Prof. Wolfgang Glänzel
Editor-in-Chief
Scientometrics

In Prague, July 12th, 2021

RESPONSE TO:
Email from 2 July, 2021 11:24 on "Article Retraction Request ("Predatory publishing in Scopus: evidence on cross-country differences" – Scientometrics 126:1897–1921)"

Dear Prof. Glänzel,

Thank you for the extended discussion of our paper.

We are confident that we can answer all of the new questions satisfactorily. We provide detailed answers on individual comments that have been raised in the post-publication peer reviews bellow (for clarity the original text is included and our answers are highlighted in yellow).

Let us kindly ask you to keep this procedure focused on issues that are decisive for the retraction request. All comments are welcome, but issues regarding the style of writing, additional references, potential avenues for research extensions, etc. are arguably not relevant at this point.

The post-publication peer-reviewers seem to be strongly critical about the use of Beall's lists and hence also inevitably about the evidence presented in our paper – one can even say that their comments are unusually aggressive. But this is not representative to the overall stance of the scientific community to this matter. It would be fair to consult reviewers who are ex-ante neutral, who do not have predetermined opinions in this regard, who in particular did not publish papers critical of Beall's lists by themselves in the past, and who could therefore provide assessment of the content and relevance of our paper that is representative and free of emotions.

Let us also point out that at least some of the comments seem to be put forward by a researcher from the field of natural sciences, who is used to deal with controlled experiments but who might not be familiar with how regression analysis is used in social sciences. We hope that this would not lead to a confusion.

We sincerely hope that this helps to clarify the issue and that our article retraction request will be dismissed soon.

Yours sincerely,

Martin Srholec and Vít Macháček

Detailed response to the post-publication peer reviewers' comments:

This is to inform you that, in the meantime, we have received two post-publication peer reviews following concerns being raised about your article (Scientometrics 126:1897–1921). In these comments, two further major points have been raised, which I am summarising below.

In line with COPE guidelines, we would like to give you the opportunity to respond to these points.

1.   This point refers to the regression analysis that has been conducted for the study. In this context, the question arose of why the authors have not used Cabell's blacklist, which was introduced in 2017 after Beall has withdrawn his list and which is still maintained and seems to be regularly updated. So, what would change in comparison with the authors' results? As to the regression analysis itself, the following arguments have been adduced.

ANSWER: We did not use Cabell's blacklist for the following reasons:

i) As clearly explained in our first response letter, Beall's lists are the dominant source of data in this line of research that provide sufficient inputs for conducting our study. In contrast, Cabell's blacklist is relatively new data source that have been seldom used in research papers on this topic so far. Adding results based on Cabell's blacklist is a possibly fruitful idea for a research extension, not a discussion point for retracting our paper. In fact, we are not aware of any study that attempted to measure the propensity to publish in predatory journals at the national level, which is the main idea of our paper, that has been based on Cabell's blacklist (probably due to issues with its accessibility, as further noted below).

In the meantime, Beall's lists continue to be used in published papers, including a very interesting recent article that came out in June 2021 issue of Scientometrics (which relies on data from Beall's lists in the same way as our paper):

Marina, T. & Sterligov, I. (2021) Prevalence of potentially predatory publishing in Scopus on the country level. Scientometrics, 126(6), 5019–5077. DOI: 10.1007/s11192-021-03899-x

ii) As indicated in our paper, Beall's lists were downloaded on April 1st, 2016 and the main database underlying our paper was very carefully and labouredly constructed in months that followed. When we compiled the database of journals and publishers on the base of Beall's lists for the purpose of our paper, Beall's lists were online and Cabell's blacklist did not even exist.

iii) Beall's lists are available for free. Cabells' Predatory Reports is a paid subscription service. It is not clear from the Cabell's webpage (http://www2.cabells.com/about-predatory) what is the subscription price and most importantly whether the whole database (or only excerpts/reports based on it) is available for download. The fact that to the best of our knowledge no paper has so far compared the full content of Beall's lists and Cabell's blacklist indicates that access to the whole Cabell's database might be expensive and/or not possible. In any case, we have published our database of journals based on Beall's lists in a

transparent way as supplementary information to our paper, so anybody who wished to perform such a comparison can use it to that purpose (and indeed this would be interesting to see).

