to be mentioned on the byline – not simply mentioned in the acknowledgement section. As just one example, the very journal they have submitted this paper to – BMJ Open – could not be clearer. In their discussion about authorship criteria, the BMJ Open Editors state: “Conversely we ask for assurance that there is no one else who fulfills the criteria who has not been included as an author.” In other words, if someone

deserves to be called an author then they need to be listed as an author.

A real world example illustrates the problem with using this paper's definition of ghostwriting. The most-often cited ghostwritten paper in the literature is *Study 329*. The reason it is considered ghostwritten is because Sally Laden, the medical writer was not included on the byline. For the readers there was an invisible author. Even though Sally Laden was mentioned in the acknowledgement section this was not the appropriate place to mention her. If one accepted the authors' definition of ghostwriting then Study 329 would not be considered ghostwriting.

When it comes to determining if a paper has been ghostwritten there is a very simple question to ask: Has a deserving author been left off the byline?" If the answer is "Yes" then the paper has been ghostwritten. The acknowledgement section does not matter. The acknowledgement section is not listed in pub med, it is not listed in systematic reviews, and it is not cited. And trying to separate ghostwriters from ghost authors is immaterial. The authors own table (Table 3) shows that this is how most people define ghostwriting and that most of the papers cited in the table do not draw a line between ghost writing and ghost authoring.

The authors also state that estimating the prevalence of ghostwriting is hampered by having different definitions of ghostwriting. However the real hindrance to estimating ghostwriting is the secretive nature of the process. Is it mainly because of legal proceedings that we know about ghostwriting. Traditionally authors don't volunteer this information because from a pharmaceutical company's point of view the main value of ghostwriting is that the company remains invisible. While we don't know the exact prevalence, we do know that virtually every single blockbuster medication is embroiled in billions of dollars of lawsuits and that there are also charges of ghostwriting for all these medications. The true prevalence is hard to detect, but there is flip side to this, which the authors downplay: Ghostwriting is neither rare nor is it limited to just a few medications.

It is important to point out here that there is nothing wrong with using medical writers. However, when a medical writer's contribution rises to the level of being called an author then the byline should reflect this. Many journal articles do this. Mentioning the medical writer in the acknowledgement section does not excuse them being left off the byline.

The authors are trying to draw a line between ghostwriting and ghost authoring. However, even if we follow these definitions there appears to be a problem. Under their guidelines, if the prevalence of ghostwriting goes down, then the level of ghost authors should go up. Take Study 329, under their definition, it would not be categorized as a "Ghostwritten" study but it would be considered a "Ghost Authored" study – a deserving author was left off the byline. Thus to simply say that Study 329 is an example of overestimating

	the prevalence of ghostwriting would only be half the story. Readers would have to be told that it has been re-categorized as "Ghost Authored." Since both practices are problematic I am not sure of the significance of a simple recategorizing of papers. The current study seems very concerned with declaring that under their unique definition the rate of ghostwriting goes down, but I don't see the same concern with declaring that ghost authoring estimates will go up. When most members of the general public hear the word ghost writing they think about who is getting credit on the byline. For instance see the recent series of articles in *The New York Times* on the musician Mamoru Samuragochi who is accused of having his pieces 'Ghostwritten." The *Times* does not use the term Ghost Authoring. In the traditional sense ghostwriting is a pejorative term. In contrast, the authors of this paper seem to be trying to redefine ghostwriting so that it is a rather innocuous term that is concerned with the accuracy of the acknowledgement section. I think the paper is trying to meld an editorial decision with a scientific paper. I think they need to do one or the other. One option would be to stick to a scientific paper and attempt to determine the true prevalence of ghostwriting. If they go this route, these authors should use the more generally accepted idea of ghostwriting and to focus on the byline and not the acknowledgement section. It is quite possible that the prevalence of ghostwriting is overestimated, but simply changing the definition is not the way to determine this. Conversely, if they want to change the commonly accepted definition of ghostwriting then I think they should write a commentary attempting to justify their ideas. While my recommendation is to reject the paper, if the paper is eventually published it will hopefully force the ICMJE to directly address the issue of ghostwriting in their own guidelines. When the ICMJE guidelines were written they were primarily concerned with making sure that all the named authors on a paper deserved to be there. At that time, the idea that the byline might not list all the deserving authors was not a major concern. Currently, this loophole in the ICMJE guidelines leads to medical writers and companies attempting to justify or excuse the presence of unnamed authors on the byline. Because of this, several journals, such as Neurology, have enacted guidelines that are stronger than the ICMJE guidelines at to ensure accurate bylines. The ICMJE needs to toughen their guidelines so that all those who deserve to be called an author are listed on the byline.

