

Appendix (*verbatim*, edited only for spelling mistakes, and structure)¹

[AR = author's response]

Reviewer 1

I'm in the situation of being asked to provide critical comments and suggestions in the form of peer evaluation for a manuscript which is very short and specialized, studying a globally relevant problem as emerged within a small, maybe obscure segment of OA publishing. My discomfort originates mainly from the lack of contextualization of this research in terms of traditional scientometric investigations, i.e. reference-based publication/citation networks and spatial scientometrics. It would have been more informative if the author further analyzed the POA citation data to form networks based on bibliographic coupling, or used geographical maps for presentation (something similar to what Bohannon did in visualizing his OA sting operation).

[AR: This is a major task, and the use of spatial scientometrics is not a skill that I possess. I agree whole-heartedly with the reviewer that such analyses would fortify the message, but at this moment, it is impossible for me to conduct such analyses. The main purpose of this short piece was to draw awareness to an issue that has not been explored yet at all, namely the way in which papers that are published in journals that were listed in the Beall blog lists of „predators” could enter the main-stream literature and reference lists, thus fuzzing the line between academic (peer reviewed) and non-academic or even pseudo-academic.]

The author also missed the opportunity to react critically, as an EIC and founder of GSB, to the increase experienced in the "*level of inclusion of POA journal references in GSB journals*" (line 72-73). Such retrospection could serve the interests of the wider audience, especially editors and reviewers of new OA journals who, in some cases, desperately needs to develop quality assurance guidelines against dishonest researchers. In the absence of such critical retrospection, the situation presented and studied in the ms may suggest at least one, but possibly all of the followings to the reader:

- the review process at GSB journals was flawed (or at least some editors or reviewers acted without due care and diligence), as the peer review process missed, in multiple occasions, to correct the literature base used in the manuscript before, and possibly after publication (as I fail to see discussions about any countermeasures or retraction notices in the text of the manuscript, and I was not able to find any in the GSB website).

[AR: No doubt about that. In fact, retractions in Nature will also indicate that the process is flawed, even in the world's best journals. In essence, which peer review process is perfect? It is precisely because it is porous that makes traditional peer review and the need to assess the influence of predatory publishing so essential. This small piece of mine simply provides a single-case example, and a tiny window on the process and **possible** effects. GSB is now a dysfunctional publisher without functionality, so criticisms of the literature published there will have to take place on blogs, or other suitable formats such as literature reviews.]

-due to this error, GSB as a publisher also may have started to lean towards being "unscientific" -from 2011 onwards and in average, virtually every published paper contained

¹ This Appendix contains 1.) the full peer evaluation reports of those two reviewers to whom the manuscript was assigned during the peer review process 2.) Dr. Teixeira da Silva's answers to the reviewers' comments and requests. The Appendix was published together with the article by KOME, to the explicit request of the author and with the knowledge and consent of the reviewers.

1-2 references to predatory journals. As it is not clear from the data whether some GSB journals contained a few papers with reference lists filled with predatory journals, or each and every paper published in GSB journals contained some references to articles published in POA journals, [AR: indeed, there was a peak in POA journal referencing in three journals, IJBPS, AAJPSB and MERJPSB, which could be linked, perhaps, to a cultural influence in the use of POA references to support factual or unfactual claims; this links to the above scientometrics analysis which may have revealed a country-POA use relationship] it is not possible to question the scientific quality of each GSB journal individually, but GSB as a publisher may be criticized for scientific rigor, which is not shedding good light on its journals either. As the author stated (l. 84-85) "*GSB journals have been serving as the cuckoo's nest for the surrogate validation of potentially non-academic, false or fraudulent scientific work*" –true, but this was a vicious circle, as the more articles with POA references published, the less scientific reputation GSB remained. Which was probably not stellar even at the start of the time period investigated, as GSB started as a new OA publisher in 2007-2008, according to its website. Maybe it would be more fair to say that GSB had the potential to become a reputable scientific publisher, but because of accepting articles which a.) aimed to validate potentially non-scientific or fraudulent works b.) accidentally validated such works due to author/reviewer/editorial failure in serving as scientific gatekeepers, this potential was never realized, or hindered to a significant extent. [AR: Once again, this is a strong possibility, given the restraints in human resources. Given my personal experience with dozens of plant science journals over two decades, at least, I can claim quite confidently that GSB represented a medium to high level of stringent review and quality control, but most certainly not perfect. Although I can appreciate the viewpoint by this reviewer, the tone is somewhat excessively harsh. Since the issue of POA was most likely not an issue for >95% of GSB authors, it was not this factor that led to the gradual crumbling of the publisher. It was suggested, in a survey which GSB conducted on plant scientists*, by a Serbian leader in the plant sciences, that the main reason why GSB would lose ground was based on three reasons:

- a) there was no open access model;
- b) there were no impact factors assigned to any of the journals;
- c) the journals were not indexed in any major data-bases.

* [http://www.globalsciencebooks.info/JournalsSup/images/Sample/AAJPSB_5\(1\)85-89o.pdf](http://www.globalsciencebooks.info/JournalsSup/images/Sample/AAJPSB_5(1)85-89o.pdf)]

- the increasing number of POA references and the significant decrease in published papers in 2013 (93 published papers in 2013jan-jul, cf. 4-500 papers per year in 2007-2012) is a causation, or simply a correlation? Are there any undisclosed/not presented factors here that can explain this decrease? Editors and reviewers started to reject papers with POA references, change in editorial guidelines or something else? It would be interesting to the readers to learn more about the background. [AR: This is a good point, and a detailed in-depth paper on the history of GSB and the lessons learned will be published in 2015, which will hopefully address the many queries that this reviewer is making. Even if they do show weakness in our editorial operations. As for the cause, or correlation, certainly two key events/reasons would have led to this slow-down of papers, and possibly the ironically inverse increase in the number of POA references appearing in our journals:

- a) A bitter battle (which is ongoing) with Elsevier about the ethics of collaboration and the definition of the terms of authorship:

<http://retractionwatch.com/2014/04/10/following-personal-attacks-and-threats-elsevier-plant-journal-makes-author-persona-non-grata/#more-19776>

b) An increasing personal bitterness towards science as the realization that GSB journals had been tainted by POA journals and that the peer review process had been imperfect (simply because no alternative existed within a limited human resource frame-work).

c) most likely these issues started to interfere with my ability to effectively steer the GSB ship, and indeed, failure or weakness in leadership ensued, which is the reason why I decided to terminate GSB, since no suitable strict (as I was) EIC could be found to replace me. During 2012-2013, precisely in the period that the reviewer is questioning, GSB had approached approximately 80 or more commercial publishers with the proposal of a take-over, but all of them, without exception, were only interested in the profit margins of GSB, and the IF scores of GSB journals, further accentuating my bitterness towards the traditional STM establishment. These personal stories will be openly disclosed in 2015.]

However, the aforementioned comments and critiques does not prevent me from advising the editors to accept and publish this manuscript. I'm certain that taking these comments in the vein they were intended would help the journal's audience to profit more from reading it; while no harm is done by leaving the manuscript unchanged. The analysis seems to be accurate (though, there is little that can be wrong with it) and the paper provides a particular insight about a so far neglected segment of predatory publishing. [AR: The very sad realization that what the review has stated, has several elements of truth to it, has in fact prevented me from dealing with this paper for resubmission to KOME. In part this is because this has been such a terrible personal and professional journey. I can fully understand that the reviewer would like to see more retrospective and even introspective analysis published alongside this simple data set, but the links and the possible correlations are extremely complex, so I do not want to muddy the paper with possibly tangential interpretations. I would hope that the reviewer and KOME can appreciate my position. I simply want a small data set to be out there, that would allow for deeper discussion at PubPeer, Retraction Watch, scholarlyoa.com and The Scholarly Kitchen. As I say, it is my intention to make all GSB content open access in 2015, and to also publish a memoir of the former publisher, which would then reference this small data set in KOME.]