iv) Cabell's blacklist is not without its own problems and has not been established as a reliable source of data for this line of research: "Cabell's blacklist has been criticized for including numerous empty journals, which "raises serious questions about the ways in which they prioritise journals for inclusion and their willingness to provide an up-to-date and useful blacklist to the scholarly community". Other concerns about the blacklist include "questionable weighing and reviewing methods" and "a lack of rigour in how Cabell applies its own procedures" as "identical criteria are recorded multiple times in individual journal entries" and "discrepancies exist between reviewing dates and the criteria version used and recorded by Cabell" (Dony, et al. 2020, as cited here https://en.wikipedia.org/wiki/Cabells%27_Predatory_Reports).

Dony, C., Raskinet, M., Renaville, F., Simon, S. & Thirion, P. (2020. How reliable and useful is Cabell's Blacklist? A data-driven analysis. LIBER Quarterly. 30 (1): 1–38. doi:10.18352/lq.10339

(See also the discussion in Teixeira da Silva & Tsigaris 2018, pp. 785-786, as referenced below)

Hence, the jury is still out on usefulness of Cabell's blacklist in this line of research. It is not clear in this respect what would be actually the main takeaway from a comparison of Beall's lists and Cabell's blacklist. If they differ, which one is the right one? What if Beall's lists are in fact more comprehensible?

Anyway, doing such comparison was well beyond the scale and scope of our paper – this is a topic of a potential separate follow-up paper. Not doing this can hardly be used as a reason to retract our paper.

v) As noted, Cabell's blacklist is updated. Hence, the current content Cabell's blacklist differs from its content several years back. For a robust comparison, one would need to obtain the historical version of the Cabell's blacklist from the same period as Beall's lists. For our paper specifically, this is not possible as Beall's lists used in our paper are from April 2016, when the Cabell's blacklist did not exist.

If someone plans to compare Beall's lists and Cabell's blacklist in a separate paper, she/he probably would need to use the last version of Beall's lists from early 2017 and the first version of Cabell's blacklist from early 2017 for that purpose. It is not clear whether Cabells will be willing to share the historical initial version of its database for the purpose of this comparison.

vi) The fact that Cabell's blacklist is updated provides little advantage for our paper, which looks back at the period 2015–2017. In fact, this could be a major disadvantage, as we would need to obtain the historical initial version of Cabell's blacklist from early 2017 for this our purpose (see also the previous point).

vii) "…Beall, who was once an informal consultant for Cabell's…" (https://www.nature.com/articles/nature.2017.22090). It well might be that the Cabell's blacklist was actually compiled with the help of Jeffrey Beall himself, at least with regards to

The authors analysed the impact of GDP per capita, size of research sector, natural resource rents, language spoken, and geographical location broken down by total, standalone, publishers excluding Frontiers, Frontiers and total excluding Frontiers. In some of the groups (total, standalone, and total excluding Frontiers) an inverse U-shaped relationship was found, which may lead to the conclusion that less developed and wealthy nations have lower propensity to publish in predatory journals with respect to countries at a medium level of economic development. Similar relationships have been found regarding size of research sector. Although the authors admonish that "the cross-sectional nature of the data does not allow for testing of causality, the estimated relationships indicate correlations, and the results should therefore be interpreted with caution", for instance, the abstract does not call for caution regarding the interpretation of the results, as the authors write:

"In the most affected countries, including Kazakhstan and Indonesia, around 17% of articles fall into the predatory category, while some other countries have no predatory articles whatsoever. … Policymakers and stakeholders in these and other developing countries need to pay more attention to the quality of research evaluation."

to the paper was required by one the peer reviewers of the paper (the regression analysis was not in the initially submitted version of the paper), which turned out to be a great suggestion for improving our paper (and one of the main beneficial outcomes of the detailed peer-review process in Scientometrics, for which we are grateful).