VERSION 1 – AUTHOR RESPONSE

REVIEWER: 1

1. Readers may be interested in the definitions used by the international committee of medical journal editors ICMJE and on whether or not they differentiate ghostwriting from ghostauthors. This appears important because according to table 2 "…but most definitions did not differentiate contributions that merited authorship from those that did not merit authorship…"

AUTHOR RESPONSE: The ICMJE do not provide specific definitions for ghostwriters and ghost authors. Rather, the ICMJE differentiate contributors as those who qualify for authorship and those who do not. Using the ICMJE definitions, a contributor qualifies for authorship if he or she meets all of the four ICMJE authorship criteria (listed below). These authorship criteria reinforce that authors are responsible and accountable for the published work.

1. Substantial contributions to the conception or design of the work; or the acquisition, analysis, or interpretation of data for the work; AND
2. Drafting the work or revising it critically for important intellectual content; AND
3. Final approval of the version to be published; AND
4. Agreement to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

The ICMJE recommend that contributors who meet fewer than the four criteria listed above should NOT be listed as authors. Rather, these individuals should be acknowledged. The ICMJE lists writing assistance, technical editing, language editing, and proofreading as examples of activities that, without other contributions, do not qualify for authorship. Hence, a professional medical writer who provides writing assistance and who does not meet all four of the criteria listed above should NOT be listed as an author. Rather, his or her contributions should be acknowledged in the acknowledgements section.

The author has added a sentence to the introduction of the manuscript to highlight the ICMJE's position on how writing assistance should be acknowledged when the person who provided the writing assistance does not meet the ICMJE authorship criteria, but has contributed to the manuscript.

2. A total of 124 publications were excluded because they did not report a numerical estimate of the prevalence of possible ghostwriting. However, summarizing "qualitative" information from these studies would be of additional value to the readers as this may complement the numerical estimates summarized in the current report.

AUTHOR RESPONSE: The author agrees with Reviewer 1 that qualitative information on how estimates of the prevalence of possible ghostwriting are described in the literature would add additional value. However, the intention of the current work was to first investigate how numerical evidence was cited and reported. Conduct of a qualitative study would require different methodology and would be a separate (and possibly larger) study.

3. Some of the selected studies had response rates of less than 50%. It would be of interest to see if the main study findings remain constant when these publications with a very low response rate are excluded

AUTHOR RESPONSE: The main findings from this study were that reported estimates of the prevalence of ghostwriting in the medical literature are highly variable and are limited by the varied

definitions used, the types of study designs employed, and the populations assessed. Exclusion of cross-sectional surveys that did not report a response rate or that reported a response rate less than 50% would be unlikely to change these findings. There were six studies that did not report a response rate or reported a response rate less than 50% (Table 1). The prevalence of ghostwriting or ghost authoring in these studies varied from 0.7% to 70% (Table 2).

4. Further data on the methodology used in this systematic review would be of help. Did the study follow the available recommendation on systematic review and metaanalysis? (PRISMA etc).

The methods for this systematic review were conducted, as much as possible, according to PRISMA. Please note that a PRISMA checklist was submitted with the author's original manuscript. Several of the PRISMA items were not applicable as the data were not synthesised and a meta-analysis was not done. The data retrieved in this systematic review were not suitable for meta-analysis because of the limited quality of the studies and the considerable heterogeneity in the definitions used to describe unethical authorship practices, the types of study designs employed, and the populations / samples assessed.