Reviewer 2

First of all I would like to say that, in my opinion, the paper examines an extremely interesting and important topic of scientific communication. Moreover, it confers on original, innovative aspects, since, according to the author, this is the first study to show how predatory open access publishing can affect the reference list of other academic publishers. Since a reference list could be conceived as a 'communicative image' or as a compendium of the corresponding article, we could say that the way of infecting a so-called normal scientific article (and, by this, infecting a normal academic journal which contains the article in question) consists of placing a POA-article into the reference list of an article which is published in a 'truly' scholarly journal. Since I think that the article decently shows the workmanship of the author and I find the topic important and relevant as regards pure communication inquiry, I propose the article for publication in KOME. However, I have to mention two indefinabilities which, I think, could and should be discussed more precisely. First, I miss a correct distinction between articles and journals in the sense of distinguishing between sets and its elements. At least in classical logic, we could not squarely transfer the property of a given set (or class) to its elements (partition fallacies). So the properties of a given journal (a platform) could not be unproblematically corresponded to the properties of a given article in this journal. But in reference lists we normally found first of all articles and

not publishers. [AR: Indeed, this is what makes the topic so complex to analyze and quantify, because one cannot say that simply because one journal may be unscholarly that the publisher is also unscholarly. So, that is why I do not want to stray into the field of random hypotheses to try and explain the possibilities behind the data, because that could be unfair. The study simply wants to say the following:

- a) There was a publisher, called GSB, that published a set of journals that we considered scholarly simply because they were subjected to really strict peer and editorial scrutiny.
- b) Based on Beall's published lists (the only ones available, even today) of POA journals/publishers, we wanted to quantify how many of the references in the reference lists of our journals, contained papers published in POA journals or publishers.
- c) Given the fact that the Beall blog has serious flaws, and given the fact that many of the criteria on Beall's lists are flawed simply because there is no quantification of the parameters that make a journal or publisher „predatory”, I do not want to extrapolate too much beyond the sample data set, because that could be unfair on some papers, journals or eve publisher who may in fact, not be predatory. In fact, to try and quantify the level of predation, I devised, in 2012/2013, the Predatory Score: [http://www.globalscience-books.info/JournalsSup/images/2013/AAJPSB_7\(S11\)/AAJPSB_7\(S11\)21-34o.pdf](http://www.globalscience-books.info/JournalsSup/images/2013/AAJPSB_7(S11)/AAJPSB_7(S11)21-34o.pdf)]

Second, the definition of a predatory open access publisher: a predatory open access (POA) publisher refers to an open access (OA) publisher that engages in practices that are deceitful, fraudulent, non-academic or otherwise meant to draw unfair benefit from scientists or authors in a dishonest or unfair way seems a bit ordinary (in a sense of commonplace-like) to me. The author should explain in details what that the 'POA' label refers to. Is it a legal, a moral, a scientific, a political or an economic category? [AR: It is all, and it is none of these. One could for example, argue that Elsevier, PLOS or Taylor and Francis** are predatory for their pricing policies, if only that aspect alone is considered. But to try to simplistically explain what a POA it is impossible, at the moment. Please see my Predatory Score which would allow for the predatory nature to be quantified, and this is something that Jeffrey Beall should have adopted, but failed to, upon my suggestion. If we can apply the Predatory Score, which now needs to be upgraded to accommodate new factors that have emerged in 2013-2014, then we can clearly say if Publisher A or B, or journal X or Y, is “predatory”, taking into consideration legal, moral, scientific, political and economic factors.

** <http://retractionwatch.com/2014/11/20/journal-retracts-paper-when-authors-refuse-to-pay-page-charges/>]