Furthermore, the question arises of why only one publisher (Frontiers) is singled out as being a flaw in the blacklist, while in other studies, for instance, in Teixeira da Silva & Tsigaris (2018) was pointed to several false positives in Beall's list. Why have no other flaws in the blacklist been excluded from the analysis?

ANSWER: Why only Frontiers is single out is a relevant question, which, however, is already answered in the article itself: "The greatest controversy was triggered by inclusion of the Frontiers Research Foundation on Beall's list of publishers in October 2015. Beall defended this decision by pointing out several articles that, according to him, should not have been published. According to critics of this move, the Frontiers publisher is "legitimate and reputable and does offer proper peer-review" (Bloudoff-Indelicato 2015). Frontiers journals appear to be quite different from typical predatory outlets on the face value of their citation rates. Only 4 journals in Frontiers' portfolio of 29 included in this study are not ranked in the first quartile in at least one field according to the Scimago SJR citation index (Scopus 2018b). Most Frontiers journals are also indexed in the Web of Science and the Directory of Open Access Journals. Hence, judging by the relevance of Frontiers journals for the scientific community, there is a question mark about their inclusion on the predatory list." (pg. 1902).

In other words, there was a strong controversy about this in respectable media and a look at citation indexes of the journals in our database in Scopus revealed that indeed Frontiers journal truly stand out in this respect; hence singling out these journals was strongly supported by hard data, which in our view provided credibility to the doubts about their inclusion in the blacklist. This is why we zoomed at Frontiers in the paper and investigated the results by total, standalone, publishers excluding Frontiers, Frontiers and total excluding Frontiers. The readers are provided these results separately in a transparent way in order to be able to make their own mind about this issue.

Importantly, however, we do not know whether Beall was right or wrong by including Frontiers on his blacklist, i.e. whether this was in fact a flaw of his list or not – maybe it was and maybe it wasn't. All that we can conclude given the hard data in hand is that "Although there is no evidence in the data presented upon which we can judge whether the inclusion of Frontiers on Beall's list was justified or not, the results at the very least clearly indicate that Frontiers is atypical." (pg. 1914).

Teixeira da Silva & Tsigaris (2018) provides an interesting general discussion about blacklists but we did not find in this paper detailed arguments about specific cases of false positives in Beall's lists that would be supported by new primary evidence and backed by hard data. The only case that they mention in more detail than just in passing is Oncotarget (pg. 790), the discussions about which we were aware of, but we did find doubts about its inclusion in Beall's list compelling enough to present our results separately for Oncotarget in a similar fashion as we did for Frontiers (because strange editorial practices surrounding this journal have been exposed and because this journal has been delisted from the Web of Science, etc.).

We found the argument for singling out Frontiers to be far stronger and most importantly supported by hard data (on journal citation impacts in respected citation databases), as compared to discussions on other possible false positives. This is why we tested for robustness of the results with regards to it.

Four language categories (English, Spanish, French, and Arabic) were used as dummy variables. This selection alone raises questions. Furthermore, the only significantly positive correlation has been found with Arabic spoken nations. Does this imply higher propensity for "predatory" publishing? What about countries (like China, India, and others), where different languages are spoken? The results of the regressions analysis are inconclusive for the other three groups and for those with languages not included in the analysis.

ANSWER: English, Spanish, French, and Arabic language categories were chosen based on the data. Again, this is already explained in the paper: "Next, we examine differences by major language zones using indicators obtained from the GeoDist database which measure whether the language (mother tongue, lingua francas or a second language) is spoken by at least 20% of the population of the country (Mayer and Zignago 2011). Only English, French, Spanish and Arabic are recognized separately, as other languages are not spoken in a sufficient number of countries." (pg. 1907). In other words, only these four languages are spoken by a significant portion of its population in more than a few or single countries. Adding dummy variables for other languages would essentially mean adding intercept dummies for a small group countries of individual countries, which is not advisable in the context of this regression model (the purpose of this variable in the regression is to control for the potential language bias, not discriminate between particular countries).