5. Numerical estimates for primary publications are easy to understand. However, the way numerical estimates were obtained in secondary publications remains obscure especially as they differed from the original source. Please clarify

The intention of this study was to assess how the numerical estimates for the prevalence of ghostwriting were reported in primary and secondary publications. The intention of including secondary publications was to determine (i) whether other data not reported in primary publications were available and (ii) to determine the accuracy of re-reporting of data from secondary publications. As such, all data included in the systematic review were included as published in the secondary publications.

The methods section (data analysis and extraction) has been modified to make it clear that the prevalence of possible ghostwriting was reported as published. No unpublished data from the retrieved literature were reported and values for prevalence were not extrapolated from published data.

6. The main novel findings of this study with regard to previous knowledge should be emphasized. Many previous reports and surveys support the idea that this phenomenon is quite prevalent. Please address. Likewise, the prevalence varied according to the type of documents yet an interpretation on the different prevalences according to the type of studies would be illustrative

This study provides a comprehensive review on papers analyzing the problem of ghost written. Although presented as a systematic review the information remains manly qualitative in nature. The novel findings of the current study with regard to existing knowledge should be clarified. Authors claim that this is the first systematic study on this subject yet the results remain rather qualitative in nature. It would have been of additional interest to elucidate the correlation between prevalence estimates and type of publications

AUTHOR RESPONSE: The author agrees with the Reviewer that many previous reports and surveys support the idea that ghostwriting is prevalent. The intention of this systematic review was to investigate how ghostwriting is reported in the medical literature and to investigate, as much as possible, the source of the data that supports the idea for the high prevalence of ghostwriting.

However, it was not possible to conduct any correlation analyses or synthesise the data because of the small number of studies retrieved and the considerable heterogeneity in the outcome measures (eg, definitions of ghostwriting), the study methodologies, and the populations / samples included. Because of this heterogeneity, the data were reported as described in the original publications and the interpretation of the findings remained observational.

The abstract and results section have been updated to emphasise that the data retrieved were not suitable for meta-analysis because of the limited quality and various definitions, study designs, and populations / samples assessed.

REVIEWER: 2
On page 15, first paragraph, the results of the surveys by Jacobs and Hamilton are presented as showing that the % figures quoted for ghostwriting refer to the % of association members who had ghostwritten at least once. This is incorrect. The figures refer to the mean % of contributions that were ghostwritten.

AUTHOR RESPONSE: The author thanks the reviewer (and author) for this clarification. The manuscript has been been revised to clarify that for this study the reported prevalence is the mean weighted percentage of manuscripts that were ghostwritten.

It is not clear how the definitions of ghostwriting as reported in table 2 match the standardised definitions given in the last paragraph of page 7. For example, the Gotzsche 2007 paper (reference 29) used, as is correctly described in the text, a non-standard definition of ghost authorship. It is not clear how this was categorised into a standard definition, and indeed I doubt that it is even possible to do so given the unusual definition used by Gotzsche et al.

AUTHOR RESPONSE: The descriptions of possible ghostwriting provided in Table 2 were not intended to reflect, or be categorised into, standard definitions of ghostwriting. As such, the definitions in column 2 were reported as closely as possible to the definitions provided by the authors of each primary study. Any interpretation of the alignment of the definitions provided by the authors of the primary publications with standard definitions was reserved for the text. The heading of column 2 has been modified to make this more clear.

The case study of the Healy & Cattell paper makes fascinating reading, but in fact the evidence from that paper is even more misleading than described. One problem with the Healy & Cattell paper that has not been addressed is that it did not include any assessment of ghostwriting. Not only did it include 2 papers where a medical writer was acknowledged, and so would not fit a standard definition of ghostwriting, but it may also have included papers which did not receive writing assistance, so were also not ghostwritten. So the 57% figure is too high, as the 55 articles were not all ghostwritten.

But it's even worse than that. The other problem is that the numerator and denominator used to calculate the % of ghostwritten articles are really not comparable. The numerator included all the articles written by the medical communications company, as it was based on an internal document, and is therefore a complete list. However, the denominator is based on a rather unsophisticated Medline search, and probably missed many articles. Therefore the total number of sertraline articles is almost certainly higher than the 96 reported, which would reduce the % of ghostwritten articles still further (though of course it has to be acknowledged that it is possible that some articles other than the CMD ones may also have been ghostwritten).

AUTHOR RESPONSE: The author agrees with the Reviewer that, to a certain extent, it is difficult to interpret the data from the Healy & Cattell paper. Healy & Cattell did not provide, or appear to

prespecify, a definition of ghostwriting for their analysis and the reporting of the literature search and denominator for estimates of ghostwriting were not well described. However, as the intention of this review was to assess how ghostwriting is reported in the published literature, the data from Healy & Cattell were included in the manuscript as originally reported by the authors.

Finally, the Prisma checklist is not complete: some items are reported as "not applicable". Although this is reasonable for some items, such as details of the meta-analysis (given that there was no meta-analysis), other items should be described. For example, item 5 on "protocol and registration" should at least state that the protocol was not published if it wasn't.

As indicated by the Reviewer, reasons why some items were reported as not applicable (summary measures, synthesis of results, risk of bias) were because the data were not synthesised and because of the considerable variation in study designs, populations, and definitions of ghostwriting used in the retrieved publications. Given the various study designs and methods used across the retrieved studies, the quality of the study methods were assessed on a study-by-study basis. The key factors considered were the study design, definition of ghostwriting used, and study population. The factors considered when determining the quality of the cross-sectional surveys have been added to the Methods section.

A review protocol was prepared for this systematic review that described the search strategy and prespecified the primary and secondary objectives of the literature review, eligibility criteria, data to be collected, and primary and secondary outcome measures. The review protocol was not registered in a public registry, but is available upon request. The methods section has been updated to emphasise the items that were prespecified.

The PRISMA checklist has been updated to provide a rationale for why some items were listed as not applicable and to indicate that a review protocol is available.

REVIEWER: 3

AUTHOR RESPONSE: The author agrees with the Reviewer that (i) readers should be able to “trust that the names on the byline were the people who wrote the article and that there are no invisible authors…”, (ii) that “the acknowledgement section of the paper is for mentioning those contributors who did not rise to the level of authorship”, and (iii) “ If someone [including medical writers – author added] rises to the level of authorship then they need to be mentioned on the byline – not simply mentioned in the acknowledgement section”. However, the author rejects the Reviewer’s perception that the author is “trying to redefine ghostwriting so that it is a rather innocuous term that is concerned with the accuracy of the acknowledgement section.” The author has clearly stated in the first paragraph of the introduction that ghostwriting is highly unethical because the role of the commercial sponsor and any other potential conflicts of interest are hidden. As recognised in the International Society for Medical Publication Professionals (ISMPP) Code of Ethics, ghostwriting, ghost authoring, and guest authoring are distinct, but equally unethical practices that must be banned.

The Reviewer’s main concerns with the manuscript and recommendation for rejection is based on a difference of opinion on the definition of authorship. However, this should not be a reason to reject this manuscript. Although there are differences of opinion in the literature on how authors, ghostwriters, and ghost authors are defined, the definitions of ghostwriting and ghost authorship used in this manuscript are consistent with those used in the cross-sectional surveys conducted by Flanagin (JAMA 2008) and Wislar (BMJ 2011). Further, the differentiation between ethical, transparent, and legitimate assistance from professional medical writers versus unethical and hidden

assistance from ghostwriters or ghostauthors is evident from guidance provided by editors and journals (eg, the World Association of Medical Editors, the Council of Science Editors, the Committee on Publication Ethics, PLoS, JAMA, Lancet, BMJ, NEJM), by medical writing organisations (eg, the European Medical Writers Association, the American Medical Writers Association, the Global Alliance of Publication Professionals, and ISMPP), by academic associations (eg, the Association of American Medical Colleges), and by government authorities (eg, US Department of Justice's Office of the Inspector General – Corporate Integrity Agreements). The author has not attempted to ignore differences of opinion with regard to how ghostwriting and ghost authorship are reported in the literature, nor has the author attempted to hide primary publications that report a high prevalence of possible ghostwriting.