China is one of only a few Chinese spoken countries (Chinese Hakka, Mandarin, Min Nan or Yue). India belongs to the large group of English speaking countries (Hindi is not sufficiently prevalent in any other country). For more detailed definition of the languages see Mayer and Zignago 2011.

Yes, the fact that a significantly positive correlation has been found between the propensity for predatory publishing and the dummy variable for Arabic spoken nations indicates that there is a relationship.

Yes, the results of the regressions analysis are inconclusive for the other three language groups – this is why we are running the analysis, i.e. to find out whether there is a relationship, if other relevant factors are controlled for.

However, it is incorrect to state that the results of the regressions analysis are inconclusive for those with languages not included in the analysis. Since dummy variables for the language groups are included, the group of countries with other language spoken represent a base category in this estimate, i.e. the English, Spanish, French, and Arabic language spoken countries are compared to this base category in the estimate. Hence, the results indicate that Arabic spoken countries significantly differ – ceteris paribus – with regards to the outcome from the base group.

As to the results of the regression analysis, there is no control group to analyse if similar patterns can be observed in the control group. The regression analysis is, therefore, not complete and the results, by default of a positive control group, cannot be meaningful.

ANSWER: The regression analysis is complete and meaningful – this is a standard way of running a regression analysis in the context of social sciences (in which controlled experiments are relatively rarely possible to conduct, especially at the country-level). However, there is extensive research literature (in economics and other disciplines) that rely on running cross-country regressions. Everything that is needed to understand how the regression analysis has been done is explained in our paper.

Why and where there should be a control group here? What would a control group be in the context of this analysis? Arguably, there are no (or a sufficient number of) countries that are not exposed to the problem of predatory publishing, which could potentially constitute a control group (and which would open a way for using matching estimator or a treatment effects model, the use of which involves a control group). There is no viable control group here.

Results and findings are merely based on a so-called blacklist and not supplemented by any results obtained, for instance, from a whitelist.

ANSWER: The point of the paper is to analyse the blacklist. More specifically, we look at the overlap between a blacklist (Beall's database) and a whitelist (Scopus). The outcome variable of our interest is the share of articles published in journals on Beall's blacklists out of all articles published in journals indexed in the Scopus whitelist. The share of whitelist articles (not blacklisted) constitute the rest up to 100%, if percentages are used. As the result, the propensity to publish the whitelist articles (not blacklisted) is inverse to the propensity to publish Beall's blacklisted articles.

2.   The article contains several unsupported statements and conclusions, notably the conclusions drawn regarding the "propensity of scholars" of several countries to publish in "predatory" journals are strongly discriminatory. Such statements can be found at several places, some examples are given from the review:

ANSWER: All of these statements are supported by the analysis and based on the presented results – they are not "discriminatory" in any way, they simply state the facts about the cross-country differences in propensities of scholars to publish in predatory journals that have been identified in the paper based on the data in hand.

"The resulting database provides more representative and comprehensive country-level evidence on the problem of predatory publishing than has been available in any previous studies." (Introduction)

ANSWER: Again, this is taken out of the context. The preceding paragraph states as follows: "Existing literature provides very scant evidence along these lines and the studies at hand are limited to individual countries and use different methodologies, so the results are not easily comparable. For example, Perlin (2018) found that suspected predatory journal articles

accounted only for about 1.5% of publications in Brazil, while Bagues et al. (2019) showed that around 5% of researchers published in such journals in Italy. No study has yet examined the penetration of national research systems by predatory publishing in a broad comparative perspective. Systematic scrutiny of cross-country differences worldwide is lacking." (pg. 1898). Our paper indeed provides by far the most representative and comprehensive country-level evidence on the propensities of predatory publishing that has been available until that point.

No evidence is given to sufficiently support this statement.

ANSWER: The whole analysis presented in our paper is the evidence to support this statement. Or is covering 172 countries, i.e. essentially all countries in the world for which bibliometric data is available, not sufficiently representative and comprehensive country-level evidence?

"India appears to be the main hotbed of predatory publishing, but in the context of India's gigantic research system, this may be much ado about little." (Introduction) This is a discriminatory comment and conclusion, which is not supported by any reference or citations.

ANSWER: Again, this is taken out of context. This sentence is not meant to be read in isolation. The text that precedes this statement and that is necessary to read to understand what this refers to and what is meant here is as follows: "So far, only a handful of studies have examined the geographical distribution of authors published in journals suspected of predatory practices by Beall (2016). On a sample of 47 such journals, Shen and Bjork (2015) found that the authors were highly skewed to Asia and Africa, primarily India and Nigeria. Xia et al. (2015) examined 7 pharmaceutical journals and also identified the vast majority of authors as being from Southeast Asia, predominantly India, and, to a lesser extent, Africa. Demir (2018) combed through 832 predatory journals and confirmed that by far the greatest number of authors are from India, followed by Nigeria, Turkey, the United States, China and Saudi Arabia. Wallace et al. (2018) focused on 27 such journals in economics, in which the authors were most frequently from Iran, the United States, Nigeria, Malaysia and Turkey.

No matter how insightful these studies are in revealing from where contributors to predatory journals originate, we still know very little about the magnitude of the problem for the respective countries and regions. India appears to be the main hotbed of predatory publishing, but in the context of India's gigantic research system, this may be much ado about little. All of the countries cited above are, unsurprisingly, quite large. Could it be that some smaller countries are actually far worse off, though they do not stand out in the absolute figures? Just how large is the propensity to predatory publishing at the national level? Which countries are most and least affected by predatory publishing, and why?" (pg. 1898).

Hence, the whole preceding paragraph cites research that shows that predatory publishers are concentrated in India –the existing studies that looked at the location of predatory publishers pointed to this fact (Shen and Bjork 2015, Xia et al. 2015, Demir 2018). Given the large size of India, which then dominates the absolute figures, it cannot be ruled out that other (smaller) research systems might be affected by this problem equally or even more than India in terms relative to their size, i.e. looking at the relative proportion of publications in predatory journals. This is what this cited sentence is attempting to argue. It might have been a

cumbersome way to put it, but surely this is not a credible reason to retract a published paper, isn't it?

"South Korea is by far the worst among advanced countries." (Cross-country patterns)

Such conclusion has been found unscholarly and discriminatory, forming false accusations.

ANSWER:

Again, this is taken out of context. This is not a standalone sentence. In the context of the whole paragraph, it should be clear to anybody what this statement refers to: "Table 2 shows figures for the top and bottom 20 countries. Kazakhstan and Indonesia appear to be the most dire, with roughly every sixth article falling into the predatory category. They are followed by Iraq, Albania and Malaysia, with more than every tenth article appearing in predatory journals. Some of the most severely affected countries are also among the largest in terms of population: India, Indonesia, Nigeria, the Philippines and Egypt, which underlines gravity of the problem. However, small countries that might have been difficult to spot on a world map, such as Albania, Oman, Jordan, Palestine and Tajikistan are also seriously affected. South Korea is by far the worst among advanced countries. All countries on the top 20 list, excepting only Albania, are indeed in or very near Asia and North Africa." (pg. 1906).

Results presented in Table 2 (and a detailed look at the overall results) clearly show that South Korea is by far the most affected advance country by predatory publishing. There is nothing unscholarly or discriminatory about it; this is simply a finding of our analysis. The sentence could have read "South Korea is by far the worst *affected by predatory publishing* among advanced countries." But adding these three words in this sentence has been deemed as too repetitive and unnecessary by us in the context of this paragraph and the whole surrounding text. It should be clear that we talk about the propensity to predatory publishing here. Not reminding the reader about this in every sentence over again makes the paper more readable in our view. What else than predatory publishing could we possibly mean in this sentence?

Reference

Teixeira da Silva, J. A., & Tsigaris, P. (2018). What value do journal whitelists and blacklists have in academia? The Journal of Academic Librarianship, 44(6), 781–792. https://doi.org/10.1016/j.acalib.2018.09.017