There is much disagreement in the medical literature over how authorship is defined, particularly with regard to what constitutes "substantial" contributions to a publication. The ICMJE criteria for authorship are the most well recognised criteria available. However, as the Reviewer rightly asserts, the ICMJE criteria are not accepted by all and leave much about authorship undefined. Despite this, the recent emphasis on author accountability in the ICMJE's fourth criterion is an attempt to better define authors as those persons who are responsible and accountable for the content of a published work, irrespective of who conducts the writing. This intention is consistent with the Council of Science Editors guidance that "In most cases, authors will be expected to take joint responsibility for the integrity of the research and its reporting". While a professional medical writer can take responsibility for the writing and how research is reported in a manuscript (ie, the writer can serve as the guarantor of ethical medical writing support), the professional medical writer cannot take responsibility for the integrity of the research or be accountable for the clinical interpretation of the findings, UNLESS he or she was involved in the generation of the research or its analysis. Responsibility for the integrity of research and accountability for the clinical interpretation of the findings is, and should always be, the responsibility of the authors.

Contributorship models have the potential to overcome many of the deficiencies of the ICMJE authorship criteria because they take a fully transparent approach to the decisions undertaken in the preparation of a publication. As proposed by Drummond Rennie in the BMJ, contributorship models allow for the identification of authors, but also require detailed descriptions of the contributions of those involved in the planning, conduct, and reporting of research. Further, contributorship that allows for the identification of one or more guarantors who are responsible for the overall content of a publication provides the reader with a clear understanding of the drivers of the intellectual content of the publication. Until a more definitive model of authorship is universally accepted, the controversy and disagreement over definitions of authors, ghostwriters, and ghost authors will remain. However, we should all be in agreement that whatever term is used, the practice of misleading the reader about potential competing interests and hiding contributions to a published work, no matter whether these contributions should be most appropriately disclosed in the author byline or acknowledgements section, is unethical and totally unacceptable. The author hopes that the issues raised in this manuscript, which have already led to healthy debate amongst the peer reviewers, will help raise awareness among authors, writers, sponsors, journal editors, and publishers about the misuse of authorship, the importance of appropriate disclosure of contributions to peer-reviewed publications, and the need to distinguish ethical, legitimate professional medical writing support from the unethical practice of ghostwriting or ghost authorship.

VERSION 2 – REVIEW

REVIEWER	Fernando Alfonso Cardiac Department. Hospital Universitario de la Princesa. Madrid. Spain.
REVIEW RETURNED	22-Apr-2014

GENERAL COMMENTS	Many of the comments have been nicely addressed. However, unfortunately, some of the suggestions have been addressed in a rather elusive manner. Many issues are discussed in the rebuttal letter alone but most of these comments have not affected the revised manuscript. Any way the general interest of the study remains unaltered. Due to the above issues I have slightly lowered my priority score for this otherwise interesting study. I hope this may be of value to the final decision to be made by the Editors

REVIEWER	Jonathan Leo LMU DCOM USA
REVIEW RETURNED	23-Apr-2014

GENERAL COMMENTS	I am rejecting this paper based on their definition of ghostwriting. In their intro, line 21, their definition of ghostwriting is not correct. They state, "Ghostwriting occurs when writing contributions to a manuscript that do not meet authorship guidelines are not disclosed in the acknowledgements" They are using a minimalist definition here. Ghostwriting occurs when a byline does not include someone who should have been listed as an author. It is really a very straightforward definition. I elaborated on this in my earlier review. I do think the BMJ editors and the ICMJE could clarify the term "ghostwriting."